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THE
CHEMICAL NEWS:

AND
JOURNAL OF PHYSICAL SCIENCE:

(WITH WHICH IS INCORPORATED THE "CHEMICAL GAZETTE")

A Journal of Practical Chemistry

IN ALL ITS APPLICATIONS TO

PHARMACY, ARTS, AND MANUFACTURES.

EDITED BY
WILLIAM CROOKES, F.C.S.

VOLUME III.—1861.

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THE CHEMICAL NEWS

VOL. I. No. 57.—January 5, 1861.

THE CHEMICAL NEWS AND JOURNAL OF PHYSICAL SCIENCE.

THE present Number commences what may almost be termed a new era in the existence of the CHEMICAL NEWS, and we owe it to our readers to explain the reasons why we have considered it necessary to increase its price, and to make a more than corresponding increase in its dimensions.

Most, if not all, of our learned and scientific Societies have a special organ of their own, published sometimes monthly, but generally less frequently. These publications, as a rule, give only the proceedings of the Society to which they respectively belong, and the papers that are read at its meetings. The papers are given in full, as they ought to be under such circumstances; and it is with no desire to detract from their value that we say that, for the general reader, they, as a consequence of this, remain unread. The working-men,—that is to say, the chemist, whose days are spent in the laboratory; the engineer, whose hours are employed in carrying out plans already complete, and in elaborating others; the electrician, who labours to simplify the transmission of intelligence, or to find a method of perfecting the means already in existence; and the host of scientific men, and those who take an interest in scientific subjects, who have daily avocations which exhaust their energies,—have not the courage to sit down and tax their intellect still further, by the study of an article which commonly requires a closer examination in proportion as its merits are greater. They would, however, be glad enough to read an abstract of such a paper, which embodied its chief points, the result of which would be that they themselves would receive new information and ideas, and the writers their reward, in the shape of a far more extended fame and reputation, which is what they principally seek.

Then, again, these Journals are usually confined to some particular branch of science; and to obtain a complete knowledge of what is going on in the scientific world, it is necessary to see them all. Now, the outlay of a few pounds annually for such a purpose is a matter hardly worth consideration to the few, but to the many it is a serious obstacle. We do not, however, lay so much stress on this point as on the time which would be required to read them.

Having shown how the official organs fail to supply a want,—and we must again repeat that we have not done so in the way of reproach, because any alteration in them would only lessen their value,—we need say nothing more to prove the necessity which existed for the establishment of THE CHEMICAL NEWS AND JOURNAL OF PHYSICAL SCIENCE, which is henceforward to be the title under which we shall appear. As hitherto, Chemistry will be the leading feature; but the greater amount of space at our disposal will enable us to introduce important matter relating to other departments of Physics. Since a very early period in the existence of

the CHEMICAL NEWS, it has been a source of constant anxiety to us to find space for valuable articles which we felt our readers ought to have an opportunity of perusing, but which the steady influx of matter of more general interest compelled us to defer from time to time, until the period had gone by when we could publish them. In many instances, these have been communications from men who have made a reputation, and one principal inducement to enlarge our space has been the facility it would give us for publishing more of such communications in future; for there can be no more doubt of our willingness to do so than of the advantage, so far as reputation is concerned, that would accrue to the writers from the publication of their papers in a journal of the circulation to which this has attained. While Chemistry, in all its various branches, Scientific and Analytical, Technical, or in its relation to Agriculture, will still form the principal subject treated of, the Medical Profession will not fail to find recorded in its pages every new discovery relating to Toxicology, Materia Medica, and Pharmacy. We have arranged for the transmission to us of every scientific journal published on the Continent, and from these we shall translate, in full or in abstract, everything of importance. Of course, while paying such close attention to what is passing abroad, we shall not omit to give due prominence to the proceedings of Societies at home, more especially to those of the Royal Institution and the Chemical and Pharmaceutical Societies.

All books forwarded to us will receive impartial criticism, with as little delay as is consistent with a proper examination of their contents; and we propose each week to publish a notice of recent Chemical Patents, describing, not merely the purpose which the invention is intended to effect, but the probability of its effecting what it professes, and the degree in which it resembles any patent taken out previously, having reference to a similar object,—the abuse of the patent-law being one of the crying evils of the day.

As heretofore, our columns will be open for the record of any new facts which may be observed in the conduct of operations in the laboratory; and we invite those who may observe such facts to forward them to us, in their own interest as well as in that of others engaged in the same professional pursuit.

In conclusion, we will state, as briefly as may be, what we aim at effecting by the alteration, and then leave it to our readers to decide in the future how far we accomplish what we propose.

Up to the present, there has not existed any Journal in Great Britain which professed to give a notice of all events of interest which occur in the scientific world. One Journal occasionally devotes a portion of one of its columns to the record of the most striking facts: but such desultory information does not meet the eye of the persons who are chiefly interested in such matters. We have, therefore, determined on making THE CHEMICAL NEWS AND JOURNAL OF PHYSICAL SCIENCE the per-

manent record of all matters interesting to the chemical philosopher, the man of science, and to educated men generally. We do not aim at superseding the recognised organs of the various learned Societies. The papers they contain, valuable as they are as records, are too full of detail, and often too abstruse to be published elsewhere at length; but such portions of them as are of principal importance we shall, as heretofore, embody in a summary,—an arrangement which we have found to be as gratifying to the authors as to our readers. In this way they will be kept thoroughly informed of all that transpires in any part of the world, whether relating to the discovery of a new chemical compound, or a novel application of something already known. That continually-increasing class of men, who, without being engaged in such subjects professionally, desire to be kept informed on their progress, will find in its pages all that he desires to know; and, that every facility may be given for reference to the past, a most copious Index will be published on the completion of each Volume.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Opacity of the Yellow Soda Flame to Light of its own Colour, by WILLIAM CROOKES.

In some remarkable investigations on the colours which certain substances impart to the flames in which they are heated,¹ Professors Kirchhoff and Bunsen describe certain experiments by which they prove that the luminous lines which are produced in the spectrum of a spirit- or gas-flame, when salts of the alkalies or alkaline earths are caused to volatilise therein, become reversed (i.e. that the bright lines become changed to dark ones) when a source of light of sufficient intensity, and giving a continuous spectrum, is placed behind the coloured flame. Amongst other experiments which were given to illustrate this point, was one in which, a solar spectrum showing the dark line D having been first obtained, a gas-flame, coloured yellow by the introduction of a bead of chloride of sodium, was interposed, when the dark line D (coincident with the yellow sodium line) appeared with increased blackness, instead of being partially, if not entirely, filled up by the luminous sodium line. In another experiment they showed that the dark double line D also appeared when a yellow soda flame was introduced in the apparatus in front of a white hot platinum wire; the previously continuous spectrum of the incandescent wire being at once cut across by a black line, occupying the position of D, when the soda flame was interposed (which *per se* would have given a luminous line, occupying the position of the now dark lines). The most conclusive experiment, however, was the following:—They held between the incandescent platinum wire and the optical part of the apparatus, a test-tube containing some sodium amalgam in a state of ebullition; where the luminous rays from the wire traversed that part of the tube containing sodium vapour, the dark line D was at once produced, disappearing as soon as the metallic vapour was removed from the path of the light. The authors conclude from this important experiment that sodium vapour, at a temperature much below that at which it becomes luminous, exerts its absorptive power at exactly the same point of the spectrum as it

does at the highest temperatures which we can produce. Professor Kirchhoff,² describing some experiments similar to the above, writes:—"I conclude from these observations, that coloured flames, in the spectra of which bright sharp lines present themselves, so weaken rays of the colour of these lines when such rays pass through the flames, that, in place of the bright lines, dark ones appear as soon as there is brought behind the flame a source of light of sufficient intensity, in the spectrum of which these lines are otherwise wanting. I conclude further, that the dark lines of the solar spectrum which are not evoked by the atmosphere of the earth, exist in consequence of the presence, in the incandescent atmosphere of the sun, of those substances which in the spectrum of a flame produce bright lines at the same place. . . In the course of the experiments which have at present been instituted by us in this direction, a fact has already shown itself which seems to us to be of great importance. The Drummond light requires, in order that the lines D should come out in it dark, a salt flame of lower temperature. The flame of alcohol, containing water, is fitted for this, but the flame of Bunsen's gas-lamp is not. With the latter the smallest mixture of common salt, as soon as it makes itself generally perceptible, causes the bright lines of sodium to show themselves."

During some researches on the spectra of artificial flames which I have been carrying on simultaneously with MM. Kirchhoff and Bunsen, one or two forms of experiment suggested themselves which, while they perfectly corroborate most of the facts mentioned above, seem to merit attention, from the facility with which they may be performed, and the striking manner in which the phenomena can be exhibited to several persons at once without the necessity of employing any optical apparatus. The atmosphere of the room—or, rather, that part of it in which the illustration is performed,—is to be first impregnated with soda-smoke, by igniting a piece of sodium, the size of a pea, on wet blotting-paper. Any flame, whether of gas, spirit, a candle, &c., which may now be burning anywhere in that part of the room, will exhibit in a marked manner the well-known yellow soda-flame; and if the full amount of gas in an ordinary wire-gauze air-burner is turned on and ignited, it will give a uniformly brilliant yellow flame, upwards of a foot high and three inches in diameter.

If a smaller flame be now moved in front of this large one it will exhibit a curious phenomenon. Those parts of it which are ordinarily seen to be luminous will suffer no change, other than that slight diminution in intensity which might be anticipated from their projection in front of a broad but not very brilliant source of light; but beyond these there will appear a sharply-cut and intensely black narrow border, closely surrounding the visible flame, and presenting the curious appearance of the latter being set in an opaque frame. A closer scrutiny will show that the position of this black rim is not, as I at first supposed, in that outer cone in which the yellow soda-flame is most distinctly seen, but that it lies in the dark space immediately outside the luminous part of the flame, affording proof of the existence of another invisible cone of vapour. The flame from a tallow candle shows this appearance better than that of wax or sperm, probably on account of its inferior luminosity. A small spirit- or gas-flame will also answer very well; but I think a tallow candle shows the phenomenon in a more striking manner.

¹ "Chemical Analysis by Spectrum Observations," by Professors Kirchhoff and Bunsen. *Phil. Mag.*, s. 4, vol. xx. p. 89. August, 1860.

² "On the Simultaneous Emission and Absorption of Rays of the same Definite Refrangibility." *Phil. Mag.*, s. 4, vol. xix. p. 193. March, 1860.

The fact of the cone of yellow soda-flame being transparent, while the outer, non-luminous, space is perfectly opaque to the same kind of light placed behind it, appears worthy of attention. It seems to show that the yellow flame caused by the presence of incandescent solid particles of a sodium compound has no very marked absorptive action on light of its own colour; but that to give rise to this kind of opacity it is necessary that the sodium compound should be in the state of vapour. It appears, moreover, to prove that it is not necessary for this vapour to be in the metallic state; for it could hardly be supposed that so highly combustible a vapour as that of metallic sodium could be present in that part of the flame which is seen to possess this great opacity. That soda salts are easily volatile at the temperature of flame, is a fact abundantly proved by Bunsen.¹ The reason why the opacity is only exhibited by that part of the outer shell of vapour which is situated at the edge of the flame, and not by its entire extent, is owing to its thickness being insufficient to produce sensible absorption on rays which traverse it perpendicularly; an appreciable action taking place only when they pass as a tangent to the edge of the flame, and thus traverse a considerable extent of absorbing medium.

Cæsium.

At the last meeting of the Chemical Society, Dr. Roscoe gave a short account of Professors Kirchhoff and Bunsen's spectrum researches, and mentioned that the new alkali-metal² which they had discovered by that means had been named *Cæsium*, from the Latin word *cæsius*, signifying grayish-blue, that being the tint of the two spectral lines which it shows. By working with the residues from twenty tons of the mineral waters of Kreuznach, Professor Bunsen had succeeded in obtaining about 250 grains of the platinum salt of the new metal. Cæsium is closely allied to potassium in its chemical characters, the chief point of difference being the solubility of its nitrate in alcohol. Its equivalent number is 117,—exactly three times that of potassium.

On the Determination of Phosphoric Acid in Complex Natural Substances and particularly in those containing Iron, by M. G. CHANCEL.

WHEN I published the process for the separation and estimation of phosphoric acid under the form of phosphate of bismuth,³ I took care to insist on the necessity of previously eliminating the chlorine and sulphuric acid. It remains for me to warn chemists who wish to use this process against a source of error which iron will occasion when that metal is in the state of sesquioxide. In this case the phosphate of bismuth is precipitated with difficulty, and always carries down iron with it, which gives a red colour to the precipitate after calcination. But it is only necessary to reduce the iron to the minimum of oxidation to restore to the process all its generality, and to allow of its successful application to the analyses of ashes, arable land, coprolites, mineral waters, iron ores, &c. A very simple plan, which has always succeeded with me, consists in effecting this reduction with sulphuretted hydrogen.

The better to answer the various inquiries made of me, I think it necessary to give a summary account of the analytical process which ought to be followed in determining phosphoric acid in the most complicated cases.

1. Heat the weighed substance with excess of warm concentrated nitric acid, to transform, if expedient, the meta- or pyro-phosphoric acids into tribasic phosphoric acid; dissolve in a proper quantity of nitric acid, add water, and filter if necessary.

2. Eliminate from the diluted solution first the sulphuric acid by nitrate of baryta; then the chlorine, by nitrate of silver; separate each of these precipitates by filtration.

3. Bring the iron to the minimum of oxidation by passing to saturation a current of sulphuretted hydrogen through the filtered liquid. By this means the silver added in excess is precipitated, as well as the other metals whose sulphides are insoluble in weak acids. Numerous experiments have proved that the reduction of iron under these circumstances is always complete, even when the liquid contains a considerable quantity of free nitric acid. The operation is finished when the precipitate has collected together and the liquid is perfectly limpid. Before filtration all the sulphuretted hydrogen must be expelled. This result is speedily obtained by passing through it a current of carbonic acid, until the gas which is disengaged ceases to discolour paper saturated with acetate of lead.

4. Under these circumstances, the estimation of the phosphoric acid presents no difficulty. It is only necessary to add excess of acid nitrate of bismuth to the filtered liquid; allow the precipitate to deposit, then collect it on a filter, and, after well washing it with boiling water, dry, calcine, and weigh it. The nitrate of the protoxide of iron being remarkably stable, there is no cause to fear the peroxidation of the iron before the addition of acid nitrate of bismuth, provided the operation is conducted at the ordinary temperature. Moreover, I am convinced that the liquid may be boiled, without danger, after the phosphate of bismuth is collected; the sesquioxide of iron which then forms being no impediment to the determination of the phosphoric acid.

5. Treat the filtered liquid again with sulphuretted hydrogen, which eliminates from it the excess of bismuth, then determine the bases by the ordinary processes.

It should be remarked that the various foregoing operations are all exceedingly simple and speedily performed, and that this method serves not only for the quantitative determination of phosphoric acid, but also to detect its existence in substances which contain only traces of it, as mineral waters, certain iron ores, &c. In these qualitative trials, when the acid nitrate of bismuth yields a precipitate, it is easy to identify the phosphoric acid in it. It is sufficient for this purpose to treat the precipitate, suspended in a little water, with sulphuretted hydrogen, then to add excess of nitrate of silver, and shake it. The liquid cleared by the filter of the sulphides of bismuth and silver, is then free from sulphuretted hydrogen; if it contains phosphoric acid it will give the yellow precipitate so characteristic of phosphate of silver when exactly neutralised with very diluted ammonia.

In conclusion, I will point out a good method for the preparation of acid nitrate of bismuth; it consists of substituting for the subnitrate, crystallised neutral nitrate, $\text{BiO}_3\text{NO}_3 + 10\text{Aq}$, which it is easy to obtain perfectly pure. A convenient re-agent is obtained by dissolving

¹ *Phil. Mag.*, s. 4, vol. xviii. p. 513.

² *CHEMICAL NEWS*, vol. ii. p. 281.

³ *Comptes Rendus*, t. l. p. 416; and *CHEMICAL NEWS*, vol. i. p. 230.

68.45 grammes of neutral crystallised nitrate in a quantity of nitric acid representing 68.5 grammes of anhydrous nitric acid, and in then adding water, so that the solution occupies exactly the volume of a litre. Each cubic centimetre of the re-agent thus prepared will precipitate 1 centigramme of phosphoric acid.—*Comptes-Rendus*, t. li. p. 382.

On the Insoluble Matter of Zinc, by

GEORGE F. RODWELL.

IN making hydrogen by the action of dilute sulphuric acid on commercial zinc, we observe a number of black flocculent particles floating on the surface of the liquid, which, when the zinc is all dissolved, gradually sink to the bottom, and crumble down to a grayish powder. This residue, in 100 parts of ordinary sheet zinc, amounted to

I.	II.	III.	IV.
1.3142	1.3661	1.3388	1.3017

or, taking the mean of the four determinations, 1.3339. It was found to consist of sulphate of lead, together with about .5 per cent. of carbon, and a trace of iron. The black particles appear to be suboxide of lead, which, when the evolution of hydrogen has ceased (and not till then) are slowly converted into sulphate of lead.

The lead undoubtedly exists in the zinc as metallic lead, and its quick conversion into suboxide is probably due to the electric current which is established between it and the zinc with which it is in contact; for if a clean piece of lead be immersed in dilute sulphuric acid it will remain bright for some time; but if, now, a piece of zinc be placed in the liquid, so as to touch it, the lead will be speedily coated with a black film.

TECHNICAL CHEMISTRY.

On a Simple Means for Concentrating Sulphuric Acid without employing Platinum Retorts, by W. T. CLOUGH.¹

IT is a well-known fact that lead is with difficulty attacked by cold concentrated sulphuric acid, whilst the same acid when hot, very rapidly transforms it into sulphate of lead.

M. Clough has endeavoured to take advantage of this circumstance to concentrate the crude sulphuric acid from the chambers in a leaden vessel kept cold. The arrangement of his apparatus is as follows:—

Place a large leaden holder in an iron one of the same shape but larger, and fill the interspace with water, which should always be kept cold. Line the interior of the leaden holder completely with some kind of brick or stone not affected by sulphuric acid, and surmount it with two brick arches, that the holder may be, as it were, the sole of a reverberatory furnace. Having filled the holder with sulphuric acid, of a density of 4° Baumé, pass over the liquid the flame of a fire placed a little distance from one of the extremities of the holder. Rapid evaporation is thus obtained without the sides or bottom of the lead holder becoming heated. In fact the warm acid becomes almost completely cold by passing across the bricks (non-conductors of heat), before reaching the lead which is itself maintained cold by the water

in the iron holder. Between the two brick arches, so as to completely close and complete the arch, insert a boiler made of lead, in which the weak acid from the lead chambers is heated and partly concentrated before being made to flow into the lower holder.

[Is not a considerable loss of sulphuric acid to be feared, which when near its maximum of concentration and heated to 300°, forms vapour abundantly? These vapours are easily affected by the flame, and the current of warm air passing under the acid. When the sulphuric acid is concentrated in platinum retorts, the acid vapours given off in considerable quantities are collected by condensation, especially when near the boiling point and the maximum of concentration of the sulphuric acid.

M. Kuhlmann has proposed another method for concentrating sulphuric acid in lead. This method is founded on evaporation *in vacuo*.—*E. Kopp*.]

PHARMACY, TOXICOLOGY, &c.

On the Uncertainty of Composition of the Aqua Lauro Cerasi,² by HARRY NAPIER DRAPER, F.C.S.L.

MY object in this paper is to draw attention to the very uncertain constitution of cherry laurel water, and to the importance of providing some substitute in the shape of the much more stable hydrocyanic acid. Laurel water has on its side all the claim of antiquity, and I am aware that it is not easy to persuade the practitioner who has long been in the habit of prescribing any one remedy to use another in its stead. I am also fully alive to the importance of caution in presuming upon the value of any therapeutic agent, solely from its chemical composition, seeing that we possess many very valuable remedies, about the constitution of which we either know nothing, or nothing which will serve to elucidate their mode of action. If, therefore, experience pointed out any difference in the effects of laurel water and of dilute hydrocyanic acid, a proposition to substitute the latter for the former would be unjustifiable; but as it is the opinion of all who have written upon the subject that the action of the two remedies is in every respect the same,³ if we become convinced that we gain an important advantage by the change we should certainly adopt it.

The aqua lauro cerasi is not included among the formulæ of the London Pharmacopœia, but finds a place among those of the Pharmacopœias of Dublin and Edinburgh. The process for its preparation is in both of these essentially the same. One pound of fresh laurel leaves are digested in two and a-half pints of water, and a pint of the liquid is distilled over and filtered. Laurel water, as thus prepared, is nothing more than a dilute solution of hydrocyanic acid, to which the presence of a small quantity of volatile oil gives its peculiar odour and taste. The dose is by no means well fixed, being given by some authorities at from 10 minims to 20 minims, and according to others ranging from 3ss. to 3j.

The leaves of the cherry laurel have never been perfectly examined, but of the nature of two of their constituents there exists no doubt. These are,—firstly, a volatile oil, resembling in its physical properties very closely that obtained from bitter almonds, and in chemical constitution agreeing with it in every particular;

¹ *Dublin Medical Press.*

² According to Göppert, however, the poisonous action of cherry laurel water does not depend on the hydrocyanic acid which it contains, but upon some property peculiar to itself.

³ American Pat. Off. Report, vol. i. p. 495, and vol. iii. p. 166.

secondly, hydrocyanic acid. Both of these substances are, as has been already stated, held in solution by the distillate from the leaves.

The chief cause of the uncertainty of this preparation lies in the fact that the quantity of hydrocyanated oil obtained from the leaves varies with their age. Thus, Christison ("Dispensatory," p. 592) found that 1000 grains of the buds and unexpanded leaves of May and June gave 6.33 grains of oil, but when they had, in July, attained their full size, the same quantity yielded but 3.1 grains, while in the May following the quantity had diminished to 0.6 grains. Not only is the quantity of oil liable to variation, but, what is still more important, the quantity of hydrocyanic acid which it contains is uncertain. It is stated by some writers to contain 2.75 per cent., while according to others who have investigated the subject, it may exist in as large a proportion as 7.66 per cent. Christison confirms this discrepancy, he having obtained from developed young leaves twice as much acid as from old ones. Then, again, while it is well known that the leaves are constantly employed with impunity as a flavouring for articles of food, many cases have occurred in which serious results have supervened from their use in this manner. In a case cited by Dr. Paris ("Med. Jur." vol. ii. p. 400), several children were nearly poisoned by eating a custard so flavoured. Zeller (Quoted by Taylor, "Poisons," p. 717) states that the leaves gathered in wet, cold weather yield more hydrocyanic acid than when they are collected in hot, dry seasons.

From this collection of facts it will be seen that it is impossible for any two samples of laurel water to contain the same proportion of active constituent; but this is by no means the only source of error. Others, which are no less conducive to uncertain composition, creep in during the process of manufacture. As the whole of the oil comes over in the distillation with the first few ounces of water, if the whole product be not strongly agitated before filtration, a uniform preparation will not, even if other conditions be favourable, be obtained. Moreover, constant variations from the process given by the Pharmacopœias are frequently adopted to suit the conveniences of manufacture on the large scale. The most usual of these modifications consists in passing steam through a vessel containing the crushed leaves until the condensed vapour measures the proper quantity. More rarely it is prepared by adding a known proportion of the oil with distilled water, as is practised in the preparation of the aromatic waters of the Pharmacopœia.

In addition to the above sources of inaccuracy, there is one other which has by no means attracted the attention which the importance of the question seems to me to deserve. It is this:—Liebig and Wöhler (Pereira's "Materia Medica," vol. ii. part ii. p. 1782), though unable in their analysis of laurel leaves to detect amygdalin, concur in opinion as to its existence. If this be the case,—and all analogy leads to the inference that it is,—the well-known action by which this body gives rise to hydrocyanic acid should surely not be overlooked, as the duration of the digestion of the leaves in water, —now considered as a matter of but secondary importance,—would then exercise considerable influence over the amount of active matter contained in the product. Closely connected with this part of the question are the results obtained by Pereira ("Materia Medica," vol. ii. part ii. p. 1776) in experimenting on bitter almond oil. He found that a quantity of this substance, which had been carefully purified, and in which he

could not detect a trace of hydrocyanic acid, gave, after having been kept for a few months, distinct evidences of its presence. If we assume—as we can scarcely avoid doing—that an analogy exists between this oil and oil of cherry laurel, another condition of change is introduced, and an increase of hydrocyanic acid in the water must result from its being kept. Experiments in this direction are, however, still wanting.

My next step is naturally to inquire in what degree this inconstancy in the composition of laurel water, which we assume upon theory, is borne out by experience of its effects, and we find that here facts bear us out more fully even than before. A short summary of these facts will, while occupying less space than a more detailed account, at the same time illustrate the case more clearly.

Fonquier (Richard: "Elém. d'Hist. Nat." t. ii. p. 447) has given *twelve ounces* in the course of a day without any evident effect.

In 1781 Sir T. Boughton was poisoned, death resulting in half-an-hour, by *two ounces* (Report of the Trial of Donellan).

In another case, quoted by Taylor (*Op. cit.*, p. 719), *one ounce and a-half* caused death in an adult.

Then, as regards chemical analysis, Pereira (quoted by Taylor) gives the average strength as equal to a solution of hydrocyanic acid containing 0.25 per cent. Two samples which I have examined, and which were obtained from two of our first pharmaceutical laboratories, yielded respectively 0.011 and 0.009 per cent.; in fact, mere traces, the numerical value of which could not, but that large quantities were operated upon, have been stated at all.

Surely further evidence is not needed to show that this preparation is most inconstant, and that even if, on the one hand, no bad results often ensue from its administration, on the other, the practitioner must frequently experience much disappointment from the inactivity of a medicine which he believes to contain 12 per cent. of pharmacopœial hydrocyanic acid.

As it is not likely that in this country, where, from long usage, laurel water has become firmly established, the much more stable and reliable hydrocyanic acid will ever supersede it, I would suggest two methods by which the evil may be, if not wholly remedied, at least palliated, and the preparation at the same time be retained.

The first of these is, that, as laurel leaves can be obtained at all times of the year, the preparation should be much more frequently made, and, when made, be standardised in the manner directed by the London Pharmacopœia for fixing the strength of its dilute hydrocyanic acid. In this way a uniform product—as far, at least, as the acid is concerned—would be obtained.

By the second method the pharmacist would be rendered independent of circumstances which might prevent fresh, and especially young leaves being obtained, and, moreover, the proportion of volatile oil in the liquid, which by the first method could not be constant, would be always the same. I propose, then,—firstly, that the volatile oil be distilled from the leaves, and, by any of the well-known means, wholly deprived of hydrocyanic acid, the subsequent formation of which might be guarded against by keeping some peroxide of mercury in the bottle which contains it; secondly, that a fixed proportion of this oil be dissolved in distilled water, and that hydrocyanic acid of known strength be added to the solution in such quantity as to make it bear an easily-remembered relation of strength to the dilute acid of the Pharmacopœia.

It is clear that the time for such a change will be upon the advent of the New Pharmacopœia; but, by way of example, supposing that a dilute acid of 2 per cent. be decided upon, the following would be an advantageous formula for the preparation of *aqua lauro cerasi*:—

Dilute hydrocyanic acid . . .	35 grs.
Volatile oil of cherry laurel . . .	5 "
Distilled water	4 oz.

This solution would contain 2 per cent. of hydrocyanic acid (which in its turn would contain 2 per cent. of real acid), or, in other words, 50 minims would be equivalent to 1 minim of dilute hydrocyanic acid. From the facility with which this water could be made, no valid excuse could be found for not throwing it away when from any cause it had become deteriorated.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures (adapted to a Juvenile Auditory), on the Chemical History of a Candle; by M. FARADAY, D.C.L., F.R.S., Fullerton Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.

LECTURE I. (Dec. 27, 1860.)—*A Candle: The Flame—its Sources—Structure—Mobility—Brightness.*

I purpose thanking you for the honour you do us in coming to see what are our proceedings here, by bringing before you the Chemical History of a Candle. I have done so on a former occasion, and if I had my own will I should do it almost every year; so abundant is the interest that attaches itself to the subject, so wonderful are the varieties of outlet which it gives into the various departments of philosophy. There is not a law under which any part of this universe is governed which does not come into play and is touched upon in these phenomena. There is no better, there is no more open door by which you can enter into the study of natural physical philosophy, than by considering the phenomena of a candle. Therefore I believe I shall not disappoint you in choosing this for my subject rather than any newer form, which could not be better, if it were so good.

And having said so much to you, let me say this also: that though our subject be so great, and our intention that of treating it honestly, philosophically, and seriously, yet I mean to pass away from all those here who are seniors. I claim the right of speaking to juveniles as a juvenile myself. I have done it on former occasions, and, if you please, I shall do it again. And though I know that I stand here with the knowledge of having the words I utter given to the world, yet that shall not deter me from speaking in the same familiar way to those whom I esteem nearest to me on this occasion. You know that though we make no publication of our proceedings—neither I nor the authorities—we give all facilities to those who honour us by supposing that what they hear here is worth conveying further—we give them every facility to hear us and write about us, but it is entirely their own act. You have here the original, in whatever shape it appears anywhere else.

And now to my boys and girls.

I must first tell you what candles are made of. Some are very curious things. I have here some bits of timber, branches of trees particularly famous for their burning. And here you see a piece of that very curious substance taken out of some of the bogs in Ireland, called *candle-wood*, a hard, strong, excellent wood, evidently fitted for good work as a resister of force, and yet withal burning so well that when it is found they make splinters of it, and torches, since it burns like a candle, and gives a very good light indeed. And here in this wood is one of the most beautiful illustrations of the general nature of a candle that I can possibly give. The fuel provided, the means of bringing that fuel to the place of chemical action, the regular and gradual supply of air to that place of action—heat and light—all produced

by a little piece of wood of this kind, forming, in fact, a natural candle.

But we must speak of candles as they are in commerce. Here are a couple of candles commonly called dips. They are made of lengths of cotton cut off, hung up by a loop, dipped into melted tallow, taken out again and cooled, then redipped, until there is an accumulation of tallow round the cotton. In order that you may have an idea of the various characters of these candles, you see these which I hold in my hand—they are very small and very curious. They are, or were, the candles used by the miners in coal mines. In olden times the miner had to find his own candles, and it was supposed that a small candle would not so soon set fire to the fire-damp in the coal mines as a large one; and for that reason, as well as for economy's sake, he had candles made of this sort, 20, 30, 40, or 60, to the pound. They have been replaced since then by the steel-mill, and then by the Davy-lamp, and other safety-lamps of various kinds. I have here a candle that was taken out of the *Royal George*, it is said, by Sir George Pashley. It has been sunk in the sea for many years, subject to the action of salt water. It shows you how well candles may be preserved, for though it is cracked about and broken a good deal, yet when lighted it goes on burning regularly, and the tallow resumes its natural condition as soon as it is fused.

Mr. Field, of Lambeth, has supplied me abundantly with beautiful illustrations of the candle and its materials; I shall therefore now refer to them. And, first, there is the suet—the fat of the ox—Russian tallow, I believe, employed in the manufacture of these dips, which Gay Lussac, or some one who entrusted him with his knowledge, converted into that beautiful substance, stearine, which you see lying beside it. A candle, you know, is not now a greasy thing like an ordinary tallow candle, but a clean thing, and you may almost scrape off and pulverise the drops which fall from it without soiling anything. This is the process he adopted:—The fat or tallow is first boiled with quick lime, and made into a soap, and then the soap is decomposed by sulphuric acid, which takes away the lime, and leaves the fat rearranged as stearic acid, whilst a quantity of glycerine is produced at the same time. Glycerine—absolutely a sugar, or a substance similar to sugar—comes out of the tallow in this chemical change. The oil is then pressed out of it; and you see here this series of pressed cakes, showing how beautifully the impurities are carried out by the oily part as the pressure goes on increasing, and at last you have left that substance which is melted, and cast into candles as you here see them. The candle I have in my hand is a stearine candle, made of stearine from tallow in the way I have told you. Then here is a sperm candle, which comes from the purified oil of the spermaceti whale. Here also is yellow beeswax and refined beeswax, from which candles are made. Here, too, is that curious substance called paraffin, and some paraffin candles, made of paraffin obtained from the bogs of Ireland. I have here also a substance brought from Japan since we have forced an entrance into that out-of-the-way place—a kind of wax, which a kind friend has sent me, and which forms a new material for the manufacture of candles.

And how are these candles made? I have told you about dips, and I will show you how moulds are made. Let us imagine any of these candles to be made of materials which can be cast. "Cast?" you say, "Why a candle is a thing that melts, and surely if you can melt it you can cast it." Not so. It is wonderful, in the progress of manufacture, and in the consideration of the means best fitted to produce the required result, how things turn up which one would not expect beforehand. Candles cannot always be cast. A wax candle can never be cast. It is made by a particular process which I can illustrate in a minute or two, but I must not spend much time on it. Wax is a thing which, burning so well, and melting so easily in a candle cannot be cast. However, let us take a material that can be cast. Here is a frame, with a number of moulds fastened in it. The first thing to be done is to put a wick through them. Here is one—a plaited wick, which does not require snuffing—supported by a little wire. It goes to the bottom, where it is pegged in—the little peg holding the cotton tight and stopping the aperture, so that nothing fluid shall run out. At the upper part there is a little bar placed across, which stretches the cotton and holds it in the mould. The tallow is then melted, and the moulds are filled. After a certain time, when the moulds are cool, the excess of tallow is poured off at one corner, and then cleaned off altogether,

¹ Reported verbatim by special permission.

and the ends of the wick cut away. The candles alone then remain in the mould, and you have only to upset them, as I am doing, when out they tumble, for the candles are made in the form of cones, being narrower at the top than at the bottom, so that what with their form and their own shrinking, they only need a little shaking and out they fall. In the same way are made these candles of stearine and of paraffin. It is a curious thing to see how wax candles are made. A lot of cottons are hung upon frames, as you see here, and covered with metal tags at the ends to keep the wax from covering the cotton in those places. These are carried to a heater where the wax is melted. As you see, the frames can turn round, and as they turn a man takes a vessel of wax and pours it first down one, and then the next, and the next, and so on. When he has gone once round, if it is sufficiently cool, he gives the first a second coat, and so on until they are all of the required thickness. When they have been thus clothed, or fed, or made up to that thickness, they are taken off and placed elsewhere. I have here, by the kindness of Mr. Field, several specimens of these candles. Here is one only half finished. They are then taken down and well rolled upon a fine stone slab, and the conical top is moulded by properly shaped tubes, and the bottoms cut off and trimmed. This is done so beautifully that they can make candles in this way weighing exactly four, or six, to the pound, or any number you please.

We must not, however, take up more time about the mere manufacture, but go a little further into the matter. I have not yet referred you to luxuries in candles (for there is such a thing as luxury in candles). See how beautifully these are coloured; you see here mauve, Magenta, and all the chemical colours recently introduced, applied to candles. You observe also, different forms employed. Here is a fluted pillar most beautifully shaped; and I have also here some candles sent me by Mr. Pearsall, which are ornamented with designs upon them, so that as they burn, you have as it were a glowing sun above and a bouquet of flowers beneath. All, however, that is fine and beautiful, is not useful. These fluted candles, pretty as they are, are bad candles, they are bad because of their external shape. Nevertheless, I show you these specimens sent to me from kind friends on all sides that you may see what is done and what may be done in this or that direction, though, as I have said, when we come to these refinements, we are obliged to sacrifice a little in utility.

Now as to the light of the candle. We will light one or two, and set them at work in the performance of their proper functions. You observe a candle is a very different thing from a lamp. With a lamp you take a little oil, fill your vessel, put in a little moss of some cotton prepared by artificial means, and then light the top of the wick. When the flame runs down the cotton to the oil, it gets extinguished, but it goes on burning in the part above. Now, I have no doubt, you may ask, how is it that the oil which will not burn of itself gets up to the top of the cotton where it will burn? We shall presently examine that; but there is a much more wonderful thing about the burning of a candle than this. You have here a solid substance with no vessel to contain it; and how is it that this solid substance can get up to the place where the flame is? How is it that this solid gets there, it not being a fluid? or, when it is made a fluid, then how is it that it keeps together? This is a wonderful thing about a candle.

We have here a good deal of wind, which will help us in some of our illustrations, but tease us in others; for the sake, therefore, of a little regularity, and to simplify the matter, I shall make a quiet flame, for who can study a subject when there are difficulties in the way not belonging to it? Here is a clever invention of some costermonger or street stander in the market-place for the shading of their candles on Saturday nights, when they are selling their greens, or potatoes, or fish. I have very often admired it. They put a lamp-glass round the candle, supported on a kind of gallery, which clasps it, and it can be slipped up and down as required. By the use of this lamp-glass, employed in the same way, you have a steady flame, which you can look at, and carefully examine, as I hope you will do, at home.

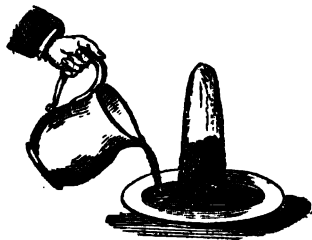
You see then, in the first instance, that a beautiful cup is formed. As the air comes to the candle it moves upwards by the force of the current which the heat of the candle produces, and it so cools all the sides of the wax, tallow, or fuel, as to keep the edge much cooler than the part within; the part within melts by the flame that runs down the wick as far as it can go before it is extinguished, but the part on the outside does

not melt. If I made a current in one direction, my cup would be lop-sided, and the fluid would consequently run over,—for the same force of gravity which holds worlds together holds this fluid in a horizontal position, and if the cup be not horizontal, of course the fluid will run away in guttering. You see, therefore, that the cup is formed by this fine, uniform, regular ascending current of air upon all sides which keeps the exterior of the candle cool. No fuel would do for a candle which has not the property of giving this cup, except such fuel as the Irish bogwood, where the thing is like a sponge and holds its own fuel. You see now, why you would have had such a bad result if you were to burn these beautiful candles that I have shown you, which are irregular, intermittent in their shape, and cannot, therefore, have that nicely-formed edge to the cup which is the great beauty in a candle. I hope you will now see that the perfection of a process, that is, its utility, is the better point of beauty about it. It is not the best-looking thing, but the best-acting thing which is the most advantageous to us. This good-looking candle is a bad burning one. There will be a guttering round about it because of the irregularity of the stream of air and the badness of the cup which is formed thereby. You may see some pretty cases (and I expect you to think of these things) of the action of the ascending current when you have a little gutter run down the side of a candle, making it thicker there than it is elsewhere. As the candle goes on burning, that keeps its place and forms a little pillar sticking up by the side, because, as it rises higher above the rest of the wax or fuel the air gets better round it, it is more cooled and better resists the action of the heat at a little distance. Now, the greatest mistakes and faults with regard to candles, as with regard to other points, often bring with them instruction which we should not receive if they had not occurred. We come here to be philosophers, and I hope you will always remember that whenever a result happens, especially if it is new, you should say, "What is the cause? Why does that occur?" and you will in the course of time find it out.

Then there is another point about these candles which will answer a question,—that is, as to the way in which this fluid gets out of the cup, up the wick, and into the place of combustion. You know that the flames on these wicks burning in candles made of beeswax, or stearine, or spermaceti, do not run down to the wax or other matter, and melt it all away, but keep to their own right place. They are fenced off from the fluid below, and do not encroach on the cup at the sides. I cannot imagine a more beautiful and more compact thing than the condition of adjustment under which a candle makes one part subservient to the other to the very end of its action. A combustible thing like that, burning away gradually, never being intruded upon by the flame,—is a very beautiful sight; especially when you come to learn what a vigorous thing flame is—what power it has of destroying the wax itself when it gets hold of it, and destroying its proper form even before it gets hold of it, if it come too near.

Now, how does it get hold of it? There is a beautiful point about that—*capillary attraction*. "Capillary attraction!" you say,—"*the attraction of hairs*." Well, never mind the name; it was given in old times before we had a good understanding of what the real power was. It is by what is called capillary attraction that the fuel is conveyed to the part where combustion goes on, and is deposited there, not in a careless way, but very beautifully in the very midst of the centre of action, which takes place around it. Now I am going to give you one or two instances of capillary attraction. It is that kind of action or attraction which makes two things that do not dissolve in each other still hold together. When you wash your hands you wet them thoroughly; you take a little soap to make the adhesion better, and you find your hand remains wet. This is by that kind of attraction of which I am about to speak. And what is more; if your hands are not soiled (as they almost always are by the usages of life), if you put your finger into a little warm water, the water will creep a little way up the finger, though you may not stop to examine it. I have here a substance which is rather porous—a column of salt,—and I will pour into the plate at the bottom, not water as it appears, but a saturated solution of salt which cannot absorb more; so that the action which you see, will not be due to its dissolving anything. We may consider the plate to be the candle, and the salt the wick, and this solution the melted tallow. (I have coloured the fluid that you may see the action better.) You observe that now I pour in the fluid, it rises and gradually

creeps up the salt higher and higher; and provided the column does not tumble over, it will go to the top. If this



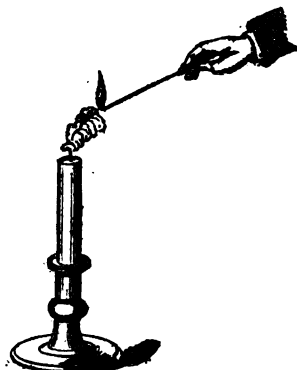
blue solution were combustible, and we were to place a wick at the top of the salt, it would burn as it entered into the wick. It is a most curious thing to see this kind of action taking place, and to observe how singular some of the circumstances are about it. When you wash your hands you take a towel to wipe off the water, and it is by that kind of wetting, or that kind of attraction which makes the towel become wet with water, that the wick is made wet with the tallow. I have known some careless boys and girls (indeed, I have known it happen to careful people as well) who, having washed their hands and wiped them with a towel, have thrown the towel over the side of the basin, and before long it has drawn all the water out of the basin and conveyed it to the floor, because it happened to be thrown over the side in such a way as to serve the purpose of a syphon. That you may the better see the way in which the substances act one upon another, I have here a vessel made of wire gauze filled with water, and you may compare it in its action to the cotton in one respect, or to a piece of calico in the other. In fact, wicks are sometimes made of a kind of wire gauze. You will observe that this vessel is a porous thing, for if I pour a little water on to the top, it will run out at the bottom. You would be puzzled for a good while if I asked you what the state of this vessel is, what is inside it, and why it is there? The vessel is full of water, and yet you see the water goes in and runs out as if it were empty. In order to prove this to you I have only to empty it. The reason is this—the wire being once wetted, remains wet; the meshes are so small that the fluid is attracted so strongly from the one side to the other, as to remain in the vessel although it is porous. In like manner the particles of melted tallow ascend the cotton and get to the top; other particles then follow by their mutual attraction for each other, and as they reach the flame they are gradually burned.

Here is another application of the same principle. You see this bit of cane. I have seen boys about the streets, who are very anxious to appear like men, take a piece of cane and light it and smoke it, as an imitation of a cigar. They are enabled to do so by the permeability of the cane in one direction, and by its capillarity. If I place this piece of cane on a plate containing some camphine (which is very much like paraffin in its general character), exactly in the same manner as the blue fluid rose through the salt will this fluid rise through the piece of cane. There being no pores at the side, the fluid cannot go in that direction, but must pass through its length. Already the fluid is at the top of the cane, and now I can light it and form it into a candle. The fluid has risen by the capillary attraction of the piece of cane, just as it does through the cotton in the candle.

Now, the only reason why the candle does not burn all down the side of the wick is that the melted tallow extinguishes the flame. You know that a candle, if turned upside down, so as to allow the fuel to run upon the wick, will be put out. The reason is, that the flame has not had time to make the fuel hot enough to burn, as it does above where it is carried in small quantities into the wick, and has all the effect of the heat exercised upon it.

There is another condition which you must learn as regards the candle, without which you would not be able fully to understand the philosophy of it, and that is the vaporous condition of the fuel. In order that you may understand that, let me show you a very pretty, but very common-place experiment. If you blow a candle out cleverly, you will see the vapour rise from it. You have, I know, often smelt the vapour of a blown-out candle, and a very bad smell it is; but if you blow it out cleverly, you will be able to see pretty well the vapour into which this solid matter is transformed.

I will blow out one of these candles in such a way as not to disturb the air round about it by the continuing action of my breath; and now, if I hold a lighted taper two or three inches from the wick, you will observe a train of fire going



through the air till it reaches the candle. I am obliged to be quick and ready, because if I allow the vapour time to cool, it becomes condensed into a liquid or solid, or the stream of combustible matter gets disturbed.

Now, as to the shape or form of the flame. It concerns us much to know about the condition which the matter of the candle finally assumes at the top of the wick, where you have such beauty and brightness as nothing but combustion or flame can produce. You have the glittering beauty of gold and silver, and the still higher lustre of jewels like the diamond and ruby; but nothing of these comes by comparison near to the brilliancy and beauty of flame. What diamond can shine like flame? It owes its lustre at night time to the very flame shining upon it. The flame shines in darkness, but the light which the diamond has is as nothing, until the flame shine upon it, when it is brilliant again. The candle alone shines by itself and for itself, or for those who have arranged the materials. Now, let us look a little at the form of the flame as you see it under the glass shade. It is steady and equal, and its general form is that which is represented in the diagram, varying with atmospheric disturbances, and also varying according to the size of the candle. It is a



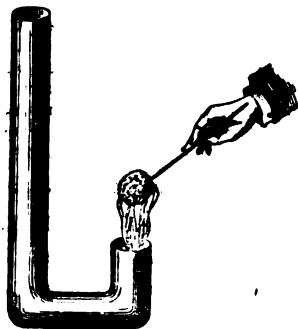
bright oblong, brighter at the top than towards the bottom, with the wick in the middle, and besides the wick in the middle certain darker parts towards the bottom where the ignition is not so good as in the part above. I have a drawing here, made many years ago by Hooker, when he made his investigations. It is the drawing of the flame of a lamp, but it will apply to the flame of a candle. The cup of the candle is the vessel or lamp; the melted spermaceti is the oil; and the wick is common to both. Upon that he sets this little flame, and then he represents what is true, a certain quantity of matter rising about it which you do not see, and which, if you have not been here before, or are not familiar with the subject, you will not know of. He has here represented the parts of the surrounding atmosphere that are very essential to the flame and that are

always present with it. There is a current formed, which draws the flame out, for the flame which you see is really drawn out by the current, and drawn upward to a great height, just as Hooker has here shown you by that prolongation of the current in the diagram. You may see this by taking a lighted candle, and putting it in the sun so as to get its shadow thrown on a piece of paper. What a remarkable thing it is that that thing which is light enough to produce shadows of other objects, can be made to throw its own shadow on a piece of white paper or card, so that you can actually see streaming round the flame something which is not part of the flame, but is ascending and drawing the flame upwards. Now I am going to imitate the sun-light, by applying the voltaic battery to the electric lamp. You now see our sun, and its great luminosity; and by placing a candle between it and the screen, we get the shadow



of the flame. You observe the shadow of the candle, and of the wick; then there is a darkish part, as represented in the diagram, and then a part which is more distinct. Curiously enough, however, what we see in the shadow as the darkest part of the flame, is, in reality, the brightest part, and here you see streaming upwards the ascending current of hot air, as shown by Hooker, which draws out the flame, supplies it with air, and cools the sides of the cup of melted fuel.

I can give you here a little further illustration for the purpose of showing you how flame goes up or down according to the current. I have here a flame,—it is not a candle flame,—but you can, no doubt, by this time generalise enough to be able to compare one thing with another,—what I am about to do is to change the ascending current that takes the flame upwards into a descending current. This I can easily do by the little apparatus you see before me. The flame, as I have said, is not a candle flame, but it is produced by alcohol so that it shall not smoke too much. I will also colour the flame with another substance, so that you may trace its course, for with spirit alone you could hardly see enough to have the opportunity of tracing its course of action. By lighting this spirit-of-wine, we have then a flame produced, and you observe that when held in the air it naturally goes upwards. You understand now



easily enough why flames go up under ordinary circumstances—it is because of the draught of air by which the

combustion is formed. But now by blowing the flame down you see I am enabled to make it go downwards into this little chimney, the direction of the current being changed. Before we have concluded this course of lectures we shall show you a lamp in which the flame goes up, and the smoke goes down, or the flame goes down and the smoke goes up. You see, then, that we have the power in this way of varying the flame in different directions.

There are now some other points that I must bring before you. Many of the flames you see here vary very much in their shape by the currents of air blowing around them in different directions; but we can, if we like, make flames so that they will look like fixtures, and we can photograph them—indeed, we have to photograph them—so that they become fixed to us, if we wish to find out everything concerning them. That, however, is not the only thing I wish to mention. If I take a flame sufficiently large, it does not keep that homogeneous, that uniform condition of shape, but it breaks out with a power of life which is quite wonderful. I am about to use another kind of fuel, but it is truly and fairly representative of the wax or tallow of a candle. I have here a large ball of cotton, which will serve as a wick. And, now that I have immersed it in spirit and lit it, in what way does it differ from an ordinary candle? Why it differs very much in one respect, that we have a vivacity and power about it, a beauty and a life utterly unlike the light presented by a candle. You see those fine tongues of flame rising up. You have the same general disposition of the mass of the flame from below upwards, but, in addition to that, you have this remarkable breaking out into tongues which you do not perceive in the case of a candle. Now, why is this? I must explain it to you, because when you understand that perfectly you will be able to follow me better in what I have to say hereafter. I suppose some here will have made for themselves the experiment I am going to show you. Am I right in supposing that anybody here has played at snapdragon? I do not know a more beautiful illustration of the philosophy of flame, as to a certain part of its history, than the game of snapdragon. First, here is my dish; and let me say, that when you play snapdragon well you ought to have the dish well warmed; you ought also to have warm plums, and warm brandy, which, however, I have not got. When you have put the spirit into your dish, you have the cup and the fuel; and are not the raisins acting like the wicks? I now throw the plums into the dish, and light the spirits, and you see those beautiful tongues of flame that I have referred to. You have the air creeping in over the edge of the dish forming these tongues. Why? Because through the force of the current, and the irregularity of the action of the flame, it cannot flow in one uniform stream. The air flows in so irregularly that you have, what would otherwise be a single image, broken up into a variety of forms, and each of these little tongues has an independent existence of its own. Indeed, I might say, you have here, a multitude of independent candles. You must not imagine that because you see these tongues all at once that the flame is of this particular shape. A flame of that shape is never so at any one time. Never is a body of flame, like that which you just saw rising from the ball, of the shape it appears to you. It consists of a multitude of different shapes, succeeding each other so fast that the eye is only able to take cognisance of them all at once. In former times, I purposely analysed a



flame of that general character, and this diagram shows you the different parts of which it is composed. They do not occur all at once; it is only because we see these shapes in such rapid succession, that they seem to us to exist all at one time.

It is too bad that we have not got further than my game of snapdragon, but we must not, under any circumstances, keep you beyond your time. It will be a lesson to me in future to hold you more strictly to the philosophy of the thing than to take up your time so much with these illustrations.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, December 11, 1860.

Dr. J. P. JOULE, President, in the Chair.

Dr. FAIRBAIRN brought before the meeting four specimens of Submarine Telegraphic Cable, as constructed by Messrs. Hall and Wells. This cable has a copper wire insulated by india rubber in the centre for the transmission of the electric current. Outside of this are twenty longitudinal strands of hemp steeped in pitch and cork dust, and eight steel wires braided together with twenty-four strands of hemp saturated with Stockholm tar. The specific gravity of the cable in sea water is 1.4 and its weight in air 0.82 ton per mile. The length that would break with its own weight when suspended in sea water is 10,810 fathoms; its tensile strength being 2.875 tons. Dr. Fairbairn presented an account of experiments which had been made on the elongation of a sample of the cable twenty feet long by the application of different tensile forces. With a force of 4,480 lbs. there was an elongation of half-an-inch, and after the weight had been removed the cable was found to be permanently stretched $\frac{1}{8}$ ths of an inch. With a force of 6,440 lbs. the cable broke after having stretched $1\frac{1}{8}$ inches.

Professor Roscoe explained the recent discoveries by Bunsen and Kirchhoff of the lines in the spectrum produced by various substances when ignited in the flame of a laboratory lamp. He exhibited beautiful chromo-lithographic drawings of the spectra produced by lithia and various other earths and alkalis. Lithia, which had formerly been supposed to be a very rare earth, was by this means proved to be one of those most extensively distributed. Professor Roscoe stated that Bunsen had, by this new and most delicate system of analysis, been led to the discovery of a new metal which was present in a mineral spring in so small a quantity that twenty tons had to be boiled down to obtain 250 grains of the metal.

A Paper was read by the Rev. W. N. MOLESWORTH, M.A., entitled, "*On the Origin of Species.*"

PHYSICAL AND MATHEMATICAL SECTION.

November 8, 1860.

Mr. Baxendell was elected a Vice-President of the Section, in place of the late Mr. Long.

Mr. BAXENDELL read a paper, "*On a System of Periodic Disturbances of Atmospheric Pressure in Europe and Northern Asia.*"

[This paper was afterwards read at the Ordinary Meeting of the Society, on November 13.—See CHEMICAL NEWS, vol. ii. No. 52]

December 6, 1860.

Mr. George Mosley was elected Treasurer of the Section, in place of Mr. Baxendell.

Mr. ATKINSON read a paper, entitled "*Remarks on Abnormal Disturbances of the Barometrical Column at certain Seasons of the Year.*"

Mr. Atkinson considers that all the movements in the atmosphere of our earth, which have received the designation of *irregular*, are caused by the reflected or radiated heat of the sun, and take place at a very moderate elevation, say within five or six miles of the general surface level;—and that these apparently irregular movements, or

shiftings from place to place of lighter and heavier air, causing oscillations in the barometric column, are mainly if not wholly due to irregularities of the earth's surface. Had our earth been a globe possessing a smooth surface of uniform texture and properties, it seems clear that the atmosphere would have been acted upon by the reflected and radiated heat of the sun, in a manner so much in accordance with a uniform sequence of physical effects, that the periodic movements of the gases composing it would have been as regular as the planetary motions themselves. In the northern parts of our hemisphere, it appears by Mr. Baxendell's valuable paper, read before the General Meeting of this Society on November 13, that the barometric oscillations are least in amount when the sun is on or near the equator. This fact points to the inference that if the plane of the earth's orbit had coincided with the plane of its equator the disturbance of the barometric column would have been comparatively small and nearly uniform throughout the year. The coincidence of these two planes not existing, it is found, that as the sun retreats from the equator towards the southern tropic, the sum of the oscillations of mercury gradually increases for a considerable time, and then rapidly mounts up so fast as to form a prominence in Mr. Baxendell's curves resembling a mountain peak. This peak or summit of the "dynamical curve" occurs above different points of its axis—that is, at different periods of time—according to some peculiarity in the position—different from the latitude or the longitude—of the locality from which the data for constructing the curves were derived.

Speaking of the northern hemisphere, as the sun withdraws southward from the equator, less or greater portions of the northern part of the terraqueous surface becomes cooled down gradually to the freezing point, according to various peculiarities of substance, elevation above the sea-level, proximity to the open ocean, or to far-inland mountain ranges, and to other analogous causes. In similar latitudes, from the varying conditions just mentioned, there will exist, side by side, spots differing, or having a tendency to differ, very much in temperature, and where consequently currents of different density—set in motion by the constant struggle going on in the air to attain a state of equilibrium—will cause frequent fluctuations in the barometer. These disturbing causes will, in any region, be much increased at the setting in of winter and the commencement of hard frost; for at this crisis a large amount of latent heat will be liberated and will contribute its influence to disturb the equilibrium of the air. A similar crisis will occur at the end of winter on the breaking up of the frost, and will necessarily be attended with similar results. As the times of these *crises* appear to correspond in a remarkable manner with the times of maximum disturbance of the barometrical column, it seems but fair to infer that a relation exists between the causes here stated to be in operation, at the critical periods just named, and the periodical disturbance of the mercury in the barometer indicated by Mr. Baxendell's "dynamical curves." The correctness of this inference, or the contrary, can only be established by future observation of phenomena, and the collection of facts, many of them of a kind seldom thought of hitherto as constituting elements for the solution of problems in meteorology.

ROYAL DUBLIN SOCIETY.

Monday, December 17, 1860.

Mr. GILBERT SANDARS in the Chair.

A paper, by Mr. G. W. JACKSON and Mr. J. W. WONFOR, students in the Laboratory of the Museum of Irish Industry, "*On the Composition of Dublin Porter.*" was read by Mr. Wonfor. The authors described the methods they adopted for

the estimation of the different organic and inorganic substances. The following are the results of their analyses :—

Specific gravity of the porter . 1019.565

The following are the amounts of the various constituents:—

IN 1000 PARTS, BY MEASURE, OF THE PORTER.

	I.	II.	Mean.
Total amount of fixed organic matter	67.566	66.426	66.996
Total amount of fixed inorganic matter	4.166	4.270	4.218
	<u>71.732</u>	<u>70.696</u>	<u>71.214</u>
Proof spirit	89.6	92.0	90.8
Acetic acid	<u>3.6</u>	<u>3.6</u>	<u>3.6</u>
Grape sugar	3.44	3.44	3.44
Albumen	8.272	7.50	7.886
Extractive matter	55.854	55.486	55.670
Silica	.290	.290	.290
Phosphoric acid	5.44		
Magnesia	3.07		
	<u>8.51</u>		
	Phosphate of Magnesia	8.51	8.51
Lime	.085	.085	.085
Phosphoric acid	.690	.703	.697
Chloride of sodium	.448	.449	.448
Sulphuric acid	.277	.274	.276
Potash	1.527	1.500	1.513
Soda	.082	.083	.082
	<u>71.816</u>	<u>70.661</u>	<u>71.238</u>

IN ONE GALLON, BY MEASURE.

Total amount of fixed organic matter	.	.	4689.70
Total amount of fixed inorganic matter	.	.	297.64
			4987.34

Alcohol (proof spirit)	6356
Acetic acid.	252
Sugar.	240'80
Albumen	552'00
Extractive matter	3896'90
Silica	20'30
Phosphate of magnesia	59'71
Phosphate of lime	11'06
Phosphoric acid	44'31
Sulphate of potash	42'00
Potash	83'16
Chloride of sodium	31'36
Soda	5'74
	4987'34

They stated that in giving the composition in 1000 parts they gave the bases and acids separately, with the exception that the chlorine they combined with sodium and the magnesia with phosphoric acid, as hypophosphate of magnesia; in the gallon they combined the lime with phosphoric acid, giving it the composition of bone-earth; the rest of the phosphoric acid and the potash and soda they left uncombined. The porter examined was Guinness's XXX.

Dr. WILLIAM BARKER read a paper, "*On the Amount of the Rain-fall in Ireland during late Years.*"

Dr. DAVY read a paper, "*On some Further Applications of the Ferrocyanide of Potassium in Chemical Analysis.*"

NOTICES OF BOOKS, PATENTS, &c.

Blue Colour for Fibres and Fabrics.

CHARLES COWPER, 20, Southampton Buildings, Chancery Lane.

(Communication from C. M. T. DU MOTAY, of Paris.)

THE patentee states the very interesting and remarkable fact that soluble Prussian blue and carmine of indigo, when mixed in the proportion of their equivalents, dissolve each other, and combine to form a new blue colour of definite chemical composition. It differs from blues generally in the important particular of retaining the same tint in solar and artificial light. The cause of this valuable property appears to lie in the circumstance that the red tint of the indigo is corrected by the supplementary green contained in the Prussian blue; the result being a pure or neutral blue. Another valuable property of this colour is that the green, formed by an admixture of it with yellow, also preserves the same tone in natural and artificial light.

Preparation.—Five pounds of prussiate of potash are dissolved in fifteen pounds (1½ gallons) of water. Two and a-quarter pounds of persulphate of iron (*sic*) are then boiled with one and three-quarter pounds of oil of vitriol, and eight pounds of water are added. [We need scarcely, perhaps, remind any of our readers who may feel inclined to try this process, of the danger of incautiously mixing water with hot oil of vitriol, and *vice versâ*.] The two solutions, prepared as above, are then to be mixed. The resulting precipitate of Prussian blue is to be thrown on a filter and drained. An equivalent of this soluble Prussian blue is then to be mixed with an equivalent of the carmine of indigo. The mixture is soluble in water, and forms the new colour. The solubility of this blue in water presents an interesting anomaly. Water, at a temperature of from 104° to 122° F., dissolves it better than when cold, but, on raising the heat above 176°, it is re-precipitated. This precipitation may be prevented by adding to the bath oxalic acid or an oxalate in the proportion of two pounds of the acid or four of the salt to every ten gallons of water. Such a bath will require ten or fifteen pounds of the new blue.

The presence of lime in water diminishes its capability of dissolving the colour. Silk dyes best between the temperature of 77° and 95° F., and wool between 140° and 176° F. The latter temperature should not be exceeded.

This very interesting patent discloses an entirely novel class of combinations. For the sulphindigotates of the alkaline bases (carmine of indigo) to combine with a complex body like Prussian blue to form a species of double salt is very anomalous, and we look with anxiety for a paper from M. du Motay on the chemical composition of this substance.

For the convenience of such of our readers as are not familiar with these compounds, we may mention that the general formula of the sulphindigotates is $C_6H_4MNO_2S_2O_6$. Substituting any metal for M we have, of course, the formula for the sulphindigotates of that metal.

Fertilising Compound for Agricultural Purposes.

M. A. F. MENNOES, of Paris, patents a method of working up urine into a manure. He takes advantage of Dr. Stenhouse's observations regarding charcoal. The absorbent power of gypsum is also made use of. The patentee places the urine to putrify in reservoirs closed by wire-gauze trays, containing a mixture of 2 to 5 quarts of pulverised vegetable charcoal, and 5 to 10 lbs. each of mould and gypsum. As soon as the mixture has become saturated with the gases, &c., it is taken from the tray and mixed with 25 gallons more urine; also 2½ lbs. of each of

the following ingredients,—phosphate of ammonia, phosphate of lime, ammonio-magnesian-phosphate, nitre, nitrate of soda, sulphate or nitrate of magnesia, common salt, and about 4½ lbs. weight of sulphate of iron or of crude sulphate of zinc and magnesia mixed in equal parts; he then adds 14 lbs. of gypsum and, to bring the whole to a pasty consistence, 2 cwt. of burnt-clay is added. The mass is then dried and powdered. The process appears to be complicated, troublesome, and by no means satisfactory. Any method is more or less defective which requires us to add large quantities of such substances as gypsum and burnt-clay. Moreover, the valuable ingredients of urine may be separated by far more convenient processes. The patent is dated 4th January, 1860.

Separation of Silver and Lead.

P. J. WORSLEY, of Rotherhithe, patents a plan for the above, which appears to us to be almost absolutely the same as that generally known as Pattinson's crystallising process,—the chief difference being that, instead of fishing out the crystal with perforated ladles, the pots have strainers in them, and a hole at the bottom to allow of the liquid metal being run off. The process may be an improvement, but if such a modification be allowed to be worked as an original invention, there is not much encouragement for discoverers to patent their discoveries.

Preparation of Chlorine.

M. C. P. P. LAURENTS, of Rouen, patents the use of chloride of copper as a source of chlorine, as it gives that element off on heating, and may be reconverted into chloride by the action of hydrochloric acid an indefinite number of times. The patentee claims any metallic chloride capable of doing the same thing. This reaction is not in the least novel; chemists have been aware for many years that chloride of copper heated to a red heat gave off half its chlorine, and became converted into the sub-chloride. We doubt whether such a patent could stand for an instant in a court of law. We do not believe that the process possesses the merit of being economical; but if it is, and we wanted to obtain chlorine in quantity, we think we could find much more convenient sources of it than chloride of copper.

For Rendering Cotton and other Goods Uninflammable.

S. ROWBOTTOM, of Putney, and T. GRAFTON, of Derby, patent the borates (neutral and acid) for this purpose. The method of applying them is pretty much the same as with the numerous other salts employed with the same intention.

Chimie Organique fondée sur la Synthèse. Par MARCELLIN BERTHELOT. Paris: 1860.

ANALYSIS and Synthesis are the two opposite aspects of the chemical conception of Nature. In Mineral Chemistry we have long proceeded by the latter method, building up the more from the less complicated; while in Organic Chemistry the exact reverse has been the case. The chemist, in his fermentations, combustions, destructive distillations, operates for the most part analytically, and in direct opposition to the action of living Nature, which reconstructs the edifice broken down by these chemical processes. It is the object of the present treatise to show how the synthetical laws of Mineral Chemistry may be applied to the formation of organic compounds; to point out, in fact, how Organic Chemistry, instead of starting from the phenomena of life only, possesses an independent origin.

The two ponderous volumes, in which the author

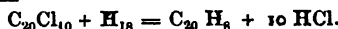
expounds his views and details his experiments, are replete with interesting facts and suggestions, but there is in them much repetition, and the style is in many places exceedingly diffuse. We shall scarcely attempt to do more than quote a few illustrative examples of synthetical methods, referring our readers to the work itself for further information.

From carbon we may obtain carbonic oxide; then by combining this with water, formic acid is produced; this may be converted into a metallic formate, which, submitted to distillation, gives rise to various hydro-carbons, such as marsh-gas, olefiant-gas, and propylene. These hydro-carbons become, in their turn, means of constructing alcohols, for marsh-gas, united with oxygen, becomes methylic alcohol, and olefiant-gas, with the elements of water, constitutes ethylic alcohol.

It is in the first steps, the production of the hydro-carbons and the alcohols, that the chief difficulties of organic synthesis lie; the subsequent developments, such as the formation of the aldehydes, the acids, the ethers, the amines, and the amides being comparatively easy. Given any one of the four simple carbon compounds,—CO₂, CS₂, C₂Cl₄, C₂H₄, the other three may be obtained from it. Take the chloride as an instance. This body, heated with a mineral sulphide, yields CS₂; with an alkali, CO₂; and at a dull red heat with hydrogen it produces C₂H₄. So we may return in each case from the transformed to the primitive substance.

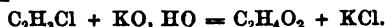
A résumé of the historical development of Organic Chemistry, and an account of the general laws of reactions, and the influence under which they are effected is given. One thing we must here mention,—the inadequate references to the labours of Laurent and Gerhardt: in his infrequent notices of their discoveries, the author sometimes contrives to misrepresent their views (e. g. p. lxxxv. of the Introduction); if it be unintentional the mistake is unfortunate; but if intentional, it is indeed to be deplored. The subjects treated of in the Introduction, which extends to 188 pages, are grouped as follows:—§ 1. Analysis and Synthesis. § 2. Elements and Immediate Principles. § 3. Elementary Composition; Equivalents. § 4. Analysis by Gradual Decomposition. § 5. Chemical Functions of Organic Bodies, and their Classification. § 6. Of Synthesis in Organic Chemistry.

In the First of the Four Books, into which the author divides the work itself, the synthesis of hydrocarbons is described. The production of formic acid from carbonic oxide and water is here the preliminary step, for by the distillation of formiates various hydrocarbons may be formed. One of the most interesting examples given is the formation of naphthaline from the so-called "chloride of Julin," C₂₀Cl₁₀, which is found among the last products of the decomposition of the chloride of carbon. The reaction is—



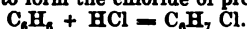
As we mentioned above, olefiant gas appears among the products of the dry distillation of formiates; from this gas alcohol may be prepared, and thence acetic acid. From acetates, propylene, butylene, and amylene may be produced. From alcohol itself, by submitting its vapour to a red heat, benzole, naphthaline, and phenic acid are obtainable.

Book II details the synthetical processes by which the various alcohols may be produced. In marsh-gas, the hydride of methyle, 1 equivalent of chlorine may be substituted for 1 equivalent of hydrogen, and the true chloride of methyle thus formed. This, heated with potash-solution at 100° C. for a week, yields methylic alcohol—



The author proceeds to describe at length the procedure in the case of ethylic alcohol produced from olefiant-gas, and of propylic alcohol from propylene. In the former instance the result is attained by means of sulphuric acid,

and the formation of an ethylsulphate; in the latter, by means also of hydrochloric acid, with which propylene unites directly to form the chloride of propyle—



Sect. 8 contains an interesting account of the formation of the benzylic, cymenic and cinnamic alcohols. From toluole, C_6H_5 , we may prepare the chloride of toluenyle, C_6H_5Cl , from this the acetate of toluenyle, $C_6H_5O_2C$, and from this benzoic (called by Berthelot *benzylic*) alcohol. This alcohol may also be formed by taking as a starting point benzoic acid, the successive steps being the formation of the chloride of benzoyle, $C_6H_5O_2Cl$; the cyanide, $C_6H_5O_2Cy$; and the hydride or benzoic aldehyde, $C_6H_5O_2H$.

Berthelot gives the following scheme, by means of which we may arrive at the higher terms of the benzoic series:—

Starting from benzoic acid	$C_6H_5O_2$
we may prepare benzylic alcohol	$C_6H_5O_2H$
and from this the cyanide	$C_6H_5O_2Cy$
which boiled with potash yields an acid	$C_{10}H_7O_2$
and which, by analogy, should give an aldehyde	$C_{10}H_7O_2H$
and thence xylene alcohol	$C_{10}H_7O_2H$
from this the derived cyanide	$C_{14}H_9O_2Cy$
might be obtained, and thence an acid	$C_{14}H_9O_2$
and so on, consecutively, we might pass to cumolic	$C_{18}H_{11}O_2$
and to cymenic alcohol	$C_{20}H_{13}O_2$

But, as Berthelot observes, it is easier to indicate than to realise this series of reactions. Several chemists have, however, already made some progress in this direction, and it offers a field of inquiry full of promise. In the above list we have used the terms assigned by Berthelot to the various compounds mentioned, but we shall have a word to say on this nomenclature further on.

In Sect. 9 the author gives a method for the formation of campholic alcohol by heating camphor with potash. The campholic series is as follows:—

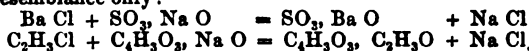
Campholic hydride (camphene)	$C_{20}H_{16}$
chloride	$C_{20}H_{15}Cl$
alcohol (camphol)	$C_{20}H_{16}O_2$
aldehyde (camphor)	$C_{20}H_{14}O_2$
Camphic acid	$C_{20}H_{16}O_4$
Camphoric acid	$C_{20}H_{14}O_8$

The extracts and remarks which we have now made will give some idea of the nature of the work before us, and our limits will not allow us to do more than quote the titles of some of the sections which constitute the remainder of the work. We have details concerning cholesterine, or cholesteric alcohol and its ethers, concerning the derivatives of the alcohols properly so called, concerning the nomenclature of organic compounds, concerning ethers, hydrocarbons, alkalies, compound metallic radicals, aldehydes, acetones, acids, amides, cyanides and cyanic compounds, polyatomic alcohols and aldehydes, and phenoles. The Third Book contains an elaborate account, well worth attentive study, of glycerine and its numerous derivatives, and also of the various kinds of sugar. The Fourth Book relates to "Methods."

We cannot conclude the present notice of M. Berthelot's treatise without a few observations on his formulæ and nomenclature.

Berthelot rejects the expression toluole, cumole, &c., because they refer to acids whose carbon equivalents are higher than those of these derived bodies; and he, in consequence, introduces a further confusion into the names of these compounds. For instance, toluole becomes *benzoëne*, and its derivatives therefore liable to be mistaken for those of benzine; thus the alcohol corresponding to the former hydro-carbon is named the *benzylic*, that corresponding to the latter the *benzenic*. So also cumenic alcohol, $C_{20}H_{14}O_2$, is termed "cymenic alcohol," while a similar change is made in the names of the lower members of the series. The author's reasons for such awkward altera-

tions seem by no means conclusive. He says he has abandoned the expressions "hydride of ethyle," "chloride of ethyle," &c., because they rest upon an unproven hypothesis. Other names, as those of the compound ethers, are rejected, because they are founded on hypothesis, and tend to affirm an illusory resemblance between ethers and salts. We suggest that such resemblance is by no means altogether illusory. What is the meaning of the following parallel equations, quoted from M. Berthelot's own work,—do they establish an illusory resemblance only?



The reactions are parallel, and the products present analogies quite as distinct as could be expected between bodies whose constituents are so different, especially when the complicated character of organic compounds, and the multitude of metamorphoses to which they are consequently subject, is considered. So, according to our author, all formulæ are to be rejected which rest upon hypothesis; but are there not at least four hypotheses contained in the expression given by Berthelot for the propylsulphate of baryta?—



We confess to have been surprised at the parallelism which Berthelot insists on, between the oxides and chlorides of organic radicals. The author seems to pride himself on a return to obsolete or obsolescent formulæ and views; with the remembrance of Williamson's and Gerhardt's researches fresh in our minds, the following equation scarcely pleases us,—



Surely the expression methylchlorhydric ether involves as many suppositions as the simpler form, chloride of methyle. Ether, C_4H_5O , becomes hydric ether with the formula C_4H_5O . Anhydrous formic acid is actually written with the formula C_2HO_3 , and made to correspond exactly with the perchloride of formyle, C_2HCl_3 , in direct contradiction to the principles involved in its synthetical production, upon which Berthelot in many cases lays so much emphasis. Our author's views of the probable constitution of the mixed alcohol radicals are strange; methylethyle, C_2H_3 , C_4H_5 , becoming with him, ethylformène with the formula C_2H_4 , C_4H_6 . It is not easy to understand the true relations of glycerine and its numerous derivatives—the history of which has been, for the most part, traced by Berthelot himself,—from the reactions and formulæ given in the present work. He accepts the view propounded by other chemists, that glycerine is a triatomic alcohol, but makes no attempt to show how the constitution of the various compounds which can be obtained from this interesting substance may be illustrated by the development of this view.¹

CORRESPONDENCE.

Preparation of Chrome Yellow.

To the Editor of the CHEMICAL NEWS.

SIR,—In reply to a correspondent whose letter appears in No. 55 of the CHEMICAL NEWS, the orange colour of his precipitate is owing to the presence of a subchromate of lead. The colour of the chromates of lead varies from a bright red to a pale yellow,—all the intermediate colours may be produced by varying the mode of preparation.

To prepare the red chromate of lead, precipitate tribasic acetate of lead with monochromate of potassa (the tribasic acetate may be prepared by boiling ordinary sugar of lead with litharge). To prepare the yellow chromate of lead, precipitate the monobasic acetate of lead with bichromate of potassa.

¹ See CHEMICAL NEWS, vol. II., p. 141.

The ordinary sugar of lead generally contains an excess of base, which gives the precipitated chromate of lead an orange colour. This may be corrected by adding sufficient acetic acid to give the solution of acetate of lead a faint but distinct acid reaction before adding the bichromate of potassa.

I may, however, remark, that the use of chromate of lead as a pigment is scarcely more advisable than the use of arsenite of copper, it being almost, if not quite, as poisonous.—I am, &c. JAS. HARGREAVES.

Dale's Patent for the Preparation of a Colouring Matter.

To the Editor of the CHEMICAL NEWS.

SIR,—Nothing can surprise a man much more than to find a process with which he has been for years familiar put forth as a new invention and actually patented. You may judge, therefore, what were our feelings on reading, in No. 54 of the CHEMICAL NEWS, a notice of a patent taken out by Mr. J. Dale for the "Preparation of a Colouring Matter." It so happens that our Mr. Robinson applied this very process—extracting the colouring-matter of all the known dye-woods with alkaline solutions, and afterwards neutralising with an acid—no less than six years ago! Since that time we have been in the habit of making more or less of these preparations. If asked why did we not anticipate Mr. Dale with a patent, the answer is very simple. The principle, even in our hands, was not novel! The makers of dye-wood extracts have long been in the habit of using, in certain cases, weak alkaline solutions to draw out the last traces of colouring matter, and of subsequently neutralising with an acid, or an acid salt.

We are surprised that Mr. Dale should be ignorant of a fact thus known to the trade; but we are still more unwilling to believe that he could be aware of the custom and yet make a declaration claiming the principle as his own invention. For obvious reasons we must decline pronouncing whether the mechanical arrangements, proportions, &c., specified by Mr. Dale are the best in existence. Commercially we have no interest in the matter, as we have come upon a totally distinct principle for effecting the same object in a superior manner.

We are sorry to add that this is not the only case of a patent being taken out for some well-known process. Is every person free to claim as a novelty, and to monopolise by means of a patent, any process which he does not find described in common works of reference? Are manufacturing chemists, dyers, and printers to be debarred from methods which they—and perhaps their fathers before them—have for years pursued, or to be driven into continual litigation to maintain their rights?

Does not the law as regards chemical patents sadly need improvement?—We are, &c.

ROBINSON AND SLATER.

Moorhead Chemical Works, Huddersfield.

The Homogenesis of Forces.

To the Editor of the CHEMICAL NEWS.

SIR,—Perhaps I may be late in asking Mr. Rodwell a question through the medium of your Journal. Does steel become magnetic when placed in a particular portion of the solar spectrum? Are we justified, too, in classing motion among the forces? The difference between matter in motion and at rest has engaged the attention of many an able geometer and metaphysician without eliciting anything satisfactory. Is it not more a condition of matter than a force?

But in making gravitation, the primary force, productive of physical phenomena, Mr. Rodwell seems to ignore the equally inherent attractive forces of cohesion, or the repulsive energy exhibited by all matter at insensible

distances. And what also are we to say about the operations of the centrifugal force? Until he can experimentally prove the effect which gravitation would produce when unaffected by these counterbalancing causes, I think we must hesitate to accept the proposed solution. When in such a property of bodies as elasticity we discover palpable operations of a repulsive force, we cannot yield to gravity an undivided sway over natural phenomena and physical laws.

I fear the theory is as the mouse at the tenth hole.—I am, &c. R. M. T.

Detection of Bisulphide of Carbon in Coal-Gas.

To the Editor of the CHEMICAL NEWS.

SIR,—I find that by forcing by an ordinary blow-pipe the flame of coal-gas for about a minute, on to distilled water containing a little acid chloride of barium, sulphate of baryta is formed, and the presence of sulphur in the gas thus proved.—I am, &c. J. T. B.

Chemical Notices from Foreign Sources.

1. MINERAL CHEMISTRY.

Extraction of Iodine from Peruvian Nitrate of Soda.—M. Leon Krafft has found that the mother liquors obtained in the purification of nitrate of soda by recrystallisation contain considerable quantities of iodine (*Le Moniteur Scientifique*, t. ii. liv. 95, p. 1070). The method he has adopted for separating it is by adding protochloride of copper which precipitates the whole of the iodine. M. Krafft has also found it in the mother liquor of saltpetre and in the chloride of potassium of commerce, but not in sufficient amount to pay for the extraction.

A Crystalline Alloy of Tin and Iron.—Noellner has noticed (*Annalen der Chemie und Pharmacie* bd. cxv. s. 233) that the deposit formed when salt of tin is prepared with commercial hydrochloric acid contains microscopic needles, soluble in aqua regia, which have a composition agreeing with the formula Sn_2Fe . The author has found that the compound pre-exists in some tins. He has observed also that a solution of salt of tin prepared without heating the mixture of granulated tin and hydrochloric acid contains twice the quantity of acid necessary for the formation of the salt, but the excess is saturated when the mixture is heated. There is, however, a loss of hydrochloric acid.

Desulphuration of Native Sulphides.—MM. Bronac and Deherrypon (*Polytech. Journal*, t. clvii. p. 342) effect the desulphuration and dephosphuration of ores by the use of what is called the *iron sponge*. This iron sponge is the invention of M. Chenot, and is made by reducing dry ores of iron by means of carbonic oxide. The native sulphides are reduced to powder and then mixed with a proper proportion of the iron sponge. The mixture is then placed in a reverberatory furnace and covered with a layer of charcoal powder to prevent the oxidation of the iron. The process is said to be particularly applicable to the treatment of galena and sulphide of antimony, inasmuch as it does away with the necessity for a preliminary roasting of the ore, and the reduction is effected at a lower temperature. The cost is said to be considerably lower than the usual method.

Action of Sulphuretted Hydrogen on Neutral Salts of Zinc.—Wernike has noticed (*Annalen der Physik und Chem.* bd. cx. p. 655) that when a solution of sulphate of zinc which has been treated with sulphuretted hydrogen as long as a precipitate is formed is filtered and set aside; it after a time becomes turbid and deposits plates, which, however, he has not analysed.

Oxide of Zinc for Polishing Glass.—M. Pohl (*Ibid.* p. 201) recommends oxide of zinc for polishing glass intended for optical instruments. The author states that it answers perfectly, and is quite free from the objections brought against the use of the oxides of iron and tin.

II. ORGANIC CHEMISTRY.

New Compounds of Sugar with Acids.—M. Berthelot, in a memoir to be found in the *Annales de Chimie et de Physique*, t. lx, p. 93, describes some compounds of glucose with stearic, butyric, acetic, and benzoic acids:—

I. **Stearic Glucose.** obtained by heating a mixture of stearic acid and dehydrated glucose to 120° for about sixty hours. It is a neutral, solid, colourless body, like stearine in appearance, fusible, soluble in ether and absolute alcohol, but not in water. Its formula is $C_{28}H_{48}O_{14}$.

II. **Butyric Glucose.**—Obtained by a process similar to the above. It is a neutral oily liquid, slightly soluble in water, and soluble in ether and spirit. It has a very bitter taste. Formula, $C_{22}H_{40}O_{11}$.

III. **Acetic Glucose.**—Prepared as above; is a neutral, oily, colourless liquid, soluble in alcohol, ether, and water, and having a very bitter taste.

IV. **Benzoic Glucose.**—Obtained like the above. A thick, neutral, bitter liquid, soluble in ether and alcohol, and slightly so in water.

V. **Ethyl Glucose.**—Prepared by heating a mixture of cane-sugar, hydrobromic ether and potash to 100° for several days. The product is shaken up with ether, and the ethereal solution is evaporated *in vacuo*. It is a fixed oil, insoluble in water, bitter, and having an agreeable odour. Formula, $C_{20}H_{38}O_{10}$.

The author remarks that the sugars do not combine with acids only. Like polyatomic alcohols they may combine with other alcohols; and further may unite at once with alcohols and acids.

New Cinchona Alkaloid.—Schwabe (*Archiv. der Pharmacie*, bd. cii. s. 273) has extracted from quinoidine a new alkaloid which he calls β -cinchonine. To obtain it quinoidine is dissolved in dilute hydrochloric acid, then precipitated with ammonia; the precipitate is washed first with cold, then with hot water; afterwards dried and subsequently treated with cold alcohol, which in about twenty-four hours gives a brown tincture. The residue is exhausted successively with alcohol and then with water, and afterwards dissolved in dilute sulphuric acid. This solution is warmed, and, while hot, a solution of carbonate of soda is added until a crystalline pellicle forms. It is then set aside to cool, whereupon crystals of the sulphate of β -cinchonine are deposited. These are a little coloured, but may be purified in the usual way. By dissolving the sulphate in acidulated water, precipitating by ammonia, and crystallising from alcohol, the pure alkaloid is obtained in right rhombic prisms.

Beta-cinchonine $C_{20}H_{27}NO$ is anhydrous, and fuses at 150° C.; on cooling it becomes a radiated mass; it is not volatile, and burns with a smoky flame. It is easily distinguished from the other cinchona alkaloids by its crystalline form, chemical properties, and particularly by its solubility.

The author gives the following table of the solubility of the cinchona alkaloid in different menstrua:—

	α Quinine.	β Quinine.	α Cinchonine.	β Cinchonine.
Cold water . . .	400 parts	15,000	insoluble	insoluble.
Boiling water . .	200 "	750	2500	slightly sol.
Cold alcohol . . .	2 "	45		773
Boiling alcohol . .	2 "	37	30	43
Ether	60 "	90	insoluble	174
Chloroform . . .	6 "		40	268

Fatty oils dissolve beta-cinchonine. The alcoholic solution turns the plane of polarisation to the right. The

following are the chemical reactions of salts of the new alkaloid:—Chloride of gold gives a sulphur yellow precipitate; bichloride of mercury gives a white precipitate, which soon becomes resinous; bichloride of platinum an orange yellow precipitate, soluble in much water; iodide of potassium a white precipitate; iodated water a red brown; bromated alcohol a yellow precipitate; picric and phospho-molybdic acids a yellow precipitate. With chlorine and ammonia the solution becomes yellow. Gallic acid gives a yellowish white precipitate; and prussiate of potash gives a rose coloration, as with the solutions of quinine, cinchonine, and quinoidine. The author has not succeeded in obtaining neutral salts with beta-cinchonine; all are basic. The sulphate crystallises in prisms with a rhombic base; it is insoluble in ether, but soluble in alcohol and water. The hydrochlorate crystallises with 4 equivalents of water in prisms which are slightly soluble in ether. With bichloride of mercury they form an amorphous double salt. With bichloride of platinum they form six-sided rhomboidal prisms. Iodine gives with the sulphate a salt resembling the compound obtained with iodine and sulphate of quinine. It is made by dissolving (at 30° C.) 10 parts of the sulphate in a mixture of 144 parts of acetic, and 11 parts of dilute sulphuric acid, to which is added 3 parts of iodine dissolved in 115 parts of alcohol. On cooling, plates without definite form separate from the solution.

MISCELLANEOUS.

THE SPECIMEN EXCHANGE CLUB.

For some time past we have received letters on the subject of exchanging specimens of chemical substances, some merely approving the idea, others containing suggestions for carrying it out in practice. Of the latter we have published several, but in doing so we have not desired it to be understood that we have in all cases approved the suggestions offered. One writer, for example, proposes that the sender of a specimen or specimens should communicate the best method of preparing them. Such information would, no doubt, be very useful; but is, we fear, impracticable, because no man whose time is valuable would take the trouble to write three or four or more descriptions, according to the number of specimens; not to speak of other reasons he might have for declining to furnish it; this, therefore, we cannot at present incorporate in the Regulations. Another suggests that chemists not having specimens to exchange should be enabled to purchase. There are many objections to this which we need not enumerate; it would, moreover, be impossible for us to spare the time which would be required for regulating money transactions. This difficulty might possibly be overcome at a future time, but we do not think it advisable to encounter it at present. As it is evident that the fewer the Rules the better, we now propose the following, and recommend that each member shall adhere strictly to them, until experience has shown whether any addition or alteration can be made with advantage.

Rule 1. To prevent dissatisfaction on the score of relative value of specimens, each specimen should be in such a quantity as the sender may fairly consider of the value of two shillings and sixpence. It will be evident that some such standard should be adopted, or an exchange of specimens of different values would be impracticable.

Rule 2. The specimen to be securely enclosed, if liquid, in a stoppered bottle; or, if solid, in a glass tube, distinctly and properly labelled, with full particulars as to formulae, &c.

Rule 3. Specimens of the rarer minerals may be sent, provided they are of the estimated value stated in Rule 1, packed in pasteboard boxes, and having the name legibly written on the cover.

Rule . As the amount required for postage of each specimen will be published in the list, alongside the article, every applicant must send the requisite number of stamps for the purpose.

Rule 5. Each person requesting to have his name inserted in the list will be required to send, at the same time, to the Editor, a properly labelled specimen of the substance he desires to exchange, having his own name and address written on the cover. This will be kept for comparison, in the event of any complaint being made by a member of the Club, at a future time, with respect to the quality of the substance sent; and no person's name will be inserted without compliance with this rule.

Let us now state what we are willing to do to further the wishes of our subscribers.

We will, at certain stated intervals, publish in the News a list of specimens which members may desire to exchange for others. The member's initials, or other distinctive sign will be affixed to each, together with the number of stamps required to carry it through the post. In future Numbers we will, when necessary, intimate to a member,—Mr. Smith, for instance,—to whose initials are appended the words "*Oxide of Lanthanum*, 2d.," that Mr. Jones, "*Oxide of Yttrium*, 1d.," desires to exchange yttria for lanthania, and has accordingly sent a specimen oxide of yttrium, with a 2d. postage-stamp, to our office. If Mr. Smith does not care about the exchange, he simply take no notice of it; whereas, if he desires to meet Mr. Jones's wishes in the matter, he will forward a specimen of his oxide of lanthanum, with a 1d. stamp, to our office, when we will forward the specimens to the respective addresses.

We are willing to receive the specimens sent for exchange, and to effect the exchanges in accordance with the wishes of the senders, so far as this may be practicable. It may occasionally happen that there will be a run on some particular substance, but a judicious system of exchanging and re-exchanging will, we hope, satisfy the desire of all persons. So far as regards the trouble attending this exchange, the posting of the specimens, &c., we undertake it cheerfully, only asking that, if any person considers he has cause to be dissatisfied with the arrangement, he will not visit his displeasure on us. That a hitch will now and then occur is probable, especially at the commencement, but so long as every individual acts in perfect good faith, the Specimen Exchange Club cannot fail to be beneficial to the members who compose it.

Iodide of Starch.—At the last sitting of the French Academy of Sciences, M. Duroy announced the discovery of a new neutral and colourless iodide of starch. It is well known that iodine gives a blue colour to starch, thus forming an iodide. M. Duroy brings this iodide into contact with yeast, and thus deprives it of its colour. In this state it is very soluble in water, insoluble in alcohol, sweet, gummy, and incapable of crystallisation.

The Act for Preventing the Adulteration of Articles of Food and Drink.—The various Boards of Works are busily considering the best means of securing to the public the advantages of this important measure. Public Analysts will be appointed forthwith in every locality; and no doubt properly-qualified persons will be sought out with care for this highly responsible duty. We are glad to see already, that the selection will be made in many cases in strict accordance with the rule of appointing not the local self-seeker, but the most approved candidate.

NOTICE OF REMOVAL.

Subscribers and the Trade are respectfully informed that the OFFICE of this Paper has been REMOVED to Messrs. Griffin, Bohn, and Co., 10, Stationers' Hall Court, E.C.

ANSWERS TO CORRESPONDENTS.

"* In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

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"* All Editorial Communications are to be addressed to Mr. CROOKES and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BOHN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

J. Sharples.—We are much obliged by the information. We have, however, received a previous communication on the same subject.

J. McKennie.—The book was forwarded by book-post last week.

H. Sherman.—A full answer to your question as to the best plan to produce from 1 to 7 tons per week of sulphuric acid at cheapest cost from pyrites and sulphur could not be given in the space at our disposal in this column. We will endeavour to find the best description of what you require, and will refer you to it in this column in a subsequent number.

Parchment-paper, E. Herring.—You must have had the acid too dilute. When properly prepared and well washed, parchment-paper, forms an admirable covering for jars instead of bladder, being very strong and tough when dry.

J. S.—You must file off some of the outer surface and then test the under part to see if the article is electro-plated. The deposit on the surface being pure silver would of course give the reactions of silver.

Soap Making.—Alpha desires to know if there is any monograph on the science and art of soap making.

Anhydrous Chloride of Tin.—Mix dry and warm stannic sulphate with an equal weight of dry, warm common salt, both in fine powder, distil in an iron retort and rectify the product in a glass retort with the addition of about three times its weight of oil of vitriol.

Reduction of Cryolite in Clay Crucibles.—F. W. G.—Under ordinary circumstances the silica of the crucibles will be attacked, but this can be avoided by following Wöhler's method, as follows:—Finely triturate and well dry the cryolite and mix it with an equal weight of a mixture of 7 parts of chloride of sodium and 9 parts of chloride of potassium previously fused together and finely powdered. Place the mixture into the crucible in alternate layers with discs of sodium strongly compressing the separate layers. To 50 parts of the mixture 5 to 10 parts of sodium are employed. The crucible must be previously well dried, and the whole is to be put into a hot wind furnace with a good draught, surrounded with hot coals and brought quickly to a full red-heat. The mass is readily fusible without eating through the crucible and the aluminium obtained is free from silicium, and will be about 4 per cent. of the weight of cryolite taken.

Books Received.—"The Successful Application of Charcoal Air-filters to the Ventilation and Disinfection of Sewers," a Letter to the Right Honourable the Lord Mayor, W. Cubitt, M.P., by Dr. John Stenhouse, F.R.S. London: John Churchill. "A Veterinary Toxicological Chart," by W. J. T. Morton, late Professor of Chemistry and Materia Medica, at the Royal Veterinary College. London: Longmans. "Report of Experiments on the Growth of Red Clover by different Manures," by J. B. Lawes, F.R.S., and Dr. J. H. Gilbert, F.R.S. London: W. Clowes and Sons. "On the Sources of the Nitrogen of Vegetation," by J. B. Lawes, F.R.S., Dr. J. H. Gilbert, F.R.S., and E. Pugh, Ph.D.

ERRATUM in No. 55.—P. 334, col. 2, lines 13 and 14 from top, for "1 ounce" read "1 drachm" in both cases.

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THE CHEMICAL NEWS

VOL. III. No. 58.—January 12, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Solubility of the Chloride, Bromide, and Iodide of Silver in Certain Solutions, by FREDERICK FIELD.

In continuing some experiments upon the separation of chlorine, bromine, and iodine, the solvent action of solutions of certain salts upon those elements, when in combination with silver, was tried. In a recent paper upon the subject, it was shown that a comparative large excess of iodide or bromide of potassium—provided the solution be dilute—has little or no action upon the iodide or bromide of silver. This fact has since been more fully confirmed by the following experiments:—

A solution of iodine of potassium was made by dissolving 10 grains of the salt in 6 ounces of water, and nitrate of silver carefully added. The solution of the latter salt was prepared by dissolving 1 grain in 1000 grains of water. The addition of 5 grains of this solution, containing $\frac{1}{100}$, or 0.005 grains of nitrate of silver, or 0.0031 of the metal, produced a permanent cloudiness in the liquid. 10 grains, containing $\frac{1}{50}$ of a grain of nitrate, or 0.0062 of metal, gave, as might be expected, a greater turbidity, and the liquid could be filtered clear after standing for some hours. 10 grains of bromide of potassium were next dissolved in a similar quantity of water, and the same amount of nitrate of silver added. A slight turbidity was occasioned; not quite so much, perhaps, as with the iodide, but still sufficiently apparent. 10 grains of chloride of sodium, under similar circumstances, gave a slight turbidity, which did not altogether disappear after several hours' standing.

It appears from these experiments that very weak solutions of the iodide, bromide, and chloride of the alkaline metals have little or no action upon their respective silver salts, although, when concentrated, the solvent action—especially of the iodide—is considerable.

Gmelin tells us, under the head of "Iodide of Silver" (see Gmelin's "Handbook," vol. vi.),—upon whose authority we are not informed,—that the chlorides of sodium and potassium dissolve iodide of silver abundantly. As this is in perfect contradiction to the results of my experiments in 1857, some of which are published in the *Quarterly Journal of the Chemical Society*, further researches were entered into. 0.005 of a grain of nitrate of silver were added to 4 ounces of a saturated solution of chloride of sodium, containing about 0.10 grain of iodide of potassium. As the chlorine cannot combine with the metal as long as any iodine, in the form of soluble iodide, exists, iodide of silver must be formed. 0.005 of a grain of the nitrate is equal to 0.0031 of silver, and this is converted into 0.0067 iodide of silver. This small quantity was immediately precipitated, showing the great insolubility of iodide of silver in an immense excess of cold solution of

chloride of sodium. A few grains of iodide of silver were digested with 8 ounces saturated solution of chloride of sodium for some hours. No trace of silver could be found in the filtrate. Even boiling solution of common salt only dissolves traces of the iodide, and this is re-precipitated on cooling.

A similar experiment was tried with a concentrated solution of chloride of potassium, with precisely the same results.

The action of hyposulphite of soda upon this class of silver compounds is interesting. It is generally supposed that the chloride, bromide, and iodide of silver are very soluble in a solution of this salt; but this is not strictly true with regard to the two latter, especially if they are suspended in a considerable quantity of water; and it is worthy of remark that iodide of potassium precipitates iodide of silver from its solution in hyposulphite of soda, and bromide of potassium exercises a similar action upon bromide of silver when dissolved in a solution of the same salt. No excess, however, of chloride of sodium precipitates chloride of silver when once dissolved in hyposulphite of soda.

10.00 grains of nitrate of silver were dissolved in water, and precipitated by the addition of chloride of sodium in slight excess. The chloride of silver, after washing, was suspended in 4 ounces of distilled water, placed in an eight-ounce glass-stoppered bottle.

10.00 of nitrate of silver were treated in an exactly similar manner, bromide of potassium being substituted for chloride of sodium.

10.00 of nitrate of silver were precipitated by a solution of iodide of potassium, the iodide of silver being subsequently suspended in the same quantity of water as the chloride and bromide.

A highly-concentrated solution of hyposulphite of soda was prepared and poured into a graduated burette. 200 grains by measure of this liquid added to the 8 ounces of water in which the chloride of silver was suspended, entirely dissolved that compound, the liquid becoming perfectly clear. The bromide of silver required more than 800 grains from the burette to effect its complete solution, while the iodide did not entirely dissolve until after the addition of 1750 grains. To the solution of the chloride of silver in the hyposulphite of soda from 10 to 12 grains of the iodide of potassium were added, when an immediate precipitate of iodide of silver took place, which, after a few hours' digestion, was filtered off, washed, and weighed. Nearly all the silver was found to have been removed from solution by the introduction of the iodide of potassium, as the iodide of silver weighed 13.78, within 0.04 of the calculated amount, supposing the whole to have been precipitated. The filtrate afforded a brownish colour on the addition of sulphuretted hydrogen, which, after prolonged digestion in a warm situation, deposited black flakes of the metallic sulphide. As might be expected, iodide of potassium threw down by far the greater portion of the silver from the bromine compound in the

alkaline hyposulphite, but not so completely as in the former case, owing, doubtless, to the larger quantity of hyposulphite of soda present, which had been added for the entire solution of the bromide of silver. Care was taken in each of the above experiments to add no great excess of iodide of potassium, but only just sufficient to decompose the metallic chloride and bromide, as it was found, as formerly stated, that an excess of iodide of potassium precipitates iodide of silver from its solution in hyposulphite of soda. 10.00 of iodide of potassium were added to the liquid in the bottle containing the iodide of silver in the hyposulphite, when an immediate precipitate took place. This was filtered off and found to weigh 5.34 grains, or considerably more than a third of the whole quantity of iodide of silver. The addition of a further quantity of iodide of potassium to the filtrate threw down more iodide of silver in small but distinct yellow crystals. These crystals are dissolved on heating the supernatant liquid, and deposited more beautifully as the solution cools.¹ This is not the first time iodide of silver has been obtained in a crystalline form. I mentioned² that by boiling red iodide of mercury with slight excess of nitrate of silver, the nitrate of mercury formed by decomposition dissolves the iodide of silver, which crystallises from the filtrate in brilliant crystalline plates.

There can be little doubt that iodide of silver is very insoluble in dilute hyposulphite of soda. It is only necessary to dissolve a small quantity of chloride of silver in a weak solution of this salt, and a drop of iodide of potassium in water will convince us of the fact. The same is the case, although to a far less extent, with bromide of silver. A solution of bromide of potassium precipitates bromide of silver from the solution of the chloride in the hyposulphite, and bromide of silver is also precipitated, but not in such abundance as the iodide under parallel circumstances, by the addition of bromide of potassium to its solution in hyposulphite of soda. Many years ago, when operating upon large quantities of silver mineral with a solution of hyposulphite of lime, I observed the comparative insolubility of iodide of silver in this menstruum. In order to be satisfied that the liquid which percolated through the ore still held the chloride of the precious metal in solution, a drop of iodide of potassium was added, which caused an immediate turbidity if such were the case. To refer for one moment to the former part of this paper, it will be seen that the separation of chlorine, bromine, and iodine can be effected with greater accuracy by the cautious employment of weak solutions of the iodide and bromide of potassium. Iodide of silver is very soluble in a concentrated solution of the potassium salt, but if the reaction be studied further, all cause for apprehension is lost. Add a few drops of strong nitrate of silver to a highly concentrated solution of iodide of potassium in a test tube; as each drop enters the liquid a bulky precipitate takes place, which immediately redissolves upon agitation. A considerable quantity can be thus added, without producing a permanent precipitate; but if the contents of the tube be thrown into 4 or 6 ounces of cold water, all the silver salt will be thrown down, and the filtrate will exhibit no blackening

when tested with sulphide of ammonium. The preceding observations may be interesting, as illustrating still more fully the curious relationship of iodine, bromine, and chlorine, bromine appearing to be the connecting link between the other two elements. Independently of chlorine being a gas, iodine a solid, and bromine a liquid, and that the equivalents of the first and the last divided by two give the equivalent of bromine, the phenomena attendant upon their reactions may be traced still further.

Chloride of silver is white, bromide of silver very pale yellow, iodide of silver bright pale yellow; chloride of silver highly soluble in ammonia, bromide of silver difficultly soluble, iodide of silver nearly insoluble. Chloride of silver exceedingly soluble in hyposulphite of soda, bromide of silver far less so; iodide of silver twice as insoluble as the bromide. Chloride of silver not precipitated by the addition of chloride of sodium to its solution in the hyposulphite; bromide of silver to a certain extent; iodide of silver in very large quantity. Chloride of silver slightly soluble in cold concentrated chloride of potassium, bromide of silver scarcely soluble, iodide of silver perfectly insoluble; and, to conclude, iodide of silver soluble in concentrated alkaline iodides, bromide of silver to a less extent in bromides, and chloride of silver to still less extent in chlorides.

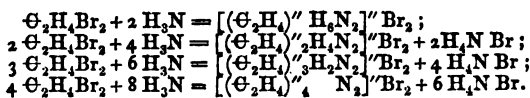
Instances, no doubt, might be multiplied in connection with the peculiarities exhibited by this interesting triad. The chloride and iodide of silver existing in the same mine, and almost in juxtaposition with the bromide, seems to be highly curious and suggestive.

[I have obtained from the mine "Delirio," at the foot of the hill of Chanarcillo, near Copiapo, in Chili, pure chloride of silver, iodide of silver, and bromide of silver, the latter in splendid crystals imbedded in carbonate of lime, through which a small vein of pure silver was running.—F. F.]

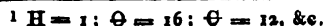
On the Polyatomic Derivatives of Ammonia, by A. W. HOFMANN, Ph.D., F.R.S.

THE author has been engaged for a considerable time in the investigation of the action of dibromide of ethylene upon ammonia; but, in consequence of the complexity of the reaction, and the difficulty of separating the various products, he has been induced to study the deportment of the dibromide with several of the derivatives of ammonia, which, owing to the smaller number of compounds formed, furnish more readily-accessible results.

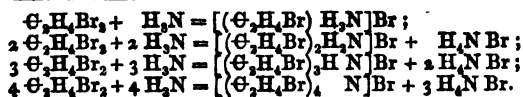
The action of dibromide of ethylene on ammonia may give rise to no less than four series of salts, according to the different proportions of the two compounds which take part in the reaction. When two equivalents of ammonia and one of dibromide of ethylene act upon one another, the following changes may occur, producing four diatomic salts:—



If, on the other hand, the reaction take place between equal equivalents of ammonia and dibromide of ethylene, another series of salts is produced which is derived from one molecule of ammoniacal salt:—

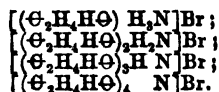


¹ In an early Number of the CHEMICAL NEWS there is an interesting note, to which the initials "C. L. B." are attached, concerning the purple colour observable in the liquid after the passage of sulphuretted hydrogen through a solution of perchloride of iron. When iodide of silver is dissolved in warm hyposulphite of soda the solution has a similar colour, appearing clear by transmitted, but slightly turbid by reflected light.—F. F.

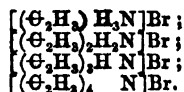


This series may be considered as derived from bromide of ammonium by the substitution for one, two, three, or four equivalents of hydrogen of one, two, three, or four equivalents of the radical $\text{C}_2\text{H}_4\text{Br}$, which may be called bromethyl.

When this series is produced in the presence of water, the bromine within the radical may be further replaced by the residue $\text{H}\Theta$, hydrobromic acid being simultaneously eliminated, a third series of salts—viz., that of the oxethylated compounds—being thus formed, which contain



Lastly, the bromethylated salts may be converted into vinyl-compounds by the simple loss of hydrobromic acid giving rise to the production of four additional bodies, containing

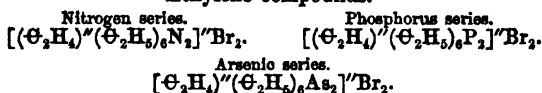


We thus see that no less than sixteen salts are, or may be, produced by the action of ammonia on dibromide of ethylene, and it is next to impossible to separate them, especially if the almost endless number of compounds intermediate between the principal classes be taken into consideration. But if the ammonia be replaced by monamines, it is obvious that in the same measure as the degree of substitution of the hydrogen in the ammonia advances, a smaller number of compounds will be obtained by the action of dibromide of ethylene; thus, a primary monamine could not give rise to the production of more than twelve salts; a secondary monamine, of not more than eight; and, lastly, a tertiary, of not more than four compounds.

By acting on dibromide of ethylene with triethylamine, triethylphosphine, and triethylarsine, Dr. Hofmann has, in fact, succeeded in obtaining in each series the four salts which are indicated by theory.

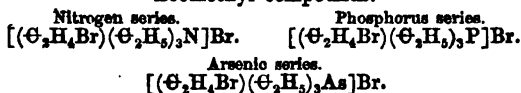
Diatomic Salts.

Ethylene-compounds.

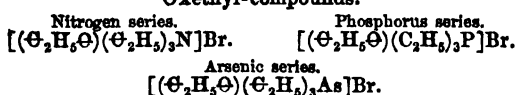


Monatomic Salts.

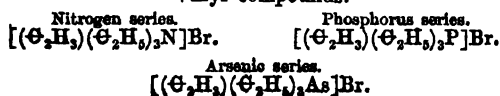
Bromethyl-compounds.



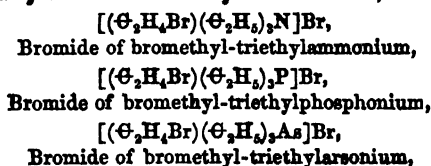
Oxethyl-compounds.



Vinyl-compounds.

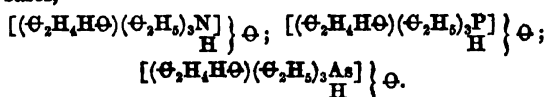


In all these reactions the principal products are invariably the three bromethylated bromides,



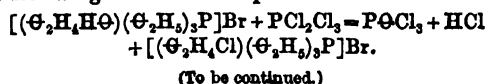
which give rise to a considerable number of interesting products.

When solutions of these compounds are mixed with silver-salts, only one-half of the bromine is precipitated in the form of bromide of silver, a salt of the bromethylated metal remaining in solution. When treated, however, with recently-precipitated oxide of silver, the whole of the bromine is eliminated and replaced by the residue $\text{H}\Theta$, and the solutions contain the oxethylated bases,



These bases, when acted upon by acids, form three classes of salts, which may be regarded as ammonium-, phosphonium-, and arsonium-compounds, in which three equivalents of hydrogen are replaced by ethyl and one by the radical oxethyl, $\text{C}_2\text{H}_4\text{H}\Theta = \text{C}_2\text{H}_5\Theta$.

Exactly as hydrates in general may be converted into bromides and chlorides by the action of pentabromide and pentachloride of phosphorus, in the same manner the oxethylated salts may be transformed into the corresponding bromethylated and chlorethylated bodies. Thus, on submitting bromide of bromethyltriethylphosphonium to the action of pentachloride of phosphorus, the following reaction takes place:—



(To be continued.)

Note on Monohydrated Sulphuric Acid, by Dr. LYON PLAYFAIR.

At the meeting of the Royal Society of Edinburgh, on the 7th instant, Dr. Playfair drew attention to the researches of Marignac on sulphuric acid. This chemist always found too much water in monohydrated sulphuric acid, and fixed its specific gravity, as Beneau and others have recently done, from 1.842 to 1.845.

It is well known, indeed, that this hydrate loses anhydrous acid when distilled or boiled, and the object of the present communication is to ascertain the exact conditions under which this loss takes place, as this knowledge is of importance in a practical point of view.

The author occasionally found on distilling, and afterwards heating, oil of vitriol, that acid of the specific gravity of 1.848 was obtained, but at other times the specific gravity was as low as 1.842. To explain this difference, the following experiments were made:—

1. Sulphuric acid, having a specific gravity of 1.848, and a per-centage of anhydride of 81.62 by the alkali-meter, was put in a retort, burned in hot sand, and distilled. The distillate had a specific gravity of 1.840, and a per-centage strength of 80.12. It had, therefore, lost by distillation 1½ per cent. of anhydride.

alkaline hyposulphite, but not so completely as in the former case, owing, doubtless, to the larger quantity of hyposulphite of soda present, which had been added for the entire solution of the bromide of silver. Care was taken in each of the above experiments to add no great excess of iodide of potassium, but only just sufficient to decompose the metallic chloride and bromide, as it was found, as formerly stated, that an excess of iodide of potassium precipitates iodide of silver from its solution in hyposulphite of soda. 10.00 of iodide of potassium were added to the liquid in the bottle containing the iodide of silver in the hyposulphite, when an immediate precipitate took place. This was filtered off and found to weigh 5.34 grains, or considerably more than a third of the whole quantity of iodide of silver. The addition of a further quantity of iodide of potassium to the filtrate threw down more iodide of silver in small but distinct yellow crystals. These crystals are dissolved on heating the supernatant liquid, and deposited more beautifully as the solution cools.¹ This is not the first time iodide of silver has been obtained in a crystalline form. I mentioned² that by boiling red iodide of mercury with slight excess of nitrate of silver, the nitrate of mercury formed by decomposition dissolves the iodide of silver, which crystallises from the filtrate in brilliant crystalline plates.

There can be little doubt that iodide of silver is very insoluble in dilute hyposulphite of soda. It is only necessary to dissolve a small quantity of chloride of silver in a weak solution of this salt, and a drop of iodide of potassium in water will convince us of the fact. The same is the case, although to a far less extent, with bromide of silver. A solution of bromide of potassium precipitates bromide of silver from the solution of the chloride in the hyposulphite, and bromide of silver is also precipitated, but not in such abundance as the iodide under parallel circumstances, by the addition of bromide of potassium to its solution in hyposulphite of soda. Many years ago, when operating upon large quantities of silver mineral with a solution of hyposulphite of lime, I observed the comparative insolubility of iodide of silver in this menstruum. In order to be satisfied that the liquid which percolated through the ore still held the chloride of the precious metal in solution, a drop of iodide of potassium was added, which caused an immediate turbidity if such were the case. To refer for one moment to the former part of this paper, it will be seen that the separation of chlorine, bromine, and iodine can be effected with greater accuracy by the cautious employment of weak solutions of the iodide and bromide of potassium. Iodide of silver is very soluble in a concentrated solution of the potassium salt, but if the reaction be studied further, all cause for apprehension is lost. Add a few drops of strong nitrate of silver to a highly concentrated solution of iodide of potassium in a test tube; as each drop enters the liquid a bulky precipitate takes place, which immediately redissolves upon agitation. A considerable quantity can be thus added, without producing a permanent precipitate; but if the contents of the tube be thrown into 4 or 6 ounces of cold water, all the silver salt will be thrown down, and the filtrate will exhibit no blackening

when tested with sulphide of ammonium. The preceding observations may be interesting, as illustrating still more fully the curious relationship of iodine, bromine, and chlorine, bromine appearing to be the connecting link between the other two elements. Independently of chlorine being a gas, iodine a solid, and bromine a liquid, and that the equivalents of the first and the last divided by two give the equivalent of bromine, the phenomena attendant upon their reactions may be traced still further.

Chloride of silver is white, bromide of silver very pale yellow, iodide of silver bright pale yellow; chloride of silver highly soluble in ammonia, bromide of silver difficultly soluble, iodide of silver nearly insoluble. Chloride of silver exceedingly soluble in hyposulphite of soda, bromide of silver far less so; iodide of silver twice as insoluble as the bromide. Chloride of silver not precipitated by the addition of chloride of sodium to its solution in the hyposulphite; bromide of silver to a certain extent; iodide of silver in very large quantity. Chloride of silver slightly soluble in cold concentrated chloride of potassium, bromide of silver scarcely soluble, iodide of silver perfectly insoluble; and, to conclude, iodide of silver soluble in concentrated alkaline iodides, bromide of silver to a less extent in bromides, and chloride of silver to still less extent in chlorides.

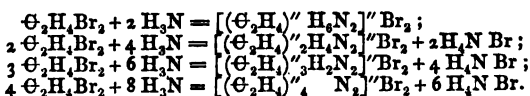
Instances, no doubt, might be multiplied in connection with the peculiarities exhibited by this interesting triad. The chloride and iodide of silver existing in the same mine, and almost in juxtaposition with the bromide, seems to be highly curious and suggestive.

[I have obtained from the mine "Delirio," at the foot of the hill of Chanarcillo, near Copiapo, in Chili, pure chloride of silver, iodide of silver, and bromide of silver, the latter in splendid crystals imbedded in carbonate of lime, through which a small vein of pure silver was running.—F. F.]

On the Polyatomic Derivatives of Ammonia, by A. W. HOFMANN, Ph.D., F.R.S.

THE author has been engaged for a considerable time in the investigation of the action of dibromide of ethylene upon ammonia; but, in consequence of the complexity of the reaction, and the difficulty of separating the various products, he has been induced to study the deportment of the dibromide with several of the derivatives of ammonia, which, owing to the smaller number of compounds formed, furnish more readily-accessible results.

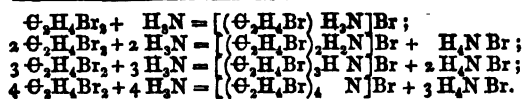
The action of dibromide of ethylene on ammonia may give rise to no less than four series of salts, according to the different proportions of the two compounds which take part in the reaction. When two equivalents of ammonia and one of dibromide of ethylene act upon one another, the following changes may occur, producing four diatomic salts:¹—



If, on the other hand, the reaction take place between equal equivalents of ammonia and dibromide of ethylene, another series of salts is produced which is derived from one molecule of ammoniacal salt:—

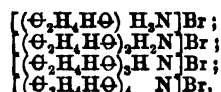
¹ H = 1; Θ = 16; Θ = 12, &c.

¹ In an early Number of the CHEMICAL NEWS there is an interesting note, to which the initials "C. L. B." are attached, concerning the purple colour observable in the liquid after the passage of sulphuretted hydrogen through a solution of perchloride of iron. When iodide of silver is dissolved in warm hyposulphite of soda the solution has a similar colour, appearing clear by transmitted, but slightly turbid by reflected light.—F. F.

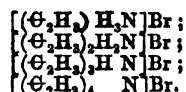


This series may be considered as derived from bromide of ammonium by the substitution for one, two, three or four equivalents of hydrogen of one, two, three, or four equivalents of the radical $\text{C}_2\text{H}_4\text{Br}$, which may be called bromethyl.

When this series is produced in the presence of water, the bromine within the radical may be further replaced by the residue $\text{H}\Theta$, hydrobromic acid being simultaneously eliminated, a third series of salts—viz., that of the oxethylated compounds—being thus formed, which contain



Lastly, the bromethylated salts may be converted into vinyl-compounds by the simple loss of hydrobromic acid giving rise to the production of four additional bodies, containing

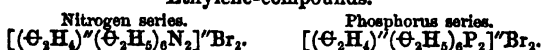


We thus see that no less than sixteen salts are, or may be, produced by the action of ammonia on dibromide of ethylene, and it is next to impossible to separate them, especially if the almost endless number of compounds intermediate between the principal classes be taken into consideration. But if the ammonia be replaced by monamines, it is obvious that in the same measure as the degree of substitution of the hydrogen in the ammonia advances, a smaller number of compounds will be obtained by the action of dibromide of ethylene; thus, a primary monamine could not give rise to the production of more than twelve salts; a secondary monamine, of not more than eight; and, lastly, a tertiary, of not more than four compounds.

By acting on dibromide of ethylene with triethylamine, triethylphosphine, and triethylarsine, Dr. Hofmann has, in fact, succeeded in obtaining in each series the four salts which are indicated by theory.

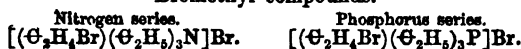
Diatomic Salts.

Ethylene-compounds.

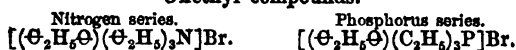


Monatomic Salts.

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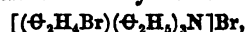
Oxethyl-compounds.



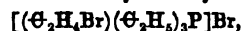
Vinyl-compounds.



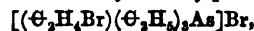
In all these reactions the principal products are invariably the three bromethylated bromides,



Bromide of bromethyl-triethylammonium,



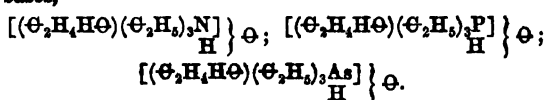
Bromide of bromethyl-triethylphosphonium,



Bromide of bromethyl-triethylarsonium,

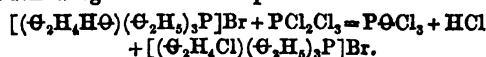
which give rise to a considerable number of interesting products.

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It is well known, indeed, that this hydrate loses anhydrous acid when distilled or boiled, and the object of the present communication is to ascertain the exact conditions under which this loss takes place, as this knowledge is of importance in a practical point of view.

The author occasionally found on distilling, and afterwards heating, oil of vitriol, that acid of the specific gravity of 1.843 was obtained, but at other times the specific gravity was as low as 1.842. To explain this difference, the following experiments were made:—

1. Sulphuric acid, having a specific gravity of 1.843, and a per-centage of anhydride of 81.62 by the alkali-meter, was put in a retort, burned in hot sand, and distilled. The distillate had a specific gravity of 1.840, and a per-centage strength of 80.12. It had, therefore, lost by distillation $1\frac{1}{2}$ per cent. of anhydride.

2. The weak acid got by the last experiment was heated for half-an-hour to 550°F ., and, after cooling, gave an acid of 1.84798 specific gravity, and strength of 81.615 anhydride.

3. A portion of this acid now restored to its full strength and specific gravity, was violently boiled for two hours. On testing the acid on cooling, it was reduced in strength to 80.01 of anhydride, and to a specific gravity of 1.838.

4. The weak acid obtained in the last experiment was kept for one hour at 550°F . On cooling, it had increased to 81.62 per cent. of anhydride, and the specific gravity was 1.84792.

As a general result of these experiments, it follows that the old specific gravity of 1.848 is more correct than that given by Beneau and Marignac; that there is a true monohydrate of sulphuric acid which loses anhydride near its boiling point, but not below 550° . The latter temperature should not be exceeded in the concentration of oil of vitriol.

On the Analysis and Chemical Constitution of Cast Iron and Steel, by M. H. CARON.

THE determination of carbon and silicium contained in cast iron and steel is not an easy operation, and no means have yet been discovered for the immediate analysis of these complex substances. It is, however, probable that their real nature will only be solved by comparing the results of a great number of analyses; and I shall, therefore, ask permission of the Academy to publish several of the means which I have actually tried, and then endeavour to throw light on some points which still remain obscure:

Carbon is easily separated from cast iron by means of a process long used in the laboratory of the Ecole Normale; but its inventor, M. H. St.-Claire Deville, has not, I believe, otherwise made it known.

In passing properly purified gaseous hydrochloric acid over the cast iron contained in a platinum vessel, in a red-hot porcelain tube, the carbon is isolated from all the associated substances, which will escape from it as volatile chlorides. Before employing the hydrochloric acid, it must be passed through a red-hot porcelain tube, containing wood cinders, or porous charcoal to remove oxygen. When this last precaution is neglected, a mixture of charcoal and silica is always obtained, whatever may be done to prevent it; and it is this circumstance which has led me to adopt the following process for estimating the silicium by the dry method.

In the preceding process, when the hydrochloric acid is replaced by a mixture of hydrochloric acid and atmospheric air, the silicium is obtained as silica, and remains in the vessel. This gaseous mixture, issuing from a small gasometer, passes, at the same time as the gaseous hydrochloric acid, through a small washing-bottle, containing a saturated solution of this acid, and then passes to the porcelain tube, where the analyses are effected. Perchloride of iron and carbonic acid are disengaged, and silica remains. If the cast iron contains titanium, aluminium, or calcium, the oxides or chlorides of these metals remain with the silica, from which it is easy to separate them. The theory of this operation is so simple that an explanation is superfluous. By this method I have succeeded in accurately weighing the silicium contained in cast iron and steel, and have found much more of it in them than is generally believed.

The actual quantities will be more suitably brought forward in a work which I shall soon have the honour of presenting to the Academy. As to nitrogen it should be searched for in cast iron and steel under two distinct forms, which it appears to me have hitherto been unperceived, since M. Fremy, in his "Note," of October 8th, 1860,¹ has not mentioned them. On this point I ask permission of the Academy to discuss several of the results published by M. Fremy.

Since the experiments of MM. Wöhler and H. St.-Claire Deville, it is well known that nitrogen has a special affinity for silicium and titanium. A considerable number of cast irons contain nitrocarbide of titanium or the titanium of the blast-furnaces. I am convinced that silicium is also to be found there,—in very small proportions it is true,—in the state of nitride of silicium. This it is which the immediate analysis ought to demonstrate, and in this the chief difficulties are encountered. The nitride of titanium and the nitride of silicium are substances which oppose an energetic resistance to chemical agents; but when they are separated in that state of tenuity to which they are brought by the powerful re-agents with which it is necessary to attack cast iron, they, unfortunately are acted on with a little more facility. An indirect process must, therefore, be resorted to when they are not visible either to the naked eye or by the microscope, as is sometimes the case with nitride of titanium. These difficulties are still further augmented by the fact that cast iron, after being dissolved, leaves a little protoxide of silicium, recently discovered by M. Wöhler. Neither must it be forgotten that the odour of the disengaged hydrogen is due almost exclusively to the presence of siliciuretted hydrogen, according to the observation of this illustrious correspondent of the Academy. However, the existence of nitrogen in cast irons is not so constant as M. Fremy seems to admit, according to the works of M. R. F. Marchand, whom he cites in his "Note;" for, in accordance with the conclusions of the German chemist, it is "impossible to be certain of the existence of nitrogen in cast iron and steel."² And, in fact, M. Marchand himself points out how easy it is to err in similar experiments. He vindicates the numerous precautions by which M. Boussingault, in his researches on azotised bodies, avoids all causes of error. M. Marchand has not yet recognised the exactitude of Schœffhautil's observations, who admitted that the nitrogen contained in iron and cast iron concentrates itself in the carbonaceous residue obtained by dissolving these metals in hydrochloric acid.

As to the brown carbonaceous substance, soluble in potash, spoken of by M. Fremy, Berzelius was perfectly acquainted with it.³ He compares it to ulmic acid, all the properties of which he, with some reason, attributes to it. He has not discovered nitrogen either in it or in the fetid oil of hydrogen which he considers as a carburetted hydrogen, apparently possessing the same composition as petroleum. If this brown substance disengages ammonia with soda,—which is not always the case,—it must be ascertained whether this nitrogen

¹ See *Comptes-Rendus*, t. II. p. 567.

² I think it is evident, after these experiments, that the existence of nitrogen in cast iron and steel should not be positively asserted. In all instances the amount of nitrogen should never be above 0.02 per cent., and in most cases, ought to be much below. If the iron contains nitrogen, it necessarily appertains to the substances mingled with the iron, which are no more integral parts of the iron than the scoria found mixed with it.—*Journal für Praktische Chemie*, v. Erdmann und Marchand, 1860, bd. xlix. p. 362.

³ Berzelius, "*Traité de Chimie*," Second Edition, t. II. pp. 697, 698.

does not proceed from the titanium, and, above all, from the silicium, which is uniformly met with.

Neither do I believe it possible to reconcile the action of sulphur, phosphorus, or arsenic, which communicate brittleness to all metals with which they are combined, by the action of carbon on the special group of metals analogous to iron. It must be admitted, it seems to me, that the cast iron hardened by charcoal differs essentially from the metals rendered gray by metalloids which indistinctly modify them.

After all, when cast iron and steel contain nitrogen, among the numerous bodies entering into their composition, which in particular fixes it? This is the query which I propose to myself. In answering it, I have been guided by the following considerations:—Pure iron and carbon do not combine directly with nitrogen at any temperature. Silicium and titanium—which latter burns in nitrogen—combine directly and very easily with nitrogen. Are not, then, the small quantities of nitrogen—M. Marchand has never found more than 1000ths, and, for the most part, much less—combined with silicium or titanium? I believe, then, that it is to the nitride of silicium or of titanium—existing in large quantities in certain cast irons—that the presence of nitrogen in the carbonaceous residuums of cast iron is to be attributed. When previously speaking of cast irons and steel, I did not include simply cemented iron in this group. This particular kind of steel may—as, in fact, Berzelius foresaw—contain paracyanogen.* After fusion it may be classed among the steels already mentioned.—*Comptes-Rendus*, t. li. p. 938.

On Nitro-prusside of Sodium as a Re-agent,
by A. OPPENHEIM, Ph.D.

THE colour of the compound formed through the action of nitro-prusside of sodium on alkaline sulphurets depends upon the nature of the alkali employed, and upon the proportion of sulphuret and alkali in solution. Ammonia always produces a purple colour; but if soda or potash are present in great excess the colour formed is red instead of purple. Organic compounds of sulphur which are not easily decomposed by alkalies seem, therefore, to give a different reaction to that of inorganic compounds. Bisulphuret of carbon, or oil of mustard, for instance, when digested with caustic soda, at a common temperature, give not a purple but a deep red colour with nitro-prusside of sodium. When boiled with soda, these compounds form sulphuret of soda more freely, and then give the purple colour with the re-agent. Albumen may be boiled for some time before the first reaction changes into the second one. Mercaptan proves the alkaline properties of its radical by forming the red colour without the addition of soda, which when added, however, intensifies it. A solution of sulphur in an organic medium, such as benzene, or oil of turpentine, when acted upon by soda, at a common temperature, does not give the red but the purple colour—the former appearing only when soda was added in great excess.

The action of nitro-prusside of sodium on alkaline sulphurets is much stronger and more rapid than that of metallic salts. I tried to determine the quantity of an alkaline sulphuret in solution by volumetric analysis, by precipitating it with alkaline solutions of metallic

oxides [of lead or of zinc], testing from time to time for the presence of the sulphuret with nitro-prusside of sodium. I found that the alkaline sulphuret was thus indicated, even in the presence of an excess of the metallic solution. For indicating the presence of alkalies, nitroprusside of sodium is superior to the best litmus paper, and for indicating alkaline earths it can be used when litmus fails to give a reaction. For the purpose of testing for these substances, sulphuretted hydrogen is passed for a short time through the solution previous to its being mixed with a few drops of the re-agent. A solution of 1 part of dry carbonate of soda in 20,000 parts of water thus assumes a colour dark enough to be diluted with 3 times its own volume of water without becoming indistinct. The reaction, however, is the slower in appearing and the quicker in disappearing in proportion as the solution is the more dilute. If, therefore, one part of carbonate of soda be dissolved in more than 40,000 parts of water, the reaction becomes indistinct. The limit of a distinct reaction for litmus paper seems to be, 1 part of carbonate of soda in 15,000 parts of water. Tincture of litmus is equally sensitive as nitro-prusside of sodium for indicating alkalies, but not for earths. One part of carbonate of lime in 20,000 parts of water containing carbonic acid is distinctly indicated through sulphuretted hydrogen and nitroprusside of sodium; whereas litmus, in such instance, indicates the carbonic acid only. Basic salts, such as the borate or phosphate of soda, give, as might be presumed, a strong reaction with nitro-prusside of sodium. Organic bases act in an analogous manner with regard to both re-agents. Those which are not indicated by litmus seem not to be capable of forming a sulphuret reacting upon nitro-prusside of sodium. Nicotine is indicated by both re-agents; quinine, cinchonia, and aniline are not. The sulphuret of nicotine, when evaporated at a common temperature over sulphuric acid, is split into sulphuretted hydrogen, which goes off, and the base which remains in solution. An alcoholic solution of aniline, when submitted to the reaction, assumes a green colour of a very transitory nature. In conclusion, it may be remarked, that nitro-prusside of sodium is capable of acting as a powerful oxidising agent. It forms brown resinous substances when acting upon volatile oils.

On the Reduction of Hydrate of Potassium to the Metallic State at the Boiling Point of Caoutchoucine,
by C. GREVILLE WILLIAMS.

It is customary to attribute the difficulties encountered by Sir Humphry Davy, in his endeavours to obtain boron, silicon, &c. by means of potassium, to the fact that he operated on the oxides. It is a popular expression, therefore, to say that he had thus to overcome more powerful affinities than would have been the case had he used the haloid compounds of the elements alluded to.

The experiments of M. Caron have shown that the chloride of calcium is not decomposed by sodium alone, whereas the iodide readily yields almost the theoretical amount of metal. This fact has served to increase the tendency to too rapid generalization as to the comparative affinities of the alkali metals for oxygen and the halogens.

It is common to say that potassium is more electropositive than sodium, and, therefore, that its affinity for oxygen is greater. I shall, however, presently relate an experiment, which, if regarded alone, and from one point of view, might be said to prove sodium to have a greater affinity for oxygen than is possessed by potassium.

The researches of Berzelius, Wöhler and Oerstedt, and the later experiments of M. Caron, have led Chemists

* In the Collection of the Ecole des Mines de Paris there are some specimens of cast iron so strongly impregnated with nitrocarbide of titanium, that the compound is visible to the eye.

† Berzelius, t. i. p. 323.

generally to the inference:—that the following is the order of difficulty of reduction

1. Oxides.
2. Chlorides.
3. Fluorides.
4. Iodides.

Again, it is usual, from the interpretation of one class of phenomena, to say that potassium is more electro-positive than any other metallic substance, accordingly it ought to decompose the oxides of all other metals, and, more especially, the oxides of comparatively electro-negative bodies like boron and silicon. If we grant this it is not correct to explain the difficulties Sir H. Davy encountered, by saying that, owing to his employing oxides, he had to overcome the highest affinities; because if silicon, boron, &c., are said to have more affinity for oxygen than for iodine, chlorine, &c., so also, on the other hand, has potassium.

The success of M. Caron's experiment, may, perhaps, be attributed less to sodium having more affinity for iodine than for chlorine, than to the circumstance of calcium having, at a red heat, a comparatively low affinity for iodine.

Further,—Sodium, in elementary works, is represented as standing next in electro-positive energy to potassium. Consequently it should decompose the oxides, chlorides, fluorides, &c., of all metals save potassium.

In these instances no allowance is made for special affinities. One or two experiments are considered sufficient to enable judgment *a priori* to be pronounced on all others. It is seldom, however, that a reaction of any one substance, though of a highly characteristic kind, can safely be taken as typical of the behaviour of all its congeners. This remark applies forcibly to organic chemistry. When the existence of homologous series began to force itself upon the attention of chemists, when instance upon instance became multiplied of homologous bodies yielding analogous reactions, an idea began to prevail of the existence of a universal law, and when exceptional reactions were observed, in many cases they were believed to arise from an insufficient fulfilment of the original conditions of experiment.

It is under the impression, therefore, of its being as useful to record apparent perturbations in laws, as to enunciate laws themselves, that I now record an observation or two, the results of which are at variance with the doctrines usually laid down in elementary works.

If, after fusing iodide of potassium in a glass tube, fragments of sodium be dropped in, no reaction becomes evident, even at a decided red heat. It is true that the sodium, after cooling, is much harder and more crystalline than before, and the globule breaks up, on the least touch, into angular fragments like starch; but no reduction takes place. This circumstance alone excites no surprise; but, taken in connection with M. Caron's mode of producing calcium, and the experiment about to be described, it becomes remarkable. If, on the other hand, we pour into a long test-tube a little caoutchouc and then a few fragments of sodium and caustic potash, and boil the mixture a short time, it will be found, on cooling, that the sodium has undergone a change in its physical properties. It is still perfectly metallic, but it no longer congeals at ordinary temperatures. Moreover, large or small fragments instantly inflame in contact with cold water. The sodium has, in fact, become largely alloyed with potassium derived from the reduction of the hydrate of potassium, at a temperature not exceeding 171° C. (339° F.). A globule formed on one occasion, was found on analysis to contain—

Sodium . .	76.5
Potassium .	23.5
	100.0

These numbers approximate to the formula, Na_2K , which requires 22.03 per cent. of potassium. I do not, however, regard this as more than a coincidence.

From the extreme ease with which the reaction takes place, it is perhaps not too much to expect that potassium may one day be formed on the large scale by a modification of this process.

As regards the theory of the reaction, it is evident that the presence of hydrogen in the hydrate of potassium facilitates its reduction, and, consequently, that under certain circumstances, an oxide may be more readily reducible than an iodide of the same metal, and that the most difficultly reducible metallic oxide may be decomposed, and the metal obtained at a lower temperature than has, hitherto, been deemed possible.

TECHNICAL CHEMISTRY.

On the Alloys of Copper and Zinc,
by FRANK H. STORER.

(Continued from page 305.)

ON the other hand, the very fact which I have myself observed, namely, that most, and probably all, alloys of copper and zinc may be crystallised in octahedrons, is in itself strong presumptive evidence that zinc is capable of assuming a similar form. Indeed, in the absence of any marked tendency in these alloys to separate by eliquation, there remains no hypothesis other than that of isomorphous mixture by which to explain their composition.

I must observe in this connection that G. Rose (*Pogg. Ann.* July, 1859, cvii. 448), has quite recently described a specimen of crystals, labelled "*Mengepresse*,"—a term applied to an alloy prepared from 1 lb. of copper from Lautenberg, and 2 lbs. of calamine,—which exists in the Royal Mineralogical Museum at Berlin, having been obtained from the collection of Klaproth. These crystals, in the opinion of Rose, appear to belong to the monometric system; but, since Professor Rose has published no analysis of them, we not only have no clue to their probable quantitative composition, but are left in doubt whether they are really a compound of copper and zinc, though the facts which I have here brought forward render the supposition an extremely probable one.

Notwithstanding this, and in spite of the fact that his alloy is only a single isolated example, Rose has urged, in direct opposition to his previous opinion, that it proves that zinc must belong to the regular system. In point of fact, however, the observation of Rose, taken by itself, evidently proves nothing of the kind. For, admitting that his crystals are really brass, he has offered no evidence to show that they do not belong to one of several possible definite alloys; only after proving that such crystals do occur through the whole series of alloys, as I have shown is really the case, and demonstrating that no definite compounds exist, could the idea of isomorphous mixture be entertained.

The crystals obtained by myself vary in size; some of them being more than half-an-inch in length, while others are quite minute. In general they are smallest in those instances where the greater part of the alloy had solidified before the crust was pierced.

In a few of the specimens of alloys, the crystals have exhibited a tendency to assume a somewhat tabular form,—a single face of an octahedron being largely developed, to the exclusion of the other faces; while in other

specimens separate individual crystals have occurred. Both of these peculiarities presented themselves in the alloy of 83.72 per cent. of copper.

Very fine groups of crystals were obtained from those alloys which contained only 1 or 2 per cent. of zinc. It is worthy of note, that, although these crystals have the same form and general appearance as those prepared by the same method from an equal quantity—3 lbs. or 4 lbs.—of pure copper, they are nevertheless much larger and more perfect. Since they may, for all practical purposes, be considered as crystals of copper with slight impurity of zinc, and are easily to be obtained, it would almost seem advisable to add 1 or 2 per cent. of zinc to the metal, employed in preparing specimens of crystallised copper for the cabinet. A similar case is presented by lead, which is very readily crystallised when it contains a little antimony,—a fact well exemplified by the beautiful cups of crystals of *Krütablei*, which are prepared by partially cooling the metal in ladles, at the Frankensharner smelt-works near Clausthal, and doubtless at other localities in the Hartz.

Since the crystals rich in copper which have just been described do not possess in any marked degree the yellow colour peculiar to brass, they are somewhat less interesting than those obtained from alloys containing more zinc. Crystals of the latter can be obtained with the greatest ease by re-melting old brass, or, better, by filling a Hessian crucible from the molten metal of the pots of a brass-founder, in which case all annoyance from the formation of a false crust of mixed oxide of zinc and metal is obviated. I mention these details, because I have myself found it somewhat difficult to acquire the knack of obtaining at will good crystals, when the alloys were prepared directly from the pure metals, having frequently been compelled to repeat a single experiment three or four times before satisfactory results were obtained. This is owing to the different degrees of rapidity with which the alloys cool,—a fact to which I have already alluded.

The most perfect individual crystals were obtained from a quantity of brazier's solder which had been prepared at the foundry of the Revere Copper Company in Boston, by fusing together 50 parts of copper with 50 parts of zinc. When an alloy of about this composition solidifies, and especially if it be suddenly cooled,—as happens when it is poured into iron ingot-moulds,—it assumes, as is well known, a highly crystalline structure, consisting almost entirely of a mass of coarse fibres, which shoot out from the points at which the alloy comes in contact with the cold metal of the mould. In the instances which have fallen under my notice, the ingots being from an inch to an inch and a half in depth, the fibres have shot up three-quarters of an inch, or more, from the bottom of the ingot, leaving only a sheet of metal about a quarter of an inch in thickness on top, which had cooled more slowly by contact with the air. Indeed these fibres, although described by Calvert and R. Johnson (*Journal of the Franklin Institute* [3], xxxvii. 200. See also *Philosophical Transactions*, vol. cxlviii. p. 367) as prismatic crystals, indicating that the alloy Cu Zn is a definite chemical compound, are evidently nothing more than collections of octohedral crystals, similar to those which form the fibres of sublimed sal-ammoniac and of several metals (*Vid. Savart, Annales de Chimie et de Physique* [2], xli. 65).

By comparing the striations on these fibres with those upon any of the crystals of the series, it is impossible to resist the conviction that the former are mere aggre-

gations. This conclusion is confirmed by the fact that the individual crystals which have been just mentioned as occurring above and among the fibres are evidently parts of the same system as the latter. It often happens that this alloy, known as "brazier's solder," is run into cylindrical moulds of two or three inches in diameter, pierced in earth. In this case the fibres shoot out from all sides towards the centre, leaving at the middle of the bar a sort of neutral ground in which tolerably well-formed crystals frequently occur. The solder is prepared for market by warming it slightly,¹ and then pounding it in an iron mortar till it is reduced to a somewhat coarse powder. With the fibres, this result is easily obtained, since no great effort is necessary to tear asunder the numberless little crystals of which they are composed; but the larger individual crystals which occur in the crust and core, as just described, are much more refractory; they are removed from the finer powder by sifting, and are subsequently remelted. Among these rejected "kernels" very good crystals may often be found.

The tendency to shoot out into fibres, which has been alluded to, and which deserves something more than a passing notice, extends over quite a space, from alloys containing 57 or 58 per cent. of copper, or even more, down to those containing 43 or 44 per cent., where it gradually disappears; as I have proved by casting a series of ingots. Although it does not altogether prevent one from obtaining crystals by the method of pouring off the still fluid portion of the alloy from that which has been allowed to solidify, still the crystals which I have obtained in this way within the limits of its influence are in general less perfect than those of the alloys containing more copper; indeed, on remelting the solder from which the finest separate crystals were obtained, and pouring off a portion of it after the rest had become solid, only indifferently good specimens could be procured, the alloy appearing to pass so quickly from the liquid to the solid state, that the crystals have but little time in which to form. It is remarkable that this inclination to form fibres is strongest in those alloys which contain nearly equal equivalents of zinc and copper, being less clearly marked as one recedes in either direction from this point, until a stringy texture analogous to that of copper is reached on the one hand, and the peculiar pastiness of zinc on the other. In preparing crystals, this pastiness manifests itself decidedly in the alloys immediately below those which are fibrous, becoming more strongly marked as the alloys are richer in zinc,—at least, so far as my own experiments have extended, i. e., to 30 per cent. or less of copper. The fracture of these white alloys is for the most part vitreous. The pasty condition appears to depend, to a certain extent, on the manner in which the alloy is cooled, being less apparent when this process has been rapid. I have repeatedly obtained fine cups, lined with tolerable crystals, from alloys which in other trials afforded nothing but a mass of paste. The transition, however, from complete liquidity to the pasty condition, when the latter is assumed, is very rapid.

The fact that the alloys just mentioned take on the fibrous texture when cooled under ordinary circumstances has moreover a very important practical bearing; alloys at the upper limit of this fibrous tendency being the lowest—i. e., richest in zinc—which can be rolled or subjected to the various processes by which metals are

¹ It must not be heated above a very moderate temperature, for, like metallic zinc at certain temperatures, it then becomes somewhat tenacious or pasty, and cannot be powdered.

wrought. Singularly enough, at a point just beyond the limit at which the fibres cease to be apparent,—viz., at 60 per cent. of copper,—an alloy of peculiar homogeneity occurs; its fracture, as seen when small bars are broken, being smooth and compact, and entirely unlike either the coarse, irregular, stringy fracture of alloys richer in copper, or that of alloys containing only a little more zinc, upon the fracture of which small bundles of fine crystalline fibres are often apparent.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

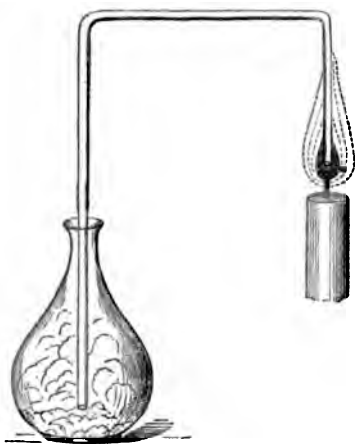
ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures¹ (adapted to a Juvenile Auditory), on the Chemical History of a Candle; by M. FARADAY, D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.

LECTURE II. (Dec. 29, 1860.)

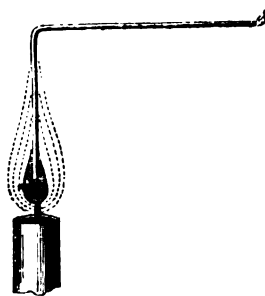
A Candle: Brightness of the Flame—Air necessary for Combustion—Production of Water.

We were occupied the last time we met in considering the general character and arrangement as regards the fluid portion of a candle, and the way in which that fluid got into the place of combustion. You see, when we have a candle burning fairly in a regular steady atmosphere it will have a shape something like the one shown in the diagram, and looking pretty uniform, although very curious in its character. And, now, I have to ask your attention to the means by which we are able to ascertain what happens in any particular part of the flame; why it happens; what it does in happening; and where, after all, the whole candle goes to: because, as you know very well, a candle being brought before us and burned, disappears, if burned properly, without the least trace of dirt in the candlestick—and this is a very curious circumstance. Now, in order to examine this candle carefully, I have arranged certain apparatus, the use of which you will see as I go on. Here is a candle; I am about to put the end of this glass tube into the middle of it—into that part which old Hooker has represented in the diagram as being rather dark, and which you can see at any time if you will look at a candle carefully, without blowing it about. We will examine this dark part first.



Now I take this bent glass tube, and introduce one end into that part of the flame, and you see at once that something is coming from the flame, out at the other end of the tube; and if I put a flask there, and leave it for a little while you will gradually see that something from the middle

part of the flame is drawn out and goes through the tube and into that flask, and there behaves very differently from what it does in the open air. It not only escapes from the end of the tube, but falls down to the bottom of the flask like a heavy substance, as indeed it is. We find that this is the wax of the candle made into a vaporous fluid—not a gas.—(You must learn the difference between a gas and a vapour: a gas remains permanent, a vapour is something that will condense.)—If you blow out a candle, you perceive a very nasty smell, consequent on the condensation of this vapour. That is very different from what you have outside the flame; and, in order to make that more clear to you, I am about to produce and set fire to a larger portion of this vapour—for what we have in the small way in a candle, to understand thoroughly, we must, as philosophers, produce in a larger way if needful, that we may examine the different parts. And, now, Mr. Anderson will give me a source of heat and I am about to show you what that vapour is. Now, here is a glass flask, and I am going to make it hot, as the inside of that candle-flame is hot, and the matter about the wick is hot. [The Lecturer placed some pieces of wax in a glass flask, and heated them over a lamp.] Now, I dare say, that is hot enough for me. You see that the wax I put in it has now become fluid, and there is a little smoke coming from it. We shall very soon have the vapour rising up. I will make it still hotter, and now we get more of it, so that I can actually pour the vapour out of the flask into that basin, and set it on fire there. This, then, is exactly the same kind of vapour as we have in the middle of the candle; and that you may see that that is the case, let us try whether we have not got here, in this flask, a real combustible vapour out of the middle of the candle.—[Taking the flask into which the tube from the candle proceeded, and introducing a lighted taper.]—See how it burns. Now this is the very vapour from the middle of the candle, produced by its own heat; and that is one of the first things you have to consider with respect to the progress of the wax in the process of combustion, and as regards the changes it undergoes. I will arrange another tube carefully in the flame, and I should not wonder if we were able, by a little care, to get that vapour to pass through the tube to the other extremity, where we will light it, and obtain absolutely the flame of the candle at a place distant from it. Now, look at that. Is not that a very pretty



experiment? Talk about laying on gas—why, we can actually lay on a candle! And you see from this that there are clearly two different kinds of action—one the production of the vapour, and the other the combustion of it—both of which take place in particular parts of the candle.

I shall get no vapour from that part that is already burnt. If I raise the tube (Fig. 1) to the upper part of the flame, so soon as the vapour has been swept out, what comes away will be no longer combustible; it is already burned. How burned? Why burned thus: In the middle of the flame where the wick is, there is this combustible vapour; on the outside of the flame is the air which we shall find necessary for the burning of the candle; between the two, intense chemical action takes place whereby the air and the fuel act upon each other, and at the very same time that we obtain light the vapour inside is destroyed. If you examine where the heat of a candle is, you will find it very curiously arranged. Suppose I take this candle and hold a piece of paper close upon the flame, where is the heat of that flame? Do you not see that it is not in the inside? It is in a ring, exactly in the place where I told you the chemical action was; and even in my irregular mode of making the experiment, if there is not too much disturbance, there will always be a

¹ Reported verbatim by special permission.

ring. This is a good experiment for you to make at home. Take a strip of paper, have the air in the room quiet, and put the piece of paper right across the middle of the flame,—(I must not talk while I make the experiment,)—and you will find that it is burnt in two places, and that it is not burnt, or very little so, in the middle; and when you have tried the experiment once or twice, so as to make it nicely, you will be very interested to see where the heat is, and to find that it is where the air and the fuel come together.

This is most important for us as we proceed with our subject. Air is absolutely necessary for combustion; and what is more, I must have you understand that *fresh* air is necessary, or else we should be imperfect in our reasoning and our experiments. Here is a jar of air, I place it over a candle, and it burns very nicely in it at first, showing that what I have said about it is true; but there will soon be a change. See how the flame is drawing upwards, presently fading, and at last going out. And going out, why? Not because it wants air merely, for the jar is as full now as it was before; but it wants pure, fresh air. The jar is full of air, partly changed, partly not changed; but it does not contain sufficient of the fresh air which is necessary for the combustion of a candle. These are all points which we as young chemists have to gather up; and if we look a little more closely into this kind of action, we shall find certain steps of reasoning extremely interesting. For instance, here is the oil-lamp I showed you,—an excellent lamp for our experiments,—the old Argand lamp. I now make it like a candle [obstructing the passage of air into the centre of the flame]; there is the cotton; there is the oil rising up it; and there is the conical flame. It burns poorly because there is a partial restraint of air. I have allowed no air to get to it, save round the outside of the flame, and it does not burn well. I cannot admit more air from the outside, because the wick is large; but if, as Argand did so cleverly, I open a passage to the middle of the flame, and so let air come in there, you will see how much more beautifully it burns. If I shut the air off, look how it smokes; and why? We have now some very interesting points to study: we have the case of the combustion of a candle; we have the case of a candle being put out by the want of air; and we have now the case of imperfect combustion, and this is to us so interesting, that I want you to understand it as thoroughly as you do the case of a candle burning in its best possible state. I will now make a great flame, because we need the largest possible illustrations. Here is a larger wick [burning turpentine on a ball of cotton]. All these things are the same as candles, after all. If we have larger wicks we must have a larger supply of air, or we shall have less perfect combustion. Look now at this black substance going up into the atmosphere; there is a regular stream of it. I have provided means to carry off the imperfectly-burned part, lest it should annoy you. Look at the soots that fly off from the flame: see what an imperfect combustion it is, because it cannot get enough air. What, then, is happening? Why, certain things which are necessary to the combustion of a candle are absent, and very bad results are accordingly produced; but we see what happens to a candle when it is burnt in a pure and proper state of air. At the time when I showed you this charring by the ring of flame on the one side of the paper, I might have also shown you, by turning to the other side, that the burning of a candle produces the same kind of soot—charcoal, or carbon.

But, before I show that, let me explain to you, as it is quite necessary for our purpose, that, though I take a candle and give you, as the general result, its combustion in the form of a flame, we must see whether combustion is always in this shape,—when I say "shape" I mean condition,—or whether there are other conditions of flame; and there are, and they are most important to us. I think perhaps the best illustration of such a point as that, being young ones, is to give you the result of strong contrast. Here is a little gunpowder. You know that gunpowder burns with flame; we may fairly call it flame. It contains carbon and other materials, which altogether cause it to burn with a flame. And here is some pulverised iron, or iron filings. Now, I purpose burning these two things together. I have a little mortar in which I will mix them. (Before I go into these experiments, let me hope that none of you, by trying to repeat them, for fun's sake, will do any harm. These things may all be very properly used if you take care, but, without that, much mischief will be done.) Well, then, here is a little gunpowder, which I put at the bottom of that little wooden vessel, and mix the iron filings up with it, my object

being to make the gunpowder set fire to the filings and burn them in the air, and thereby show the difference between substances burning with flame and not with flame. Here is the mixture, and when I set fire to it you must watch the combustion and you will see that it is of two kinds. You will see the gunpowder burning with a flame and the filings thrown up. You will see them burning too, but you will see them burning otherwise than in flame. They will each burn separately. [The Lecturer then ignited the mixture.] There is the gunpowder, which burns with a flame, and there are the filings: they burn with a different kind of combustion. You see, then, these two great distinctions; and upon these differences depend all the utility and all the beauty of flame which we use for the purpose of giving out light. When we use oil, or gas, or candle, for the purpose of illumination, their fitness all depends upon these different kinds of combustion.

There are such curious conditions of flame that it requires some sharpness and some cleverness to distinguish the kinds of combustion one from another. For instance, here is a powder which is very combustible, consisting, as you see, of separate little particles. It is called *lycopodium*, and each of these particles can produce a vapour, and produce its own flame; but to see them burn you would think it was all one flame. I will now set fire to a quantity and you will see the effect. We saw a cloud of flame, apparently in one body; but that rushing noise [referring to the sound produced by the burning] was a proof that the combustion was not a continuous or regular one. This is the lightning of the pantomimes, and a very good one too. [The experiment was twice repeated by blowing lycopodium from a glass tube through a spirit flame.] That is not a combustion like that of the filings I have been speaking of, to which I must now bring you back again.

Supposing I take a candle and examine it in that part which appears brightest to our eyes. Why, there I get these black particles, which already you have seen three or four times evolved from the flame, and which I am now about to evolve in a different way. I will take this candle and clear away the gutterage which occurs by reason of the currents of air; and if I now arrange a glass tube so as just to dip into this luminous part, as in our first experiment, only higher, you see the result. In place of having the same white vapour that you had before, you will now have a black vapour. There it goes, as black as ink. It is certainly very different from the white vapour, and when we put a light to it you will find that it does not burn, but that it puts the light out. Well, these particles, as I said before, are just the smoke of the candle; and this brings to mind that old employment which Dean Swift recommended to servants for their amusement, namely, writing on the ceiling of a room with a candle. But what is that black substance? Why, it is the same carbon which exists in the candle. How comes it out of the candle? It evidently existed in the candle, or else we should not have had it here. And now I want you to follow me in this explanation. You would hardly think that all those substances which fly about London, in the form of soots and blacks, are the very beauty and life of the flame, and which are burned in it as those iron filings were burned here. Here is a piece of wire-gauze, which will not let the flame go through it, and I think you will see, almost immediately, that when I bring it low enough to touch that part of the flame which is otherwise so bright that it quells and quenches it at once, and allows a volume of smoke to rise up.

I want you now to follow me in this point,—that whenever a substance burns, as the iron filings burnt in the flame of gunpowder, without assuming the vaporous state,—they may become liquid or they may remain solid,—they become exceedingly luminous. I have here taken three or four cases away from the candle, on purpose to illustrate this point to you; because, what I have to say is applicable to all substances, whether they burn or whether they do not burn,—that they are exceedingly bright if they retain their solid state, and that it is to this presence of solid particles in the candle that it owes its brilliancy.

Here is a platinum-wire which does not change by heat. If I heat it in this flame see how exceedingly luminous it becomes. I will make the flame dim for the purpose of giving a little light only, and yet you will see that the heat which it can give to that platinum-wire, though far less than the heat it has itself, is able to raise the platinum-wire to a far higher state of effulgence. This flame has carbon in

it; but I will take one that has not carbon in it. There is a material, a kind of fuel—a vapour, or gas, which ever you like to call it—in that vessel, and it has no solid particles in it; so I take that because it is an example of flame itself burning without any solid matter whatever; and if I now put this solid substance in it, and you see what an intense heat it has, and how brightly it causes the solid body to glow. This is the pipe through which we convey this particular gas, which we call hydrogen, and which you shall know all about next time we meet. And here is a substance called oxygen, by means of which this hydrogen can burn; and although we produce, by their mixture, far greater heat than you can get by the candle, yet there is very little light. If, however, I take a solid substance, and put that into it, we get a great light. If I take a bit of lime, which is a thing which will not burn, and which will not vaporise by the heat, and because it does not vaporise remains solid, and remains heated, you will find what happens as to the glowing of it. I have a most intense heat here produced by the burning of the hydrogen in contact with the oxygen; but there is as yet very little light—not for want of heat, but for want of particles which can retain their solid state; but when I hold this piece of lime in the flame of the hydrogen as it burns in the oxygen, see how it glows! This is the glorious lime-light, which rivals the voltaic-light, and which is almost equal to the sun-light. I have here a piece of carbon or charcoal which will burn and give us light exactly in the same manner as if it were burnt as part of a candle. The heat that is in the flame of a candle decomposes the vapour of the wax and sets free the carbon particles; they rise up heated and glowing as this now glows, and then enter into the air. But the particles when burnt never pass off from a candle in the form of carbon. They go off into the air as a perfectly invisible substance, about which we shall know hereafter.

Is it not beautiful to think that such a process is going on, and that such a dirty thing as charcoal can become so incandescent? You see it comes to this—that all bright flames contain these solid particles; all things that burn and produce solid particles, either during the time they are burning, as in the candle, or immediately after being burnt, as in the case of the gunpowder and iron-filings, all these things give us this glorious and beautiful light.

I will give you a few illustrations. Here is a piece of phosphorus, which burns with a bright flame. Very well; we may now conclude that phosphorus will produce, either at the moment that it is burning or afterwards, these solid particles. Here is the phosphorus lighted, and I cover it over with this glass for the purpose of keeping in what is produced. What is all that smoke? That smoke consists of those very particles which are produced by the combustion of the phosphorus. Here again are two substances. This is chlorate of potassa, and this is sulphuret of antimony. I shall mix these two things a little, and then they may be burnt in many ways. I shall touch them with a drop of sulphuric acid, for the purpose of giving you an illustration of chemical action, and they will instantly burn. [The Lecturer then ignited the mixture by means of sulphuric acid.] Now, from the appearance of things, you can judge whether they produce solid matter in burning. I have given you the train of reasoning which will enable you to say whether they do or do not. And what is this bright flame but the solid particles passing off?

Mr. Anderson has in the furnace a pretty hot crucible,—I am about to throw into it some zinc filings, and they will burn with a flame like gunpowder. I make this experiment because you can make it well at home. Now, I want you to say what will be the result of the combustion of this zinc. Here it is burning—burning beautifully like a candle, I may say. But what is all that smoke, and what are those little clouds of wool which will come to you if you cannot come to them, and make themselves sensible to you in the form of the old philosophic wool, as it was called? We shall have left in that crucible, also, a quantity of this woolly matter. But I will take a piece of this same zinc and make an experiment a little more closely at home, as it were. You will have here the same thing happening. Here is the piece of zinc; there [pointing to a jet of hydrogen] is the furnace, and we will set to work and try and burn the metal. It glows, you see; there is the combustion; and there is the white substance into which it burns. And so if I take that flame of hydrogen as the representative of a candle, and show you a substance like zinc burning in the flame, you will see that it was merely

during the action of combustion that this substance glowed—while it was kept hot; and if I take a flame of hydrogen and put this white substance from the zinc into it, look how beautifully it glows, and just because it is a solid substance.

I will now take such a flame as I had just now and set free from it the particles of carbon. Here is some camphine, which will burn with a smoke; but if I send these particles of smoke through this pipe into the hydrogen flame you will see they will burn and become luminous, because we heat them a second time. There they are. Those are the particles of carbon re-ignited a second time. They are those particles which you can easily see by holding a piece of paper behind them, and which, whilst they are in the flame, are ignited by the heat produced, and, when so ignited, produce this brightness. When the particles are not separated you get no brightness. The flame of coal-gas owes its brightness to the separation, during combustion, of these particles of carbon, which are equally in that as in a candle. I can very quickly alter that arrangement. Here, for instance, is a bright flame of gas. Supposing I add so much air to the flame as to cause it all to burn before those particles are set free, I shall not have this brightness; and I can do that in this way:—If I place over the jet this wire-gauze cap, as you see, and then light the gas over it, it burns with a non-luminous flame, owing to its having plenty of air mixed with it before it burns; and if I raise the gauze up, you see it does not burn below. There is plenty of carbon in the gas; but, because the atmosphere can get to it and mix with it before it burns, you see how pale and blue the flame is. And if I blow upon a bright gas-flame, so as to consume all this carbon before it gets heated to the glowing point, it will also burn blue. [The Lecturer illustrated his remarks by blowing on the gas-light.] The only reason why I have not the same bright light when I thus blow upon the flame is, that the carbon meets with sufficient air to burn it before it gets separated in the flame in a free state. The difference is solely due to the solid particles not being separated before the gas is burnt.

You observe that there are certain products as the result of the combustion of a candle; and that of these products one portion may be considered as charcoal, or soot; that charcoal, when afterwards burnt, produces some other product; and it concerns us very much now to ascertain what that other product is. We showed that something was going away; and I want you now to understand how much is going up into the air; and for that purpose we will have combustion on a little larger scale. From that candle ascends heated air, and two or three experiments will show you the ascending current; but, in order to give you a notion of the quantity of matter which ascends in this way, I will make an experiment by which I shall try to imprison some of the products of this combustion. For this purpose I have here what boys call a fire-balloon; I use this fire-balloon merely as a sort of measure of the result of the combustion we are considering; and I am about to make a flame in such an easy and simple manner as shall best serve my present purpose. This plate shall be the "cup," we will so say, of the candle; this spirit shall be our fuel; and I am about to place this chimney over it, because it is better for me to do so than to let things proceed at random. Mr. Anderson will now light the fuel, and here at the top we shall get the results of the combustion. What we get at the top of that tube is exactly the same, generally speaking, as you get from the combustion of a candle; but we do not get a luminous flame here, because we use a substance which is feeble in carbon. I am about to put this balloon—not into action, because that is not my object, but to show you the effect which results from the action of those products which arise from the candle, as they arise here from the furnace. [The balloon was held over the chimney, when it immediately commenced to fill.] You see how it is disposed to ascend; but we must not let it up, because it might come in contact with those upper gas-lights, and that would be very inconvenient. [The upper gas-lights were turned out at the request of the lecturer, and the balloon was allowed to ascend.] Does not that show you what a large bulk of matter is being evolved? Now, there is going through this tube [placing a large glass tube over a candle] all the products of that candle, and you will presently see that the tube will become quite opaque. Suppose I take another candle and place it under a globe, and then put a light on the other side, just to show you what is going on. You see that the sides of the jar become cloudy and the light begins

to burn feebly. It is the products, you see, which make the light so dim, and this is the same thing which makes the sides



of the jar so opaque. If you go home and take a spoon that has been in the cold air and hold it over a candle—not so as to soot it—you will find that it becomes dim just as that jar is dim. If you can get a silver dish, or something of that kind, you will make the experiment still better: and now, just to carry your thoughts forward to the time we shall next meet, let me tell you that it is *water* which causes the dimness, and when we next meet I will show you that we can make it, without difficulty, assume the form of a liquid.

[The Third Lecture will appear in our next Number.]

PHARMACEUTICAL SOCIETY,

Wednesday, January 2, 1861.

T. N. R. MORSON, Esq., President, in the Chair.

THE adjourned discussion on the "Process of Displacement" was resumed by the Chairman, who remarked that for the process to be successful, it was necessary that the bodies to be percolated, should be reduced to a minute state of division, so that the fluid might readily act on them. In a majority of cases, no process answered so well, and it is largely employed practically. It cannot in all cases replace maceration; but whenever it can be employed, there is no doubt it is superior.

MR. WAUGH said the great recommendation to the process was its economy. Ten gallons of spirit were put into the percolator, and ten gallons of tincture were obtained; while, in the case of a tincture prepared by maceration, a certain quantity always remained in the marc. He thought the use of a tap to control the flow very advisable, and always employed one.

MR. SANDFORD objected that the use of a tap encouraged an operator to pack carelessly.

PROFESSOR REDWOOD said, that what was wanted was an indication of the relative advantage of the two processes in particular cases. In some, the advantages of percolation were very decided: in the case of ginger, for example, one pound of which could be perfectly exhausted with a pound of rectified spirit in about two hours. The finest powder ought, however, to be used. But in many other cases the process of percolation was altogether inapplicable, and it was very desirable that a careful set of experiments should be made, in order to determine in what cases it might be used with advantage, and in what it could not. Poppy-heads, for example, could not be per-

colated; and in general, he might state that where only coarsely-powdered materials could be employed, the process was disadvantageous. Nor could all finely comminuted substances be percolated. Nut-galls in fine powder form a magma, through which nothing will pass. He might mention one case in which percolation might be useful,—that is, in the preparation of liquor potassæ. Mr. Wood had found that a solution of carbonate of potash after percolating through finely-divided lime, came out completely decomposed. Tinctures, as a class, he thought, should not be percolated; but he repeated that what was wanted, was a careful classification of the cases in which the process might be employed, and in which it ought not. Soubeiran, one of the first advocates of the process, had now given it up.

MR. ROBBINS remarked that the case of liquor cinchonæ was one in which percolation was particularly applicable, inasmuch as the bark in that way could be exhausted with very little water. In the preparation of the acetous extract of colchicum, also it was very useful, for the colchicum could be exhausted by a small bulk of fluid and a good deal of time saved in the subsequent evaporation. He might instance one case which illustrated the economy of the process. He had recently prepared a tincture of orris-root with half-a-pound of the powdered root and 16 ounces of spirit, and by adding water to displace the spirit, he had recovered 15½ ounces of tincture.

A MEMBER thought the principle of displacement good, and that there were very few cases in which it could not be employed. In the case of gummy substances, all the difficulties could be overcome by mixing them with clean fine sand before placing them in the apparatus. Leaves, such as henbane and hemlock, gave a much better result when percolated than when macerated. Poppy-heads, too, when carefully dried, and powdered fine enough to pass through a sieve with thirty holes to the inch, could be very successfully percolated.

MR. SQUIRE said that a process to be generally useful should not require much art, and he thought that for druggists who prepared but small quantities of tinctures at a time maceration was decidedly preferable. He thought that Dr. Burton's plan of placing the materials in a bag, and suspending them at the top of the vessel deserved more consideration than it had yet received. He recommended that comparative experiments should be made with substances which might be used in fine powder.

MR. HASLDEN thought that the displacement process had its advantages and disadvantages. There was no doubt that most concentrated infusions could be made best by percolation. With cascarrilla and calumba it answered perfectly, but for the compound infusions of gentian and orange peel it was not applicable.

THE CHAIRMAN said that when the form of apparatus necessary, and all the conditions to be observed—such as the state of division to which the substances should be reduced, and the length of digestion—were well understood, the process might be universally adopted, but not before.

A beautiful stuffed specimen of the Musk Deer, presented to the Society by Messrs. Allen and Peake, was exhibited to the meeting, and a short paper "*On Musk*," by MR. PEAKE, was read.

The specimen is a full grown male, with the musk pod *in situ*. In general form and size it closely resembles the common deer, but the hair is thick and strong, and very like small porcupine quills. The habits of the musk deer are a good deal like those of the hare. It is common on the spurs of the Himalayas, at an altitude of from 10,000 to 14,000 feet, and is usually snared by the hunters, but the specimen exhibited was shot. The pursuit of the animal is extremely toilsome, and sometimes hazardous, so that there is no probability of sufficient musk being obtained to cause any great alteration in the price. The musk is found in a thin membranous sac, which lies under

the outer skin of the abdomen. The sacs, or pods, usually contain from two drachms to one ounce of musk, according to the age of the animal. At one year old, the pod contains no musk; at two years old, only a yellowish milky substance is found, and it is not until the animal is three years old that sufficient musk is found for extraction. The natives, however, sometimes cut off the young pods and fill them with a mixture of the dried liver and blood of the animal, with some musk, and sell them for full-grown pods. It is difficult to prevent the natives from making additions to the best, and a correspondent of the author mentioned an instance in which musk pods given by a native prince as a valuable present, were nearly worthless from the adulteration which had been practised. The mountains on which the musk deer are found extend from the interior of India towards Thibet and Chinese Tartary, and it was probable that all the varieties of musk known as Russian, Chinese, and Nepal, came from the same district, and was the produce of the same species, the differences in appearance depending upon the age of the animal, and the way in which the pod is dried. The produce of one year received by the correspondent of the author was 120 pods, containing from 110 to 120 ounces of musk.

A vote of thanks to the donors for their valuable present was passed unanimously.

Mr. SQUIR exhibited an Improved Liebig's Condenser. It consisted of a helical glass tube, instead of a straight one, which could be enclosed in the ordinary tin casing, and set up vertically.

A paper "On the Prevention of Accidental Poisoning," by Mr. JULIUS SCHWEITZER, was then read.

The leading ideas in Mr. Schweitzer's paper have been already communicated to the readers of the *CHEMICAL NEWS* in a letter from Messrs. Savory and Moore (See Vol. ii. p. 116). It will only be necessary for us, therefore, to give a short *resumé*. The principal questions discussed in the course of the paper were the following:—

1. What is a poison? 2. Is it possible to prevent cases of accidental poisoning, and what are those cases? 3. Can we improve the present mode of keeping poison? 4. Can we insure the proper and safe mode of using strong, active medicines after they have been confided to the hands of the public? On the first question, the author observed that poisons, as a class, do not differ from other medicines in external, physical, or chemical properties: their injurious influence on healthy animal life alone distinguishes them from everything else. But as an excessive use of even the most innocent article of daily life may prove fatal, it requires more than the simple property of doing harm to constitute a poison. These enemies to health and life are dangerous or poisonous in consequence of the small amount which has the power to deprive us of life. Easy as it may seem at first sight to separate poisons from ordinary household medicines, a closer inspection reveals considerable difficulties. From the innocent manna, to the poisonous calomel and croton-oil, such a number of intermediate drugs and preparations are met with, which, from mild, to stronger and more active, lead us so gradually from one to another, as to leave us apparently no opportunity of drawing a clear line of distinction between the manna and its poisonous followers. The only characteristic of a poison, therefore, is the small quantity or dose which will prove fatal to life. As an arbitrary zero separates heat and cold, both one and the same physical force, the author proposes a similar conventional separation of medicines based on their recognised doses, by means of which a pretty correct estimate of their comparatively poisonous nature can always be formed. The selection of a zero in the scale of poisonous medicines requires a certain amount of consideration; but the author suggested that every solid, the maximum dose of which does not exceed five grains, and every liquid, the maximum dose of which does not exceed a drachm,

should be considered a poison. Leaving the criminal and intentional poisonings,—which, it was remarked, could only be prevented by the watchfulness of the chemist, and the terror of the law—the author proceeded to show that the accidental poisonings, which happened from selling a poisonous for a harmless medicine, or from a mistake in dispensing a prescription, or from the administration of an overdose by the friends or attendants of a patient might be easily prevented by keeping and sending out poisonous medicines in vessels which indicate the dangerous nature of their contents by the way in which they discharge them. As many poisonous medicines are more frequently in demand than any others in the *Materia Medica*, it would be found practically impossible to keep them secluded and in out-of-the-way places; the author therefore in such cases suggested the use of a dispensing bottle, which is so constructed as to limit the flow of the contents. It consists of an ordinary bottle having a removeable neck, with a closed bottom perforated with two small holes. When the stopper is removed and an attempt is made to pour from the bottle, one of these holes admits air, while the other allows a small stream of the fluid to flow out. So far from causing any appreciable delay in dispensing, the use of such bottles would probably save time, as they would assist in the accurate measurement of the liquid, and so obviate the necessity of pouring backwards and forwards from measure to bottle. The advantage of having a removeable neck is that the bottle may be quickly filled and easily washed out. In like manner, a poisonous solid might be dispensed from a bottle having a tapering contracted neck which could be filled through a larger aperture in the bottom. (Specimens of such bottles as the author described were exhibited to the meeting.) On the last question the author urged the propriety of always sending out "drops" in bottles with contracted necks, and poisonous external applications in others with a rough distinctive exterior. These simple and practical contrivances, he believed, would make every poisonous ingredient and medicine its own watchful monitor of danger, without occasioning the slightest impediment to its daily use; and so, he expected, would contribute to the prevention of many a fatal mistake or life-long repented oversight.

Mr. WAUGH said every one could see the advantage attending the use of the small bottles for laudanum, but he preferred a clear direction for a poison label, and feared that by permitting nurses and others to trust to the mere shape of the bottle, the mischief would be increased. The great protection in these as in other cases, was to be found in people using their common sense and keeping their wits about them. Considering how much medicine was taken, it was a standing miracle that so few accidents happened, and the only way to entirely prevent accidental poisonings, would be to get people to give up taking physic altogether, and let them die in a natural way.

Dr. SQUIR said, that in attempting to define what was a poison, Mr. Schweitzer had only shifted the difficulty from the word "poison" to "dose," there being no recognised maximum dose for anything, and with many things it would be a matter of option whether it should be placed in a poison bottle or not. A coloured label was the most useful protection, and if people would not take the trouble to read labels he thought they deserved to be poisoned. He believed that to trust to the shape of the bottle to indicate a poison was to lean on a broken staff that could afford no support.

Mr. HASLDEN said there could not be a doubt of the utility of the small bottles for laudanum, which, if sent out in that way, could never be taken in a hurry for something else.

Mr. SCHWEITZER, in reply, said that one case of accidental poisoning was one too many. By the use of the proposed bottles an oversight or mistake would be an impossibility. When a dispenser, nurse, or patient took up such a bottle by mistake and attempted to pour out a poisonous dose,

they would find that it did not flow, but came out drop by drop, which assuredly would remind them that they had taken the wrong bottle. All that these bottles were intended for was to remind the patient and dispenser, and furthermore, enforce time for consideration.

CHEMICAL SOCIETY, December 20.

Professor BRODIE, F.R.S., President, in the Chair.

THE Rev. R. W. Bowditch and Dr. L. Thudichum were elected Fellows.

Mr. J. H. SIMS read a paper "On the Laws of Gas Absorption." Regnault had shown that several gases which within certain limits of pressure obeyed Boyle's law at one temperature, disobeyed it at another. Roscoe and Dittmar had shown that many very soluble gases, examined at the temperature 0°C. did not obey Dalton's law of absorption in proportion to pressure. The author determined to examine some of these gases in order to ascertain whether the variations from Dalton's law of pressures were persistent at different temperatures, or whether, like the variations from Boyle's law, they existed at one temperature but not at another. The solubility of sulphurous acid gas in water was ascertained at the temperature of 7°, 20°, 40°, and 50° C. From these experiments it was concluded that the deviations from the law of Dalton which sulphurous acid and water exhibit, measured under pressures from 0.03 to 2 metres of mercury, vary according to the temperature at which the absorption takes place, and that this deviation becomes less as the temperature increases, so that while below 40° the non-accordance with the law is considerable, above that temperature no variation can be perceived. The author took an opportunity of examining the vapour-tension of sulphurous acid at temperatures varying from 0° to 15°. His numbers correspond very closely with those subsequently published by Regnault. Experiments on the absorption of ammonia in water were also made, and the results obtained were found to correspond with those obtained with sulphurous acid. Roscoe and Dittmar's numbers at 0° C. were compared with numbers obtained by the author at 20°, 40°, and 100°. The variations from Dalton's law diminished with the increase of temperature, and were imperceptible at 100°. In both gases the variations at low temperatures are variations of excess, that is to say, the absorption increases more rapidly than the pressure.

Dr. BENGE JONES, F.R.S., read a paper "On Sugar in the Urine." His principal conclusions were as follows:—Lehmann's process for detecting sugar in the urine by extracting the evaporated residue with absolute alcohol, and precipitating the sugar therefrom in the form of potash sugar, by means of alcoholic potash, cannot be employed when small quantities of sugar are present in large quantities of urine. The process of fermentation is stopped by the residue of the urine, by much urea, and still more decidedly by oxalate of urea. Half-a-grain of sugar in water can be detected by the alcohol produced, and may be estimated by the carbonic acid produced; but much larger quantities may be entirely overlooked in concentrated urine. In decolorising urine for examination in the polarising saccharometer some sugar is always lost. Animal charcoal removes sugar in proportion to the amount of charcoal used. This sugar may be recovered by washing with boiling water. Two-thirds of the sugar in urine may be lost by Robiquet's method of decolorising with basic acetate of lead and ammonia. Pettenkofer's test for sugar by means of cholic or glycocholic acid and sulphuric acid is the most delicate known. Two-thirds of a milligramme may be detected in a little distilled water, and the presence of a small amount of urinary colouring-

matter does not affect the reaction. Trommer's test with sulphate of copper and potash is capable of discovering 1/10th of a per cent. of sugar in urine, but when very small quantities of sugar are in solution with muriate of ammonia or urea, the reduction of the oxide is not perceived. Brücke's alcohol process was not found to be satisfactory, but his lead process furnished excellent results. The urine is precipitated first with neutral acetate of lead, then with basic acetate of lead, and lastly with ammonia. The ammoniacal precipitate contains the sugar which is extracted by treating the precipitate with oxalic acid, or preferably by sulphuretted hydrogen. By Brücke's process one-seventh of a grain of sugar added to 200 cubic centimetres of urine could be detected, and two-thirds of all the sugar added could be recovered. Moreover, the sugar is obtained free from salts, so that it can be fermented, and free from colour so that it may be examined by the saccharometer. The presence of sugar could be readily ascertained by this process in 1000 cubic centimetres of urine. The sugar separated by Brücke's process from 1000 cubic centimetres of the urine of a healthy man was estimated by the reduction test to vary from 1.4 to 2.2 grains, and in that of another man to vary from 2.3 to 3.0 grains. The sugar separated from 5000 cubic centimetres of the urine of one healthy man gave from 7 to 8 degrees of rotation in the saccharometer, and that of another healthy man from 10 to 11 degrees. The sugar extracted from 14,000 cubic centimetres of healthy urine yielded by fermentation 1.8 grains of carbonic acid, together with a recognisable quantity of alcohol. These and other experiments fully confirmed Brücke's statement as to the habitual presence of sugar in healthy urine. Hence diabetes must be regarded as an exaggeration of a healthy state, and not as a distinct and peculiar condition of the system.

Dr. OFFENHEIM read a paper "On the Separation of Tellurium from Selenium and Sulphur,"—(this will be given next week)—also, a paper "On Nitro-prusside of Sodium as a Re-agent"—(see page 21).

NOTICES OF BOOKS.

The Boy's Playbook of Science. Second Edition. By J. H. PEPPER. *The Playbook of Metals.* Same Author. London: Routledge, Warne, and Routledge, Farringdon Street. 1860.

PERHAPS there is no public scientific lecturer whose name is so familiar to the present and rising generations as that of the author of the above-named works. His presence at the Polytechnic Institution, where most of us have had the opportunity of appreciating the animated style of his delivery, and the cleverness with which he selected the most striking experiments to illustrate his subject, was a tower of strength to that establishment, which, by the way, deserves a far greater amount of patronage than it receives. By him some thousands of the rising generation have been first made practically acquainted with the nature of an electrical shock, and initiated into the effects which a magic lantern on a large scale is capable of producing. One of the first among the hundreds of engravings contained in the "Boy's Playbook of Science," is an amusing representation of the *modus operandi* by means of which certain of the accessories were produced. On one side of the screen sits the audience, on the other we see a man actively engaged in striking two sheets of metal together; another is hammering a big drum, and a little imp is tumbling head-over-heels in his eagerness to overtake a cannon-ball which is rolling rapidly towards the screen. In the same open and undisguised manner many other of the experiments which have astonished and delighted thousands of youngsters, are revealed, so that they are

taught how to produce like effects. It would, however, convey a false idea of the contents of this book, if we were to allow it to be inferred that it treats merely of tricks and illusions; on the contrary, it is full of information on scientific subjects of every kind. The style in which they are treated is as free from technical terms as is consistent with a clear exposition of the matter under consideration; and even the most abstruse of them are dealt with in a way to bring them within an ordinary comprehension. The illustrations employed are simple and accurate, and the anecdotes which are introduced are well chosen. In writing of sound, he mentions a very curious fact which, in these days of "mediums," is not without interest. He says that there was in Paris a woman who could produce musical sounds by means of the muscles and tendons of the thigh, which could be heard distinctly from one end of the room to the other. That portion of the book which treats of steam, and the construction of steam-engines, is especially good; and he seems to have been anxious to do justice to all concerned in the application of this power; but we cannot help regretting that in the list of names, which commences with that of Hero of Alexandria, who flourished, as the term is, about 100 years B.C., he has omitted that of Papin, who, in 1707, launched his vessel, moved by steam, on the river Fulda, in the presence of the Landgrave of Hesse-Cassel, and shortly after set out on a voyage to London in it, but was stopped by the watermen on one of the German rivers, and himself and his vessel seized. What became of the latter it is impossible to say, it is only known that he could not recover it. All his hopes seemed to be set on reaching the port of London in his steamer, and, having been disappointed in this, he did not live long after his arrival in this country.¹

In reference to the application of scientific knowledge to the affairs of common life he mentions two instances, which are too interesting to be passed over in silence. It will be remembered that during the war in the Crimea it was of no unfrequent occurrence for artillerymen to be shot while working the guns in the batteries, from their eagerness to see the effect of the shell or shot they had just sped in the direction of Sebastopol. All remonstrances were useless; they cared less for exposing their lives to the fire of the Russian riflemen than for seeing the damage they caused the enemy's works. In order that they might gratify this propensity without risk, the Rev. W. Taylor invented a reflecting spy-glass, which enabled them to see this without raising their heads above the parapet of the battery. The arrangement of the mirrors is fully explained and illustrated in the work before us. The same gentleman also invented another form of glass for the purpose of enabling the gunner to aim his gun in safety; and the value of the invention was so clearly demonstrated to Lord Panmure that he ordered several of them to be constructed, but, in consequence of the cessation of the war, there was no opportunity of using them. The other instance we shall mention refers to the discovery, by means of a microscope, of the locality where a box was emptied of the gold it contained, and its place supplied with sand. On the robbery being made known, Professor Ehrenberg, of Berlin, requested that specimens of the sand, at every station along the railway over which the box had travelled, should be forwarded to him. These he examined under the microscope, and compared with that contained in the box, and so arrived at the discovery of the station where the substitution had been effected.

¹ Writing to Leibnitz from Cassel, before starting on his voyage, he said:—"Il est important que ma nouvelle construction de bateau soit mise à l'épreuve dans un port de mer comme Londres, ou on pourra lui donner assez de profondeur pour appliquer la nouvelle invention qui, par le moyen du feu, rendra un ou deux hommes capable de faire plus d'effet que plusieurs centaines de rameurs."

So perfect is this book in other respects, that we regret to have to mention inaccuracies in the spelling of names, and in one instance a total change of name, which ought, certainly, not to have appeared in a second edition. Thus, Canson is spelt Cansan, Allan is spelt Allen, and E. J. Lowe is metamorphosed into E. S. Lane.

The title of a book is supposed to convey some idea of its contents; therefore when we read "The Playbook of Metals," we expect to find a work giving a little information as to the nature of the metals, and a great deal respecting the more striking instances of their application to engineering purposes, and so forth; but we certainly do not expect to find it to be a book so complete in all its parts, as that which presents itself to notice under this title. Commencing with the mineral, coal, it gives an account of its formation, the districts in which it is found, and the mode of working it. The appearances presented by the coal-bearing strata, are carefully and clearly shown by numerous diagrams; and the mode of boring for its discovery is fully explained; nor does the author fail to describe the principal fossils which have been, and are still, commonly found in the beds of this mineral. As he has himself descended into a large coal-mine, and travelled through it, he is able to give us his own experience with respect to the manner in which the operations are carried on, and the circumstances under which the hardy miner performs his part in the battle of life. With the Lundhill colliery explosion, by which one hundred and ninety persons lost their lives, and the still more recent Risca colliery explosion, which destroyed one hundred and forty-two individuals, fresh in our recollection, we should have been glad if the author had turned his scientific and mechanical knowledge to account in suggesting a light which would be more effectual in the prevention of such accidents. The Davy lamp is objected to by the miners on the ground of its giving an insufficient light; and though the improved Davy lamp constructed by Simons, is not open to that objection to the same extent, yet we cannot agree with Mr. Pepper, that the miner cannot remove the wire gauze without throwing down the extinguisher over the flame. One of these lamps was shown to us when first invented, and this latter safeguard was especially dwelt upon; but it was not difficult to see that a reckless miner—of whom there are only too many—would have no great difficulty in preventing the extinguisher from acting while he removed the gauze to light his pipe, without the overseer of the mine being likely to discover it.

After having disposed of the fuel so essential in the preparation of metals, the writer proceeds to give an account of the early workers in these substances. The principal alchemists of whom records exist are noticed in succession, and some very curious extracts from their works are given, the jargon in which they are written being very ingeniously interpreted. The means by which they performed some of their most striking experiments, the reading of which still astonishes many persons who think that, after all, these impostors must have known something which we modern chemists do not know, are explained in the simplest manner; and, on the whole, very fair justice is meted out to these worthies of the olden time. The extent to which metallurgists are indebted to their labours for information on the subject of smelting ores, and for designs of vessels, such as crucibles and so forth, is clearly shown in the quaint engravings copied from ancient MSS., and in the very complete summary which accompanies them. It is astonishing to find how many of the utensils used in mining ages ago are still retained, almost without alteration.

The method of separating gold from quartz and other substances with which it is associated, as well as of silver and the other metals, is described in a clear and popular style, as are also the various modes of performing an assay of the respective ores; and the information on this,

as well as on other points, is brought down to the present date.

There are many cautions scattered through the book, which though for the greater part unnecessary to experienced metallurgists and chemists, are yet very properly given when it is considered that it is professedly written for beginners, and among these is one founded on an example which may not be without its use to some among our readers. "Gold is attacked by mercury, and dissolved in it with considerable rapidity, and the author remembers a case of vanity reproved in which the mercury contained in a mercurial trough used for certain experiments with gases that are soluble in water, formed the corrective agent. A young gentleman who had made himself conspicuous as a pupil in a chemical class, by wearing a very showy collection of jewellery on his person, was induced by the brilliancy and pretty appearance of the quicksilver in the open trough, to stir it about with his fingers, on one of which was a diamond ring; no one made any remark, but at the end of the discourse a sudden cry was heard, 'where's my ring?' The debris of this precious ornament remained floating and dissolving in its mercurial grave. The diamond alone remained perfect, and this, if the author remembers properly, the other mischievous pupils recommended should be heated red-hot in a crucible with some nitre, and was either burnt away or lost." The moral of this anecdote is that pupils in chemistry should either abstain from jewellery altogether, or put it in their pockets before entering the laboratory.

In concluding our notice of "The Playbook of Metals," we cannot refrain from complimenting the author on the extent and variety of the knowledge he has displayed in writing it, and on his industry in searching through so many old books for valuable and curious information. The inaccuracies in the spelling of names which we noticed in the first-mentioned book do not occur in this, except in one instance, evidently due to the reader, where the name of a historical personage is so entirely changed that it would be impossible to recognise for whom it was intended but for the circumstance with which it is associated. The engravings with which it is illustrated are 300 in number, it is stated, and these are good and highly interesting, with a few exceptions which might as well have been omitted.

CORRESPONDENCE.

Homogenesis of Forces.

To the Editor of the CHEMICAL NEWS.

SIR,—Your correspondent, "E. M. T." asks if steel does become magnetic when placed in a particular part of the solar spectrum? I would refer him to any good book on Magnetism, where he will find an account of Mrs. Somerville's experiments.

"E. M. T." seems to think that I am the originator of the theory that all the phenomena of nature are produced by gravitation, motion, light, heat, electricity, and chemical affinity, and that these six may be produced by one of them, which in its turn cannot be produced; but this is quite a mistake: the sole object of my letter being to endeavour to prove, that "if the causation of all the phenomena of the universe be referred to one force," (of the six above mentioned) "it must be to gravitation, not to matter in motion;" therefore, if he has anything to say for or against the theory, he must say it to the propounder of it, not to me.—I am, &c.

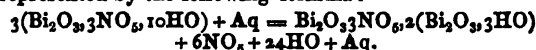
GEORGE F. RODWELL.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Preparation of Subnitrate of Bismuth.—MM. Bechamp and C. St. Pierre recommend the following

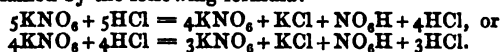
method of making subnitrate of bismuth (*Montpellier Medical*, April, 1866). They first make a neutral nitrate, by pouring nitric acid on powdered bismuth. The crystals of this neutral salt are drained, and then washed with a mixture of 1 part of acid and 3 of water. They are then dissolved in as small a quantity as possible; and the experiments of the authors have shown that a cubic centimetre of water, at 80°, containing 10 per cent. of nitric acid, is sufficient to dissolve 1 gramme of the salt. 12½ parts of water are now sufficient to precipitate a part of the crystallised nitrate so dissolved. The reaction is represented by the following formula:—



10 grammes of the crystallised nitrate should yield 6·3 grammes of the subnitrate, but only two-thirds of this quantity are obtained. The rest of the bismuth, which remains in the mother liquors in the state of subnitrate, and not as oxide, may be precipitated by carbonate of ammonia. The salt precipitated by water gives, on analysis, 16·98 of nitric acid, and 76·8 of oxide of bismuth. That precipitated by ammonia contains 80 per cent. of oxide of bismuth. The two products differ slightly. The second, when too much ammonia has not been used, has an evident crystalline texture, a faintly acid taste, and reddens litmus. The first is crystallised, and is slightly soluble in water; it reddens litmus strongly and has a decided acid taste. The authors add, that all subnitrate of bismuth should be rejected which is in the form of an amorphous powder, is without taste, and does not act on litmus. M. St. Pierre recommends that, to free bismuth from arsenic, from 2·5 to 5 per cent. zinc should be added, and the whole heated strongly for an hour, a piece of charcoal being placed in the crucible to prevent the oxidation of the zinc. After this treatment the bismuth will contain neither arsenic nor zinc.

New Use for Persulphate of Iron.—M. Bacco employs persulphate of iron instead of nitric acid in Bunsen's battery, and so, it is said, has a constant power and no disagreeable smell. (*Repertoire de Chimie pure et Appliquée*, liv. xi p. 354, 1866.)

Reactions of Soda and Potash with Nitric and Hydrochloric Acids.—The chlorides of the alkalis, according to Von Baumhauer, are more easily changed into nitrates than the nitrates into chlorides. The reaction of equivalents of hydrochloric acid and nitrate of potash are explained by the following formula:—



The reactions of equivalents of hydrochloric acid and nitrate of soda are as follows:—



The residue of the decomposition is the same when either equivalents of hydrochloric acid and nitrate of soda and potash, or of nitric acid and chloride of potassium and sodium, are taken. The same results happen if a mixture of hydrochloric and nitric acids is made to act on equivalents of potash and soda. When one equivalent of nitric acid acts on several equivalents of chloride of potassium and sodium, the results obtained are the same: 5 equivalents of nitric acid give 4 equivalents of nitrate of potash, and 3 equivalents of nitric acid give 2 of nitrate of soda. In a mixture of chloride of potassium and sodium the former is first decomposed by nitric acid, and the latter is not acted upon until after the total decomposition of the potash salt. (*Repertoire de Chimie pure et Appliquée*, liv. xi p. 355, 1866.) [The entire paper will be found in the "Verhazlen en Mededelingen der Koninkzake Akademie von Waten-Schappen," vol. x. p. 26.]

Preservation of Chloride of Lime.—After quoting Dr. Hofmann's account of the spontaneous

decomposition of chloride of lime from the *CHEMICAL NEWS*, vol. ii. p. 243, M. Barreswil states (*Répertoire de Chimie pure et Appliquée*, liv. ii. p. 350) that he has found the chances of decomposition much lessened when the chloride has been submitted to great pressure. So treated, it becomes as compact and hard as a stone, and may be kept a long time, perhaps, without undergoing any change. The plan might be worth trying when the chloride is exported. When required for use it would be necessary to reduce it to powder again, but the author thinks the cost of this extra labour would be compensated for by the diminished cost of packing.

II. ORGANIC CHEMISTRY.

Crystals of Proteine in the Potato.—Cohn has observed (*Journal für Praktische Chemie*, bd. lxxx. s. 129) that the cortical cellules lying immediately under the corky envelope of the potato contained only a few starch granules, but have as well some transparent cubic crystals, which exhibit the same reactions as crystals of proteine compounds. The cellules seldom contain more than one crystal, which is in general attached to the primordial utricle, and is, more or less, enveloped by the liquid protoplasm. Dilute potash dissolves the crystals, while dilute acids transform them into globules, which appear to be coagulated. Boiling water does not alter their appearance, but coagulates them, and renders them insoluble in ammonia and acetic acid.

LABORATORY MEMORANDA.

Action of Iodine on Turpentine.—I do not find any mention of the consequence of adding iodine to turpentine in chemical works. If it has not been the subject of observation, I think that it might be worthy of the same. After the lapse of a few seconds from the time of adding a piece of iodine to turpentine, a most violent bubbling commences, developing heat sufficient to inflame the mixture, except cooled artificially, co-existent with the development of heat is a disengagement of white gaseous vapour; the result is, a dark-brown turbid liquid, still possessing the property of changing the colour of starched paper blue. After the mixture has been allowed to rest for a week or more, the liquid becomes perfectly clear, having deposited a dark-brown resinoid body capable of being melted by heat, but not inflaming readily, not soluble in a fresh quantity of turpentine, neither does it act upon starch as the solution still does. This solution produces a change in a soap liniment not observable when a mere solution of iodine is added, causing a beautiful white emulsion. The exact nature of the chemical change will probably be the formation of hydriodic acid, which dissolves in the spirit, together with the disengagement of carbonic acid, or, on being more fully investigated, the decomposition may be more complex.—JAMES DEARDEN.

On a New Form of Iodide of Potassium.—Although iodide of potassium when pure will endure a high temperature without undergoing decomposition, I find that when intimately mixed with about equal parts of ether, magnesia, silicic acid, baryta or fused boracic acid, and raised to a red heat in a crucible, copious fumes of iodine are given off, and that, if the contents of the crucible be afterwards dissolved in water and filtered (which is easy in the case of the magnesia or silicic acid), the liquid will contain an iodide of potassium having properties different from that of the ordinary iodide. The heat, I may as well mention, should be continued as long as any scent of iodine is given off, otherwise the decomposition will not be complete. This iodide differs chiefly from the ordinary iodide in the amount of precipitate produced by acetate of lead being less owing to the

smaller quantity of iodine contained in it; also in its being less easily crystallised. Bromide of potassium appears to undergo a similar decomposition under similar circumstances, as, when mixed with the above-mentioned substances, and subjected to heat, fumes of bromine are distinctly visible; but I have not as yet either separated the salt or examined it.—BERNARD PIFFARD.

Chemical Society.—The next meeting of this Society will take place on Thursday, January 17, at 8 p.m., when there will be papers read "On the Analysis of the Saline Water of Purton, near Swindon, North Wilts," by Dr. Noad, F.R.S., and "On the Electrolytic Test for Arsenic," by Professor Bloxam.

ANSWERS TO CORRESPONDENTS.

*. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

*. All Editorial Communications are to be addressed to Mr. CROOKES and Advertisements and Business Communications to the PUBLISHERS, GIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

J. Wright.—Dragon's Blood is a gum which can be obtained at any large drug warehouse in London. You might perhaps experience difficulty in obtaining it in the country. We do not know its lowest market price at present, a short time ago it fetched from £5 to £14 per cwt. according to quality.

A Druggist.—(1), (2), (3), in our next. (4). Berthelot's "Chimie Organique," reviewed in our last. The number can be obtained of Mr. Nutt, 270, Strand. It is in two large volumes and costs £1. (5). The discount varies with every house you go to. Oertling, of Store Street, Bedford Square, is the best person to apply to for accurate weights. Either grains or grammes may be used. We prefer the former. The reduction of one into the other is easily effected.

J. C. S.—(1). Salicylate of oxide of methyl is to be met with in the crude state in commerce under the name of Wintergreen oil (oil of *Gaultheria procumbens*). It may be obtained in the pure state by rectification, collecting that portion which distils at 435° Fahr.

F. M.—To clean iron, immerse the plate in dilute sulphuric acid for four or five minutes, then well scour with bran and water; transfer to a mixture of dilute sulphuric and hydrochloric acids, clean again with bran, and transfer to lime-water until required for use. This is the plan adopted by the tin plate makers.

Orieipin.—For several good receipts for making blacking, both liquid and paste, see Beasley's "Druggists' Receipt-book," 4th Edition, Pp. 314, 315. London: J. Churchill.

L. T. writes to ask if it is possible for a person of two or three-and-twenty, by a change of country or diet, to increase in stature. The only increase would be in breadth.

A. B. P.—We have scarcely yet decided whether to report them or not.

Books Received.—"London Medical Practice; its Sins and Short-comings." London: Simpkin, Marshall and Co. "On Coal Gas," by the Rev. W. B. Bowditch. London: Van Voorst. Braithwaite's "Half-yearly Retrospect of Medicine." London: Simpkin, Marshall and Co.

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THE CHEMICAL NEWS

VOL. III. No. 59.—January 19, 1861.

THE ADULTERATION OF FOOD ACT.

In another page will be found the Report of Dr. Letheby with respect to the working of the Adulteration of Food Act in the City. The number of applications which he has received, in his capacity of Public Analyst, to analyse articles of food has been only four, and of these four articles three were adulterated,—one specimen of bread being particularly bad, containing at most only 50 per cent. of wheaten flour. We are not told that any action was taken on his certificate, and the offending baker may still be manufacturing bread of a like quality. The only other sample of bread analysed is certified by the Doctor to be of pure wheaten flour. In both cases the names of the bakers are given. Now, we have no sort of objection to this, so far as regards the fraudulent one; but we would suggest that this practice is not advisable in the case of the supposed honest one, in the present state of the law, since it gives facilities to the rogue to obtain a certificate from an eminent analyst, to the effect that the goods he vends are genuine, whereas they may be the veriest trash in reality. All that is necessary to enable a man to obtain such a certificate is to place half-a-sovereign in a friend's hand, to request him to purchase a loaf, or a pound of coffee, or anything else, at his shop, and take it to a Public Analyst to be analysed. Of course this specimen will be genuine, and the party, at the small cost of half-a-guinea, will have the satisfaction of seeing his name and address published in all, or most of the newspapers, as a vendor of genuine and unadulterated articles of food. Some may urge that it is in the interest of the public, and only just to the honest trader that his name should be published, if it were only to encourage others. To the latter argument we have only to reply, that we do not see the necessity for rewarding a man simply because he is not a rascal, every man being expected to do his duty as a matter of course. As regards the public, what we have already said with respect to the manner in which such a certificate can be obtained settles their interest in the matter. Of course, supposing the sample to be bought by a trusty official, it would be a very different matter. A certificate of genuineness would, in that case, be a voucher to be relied upon, and it would be a benefit to the public to make known the place where bread could be purchased which was not a compound of bad wheaten flour, rice, and alum, combined with 44 per cent. of water. To publish the names and addresses of tradesmen, however, of whom the analyst knows nothing beyond the fact that a person brings him samples of food bought at their respective shops, and which he finds to be genuine, can only result in the public being misled, if they do not think of the manner in which it was brought about, which the generality are pretty certain not to do.

We have no sort of doubt that whatever the Act is capable of accomplishing Dr. Letheby will make it accomplish, but this will be very little indeed. The

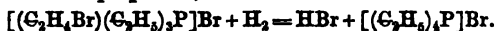
more conscientiously he labours, the more conspicuous will its weakness and inefficiency become. Timorous in its character, it betrays a fear of the trading-classes which we firmly believe to be unnecessary; for we do not think that anything like a majority of the shopkeepers sell adulterated goods, and only those who do would object to an Act which would really protect the public, and, at the same time, be beneficial to all honest tradesmen. We pointed out many of its weak points when it was in its embryo state, but it was said that there was no hope of getting anything stronger through the present House of Commons. We trust, however, that an attempt will be made during the next Session to put some vitality into it,—an attempt which would have the better chance of success now that Dr. Letheby has demonstrated that the Revenue is robbed, by adulteration in one article alone, to the extent of about £100,000 annually.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Polyatomic Derivatives of Ammonia,
by A. W. HOFMANN, Ph.D., F.R.S.

(Continued from page 19.)

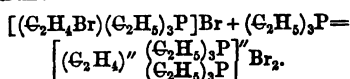
That the radical $\text{C}_2\text{H}_4\text{Br}$ may, with justice, be considered as monobrominated ethyl is fully proved by the two following facts:—First, when triethylphosphine is submitted to the action of brominated bromide of ethyl obtained by the action of bromine at a high temperature on bromide of ethyl, a brominated compound, absolutely identical with the one produced by the action of dibromide of ethylene on triethylphosphine, is formed. Secondly, when the bromethylated bromide is submitted to the action of hydrogen in the nascent state, the bromide of triethylphosphonium is obtained with all its properties,—



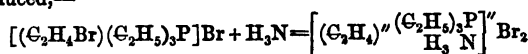
The following series is thus established:—

- Bromide of tetrethylphosphonium,
 $[(\text{C}_2\text{H}_4\text{H})(\text{C}_2\text{H}_5)_3\text{P}]\text{Br}$;
- Bromide of chlorethyl-triethylphosphonium,
 $[(\text{C}_2\text{H}_4\text{Cl})(\text{C}_2\text{H}_5)_3\text{P}]\text{Br}$;
- Bromide of bromethyl-triethylphosphonium,
 $[(\text{C}_2\text{H}_4\text{Br})(\text{C}_2\text{H}_5)_3\text{P}]\text{Br}$;
- Bromide of oxethyl-triethylphosphonium,
 $[(\text{C}_2\text{H}_4\text{HO})(\text{C}_2\text{H}_5)_3\text{P}]\text{Br}$.

When the bromide of bromethyl-triethylphosphonium is acted upon by triethylphosphine, the two substances combine, and give rise to the formation of the same diphosphonium salt which is produced directly by the action of dibromide of ethylene on triethylphosphine,—



If ammonia be substituted for triethylphosphine, dibromide of ethylene-triethylphosphammonium is produced,—



Again, the ammonia may be replaced by any primary, secondary, or tertiary monamine, when other more or less complicated bodies are obtained. Instead of monamines, monarsines may be employed; in this case, phospharsonium-compounds, containing at the same time both phosphorus and arsenic, are formed. Amongst others, the author has studied the following compounds:—

Dibromide of ethylene-trimethyl-triethyl-diphosphonium,

$$[(C_2H_4)''(C_2H_5)_3P]''Br_2;$$

Dibromide of ethylene-trimethyl-thiethyl-phosphammonium,

$$[(C_2H_4)''(C_2H_5)_3P]''Br_2;$$

Dibromide of ethylene-methyl-triethyl-phosphammonium,

$$[(C_2H_4)''(C_2H_5)_3P]''Br_2;$$

Dibromide of ethylene-tetethyl-phosphammonium,

$$[(C_2H_4)''(C_2H_5)_4P]''Br_2;$$

Dibromide of ethylene-pentethyl-phosphammonium,

$$[(C_2H_4)''(C_2H_5)_5P]''Br_2;$$

Dibromide of ethylene-hexethyl-diarsonium,

$$[(C_2H_4)''(C_2H_5)_6As_2]''Br_2;$$

Dibromide of ethylene-hexethyl-phospharsonium,

$$[(C_2H_4)''(C_2H_5)_6PAs]''Br_2;$$

Dibromide of ethylene-hexethyl-arsammonium,

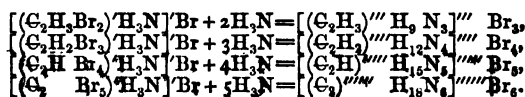
$$[(C_2H_4)''(C_2H_5)_6AsN]''Br_2.$$

What has been said with regard to the bromethylated bromide of the phosphorus series, applies with equal force to the corresponding salts in the arsenic and nitrogen groups.

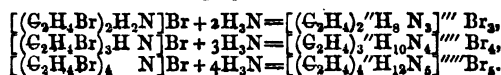
A monatomic ammonium-salt may therefore be converted into a diatomic salt by introducing into the former a chlorinated or brominated radical, the chlorine or bromine of which presents a point of attack for another molecule of ammonia, the fixation of the ammonia giving rise to the formation of a diammonium-compound. On this principle it is evident that by the introduction of several molecules of chlorine or bromine into the monammonium, several equivalents of ammonia may be associated to form a very extended series of polyammonium compounds.

The introduction of chlorine and bromine may be conceived to take place in two different ways. In the first place, one equivalent of hydrogen in the ammonium salt may be replaced by organic radicals,—ethyl, for instance, in which two, three, four, or five equivalents of bromine are substituted for the hydrogen. On submitting these bodies to the action of ammonia, two, three, four, or five equivalents of the latter may be fixed,

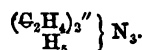
di-, tri-, tetra-, penta-, and hex-ammonium salts being formed in this manner.



Again, the accumulation of the bromine equivalents might be accomplished in a different manner. Several of the equivalents of hydrogen in the monammonium salt might be replaced by a monobrominated organic radical,—monobrominated ethyl for instance; under the influence of ammonia, these compounds also would be transformed into a series of polyatomic salts.



The author has already obtained a series of triammonium-compounds, of which the most interesting is diethylenetriamine.



On the Separation of Tellurium from Selenium and Sulphur, by A. OPPENHEIM, Ph.D.

No difficulty occurs in separating tellurium from sulphur. In fact, although they are frequently, in nature, found together, combined with bismuth, they are not isomorphous, and, although there is a simple relation between their combining numbers, there are but few analogies between their compounds, none of which are found to crystallise in the same system. On the other hand, certain compounds of tellurium with chlorine, iodine, and bromine are quite similar to the analogous compounds of antimony; both tellurium and antimony crystallise in the same system, and antimony precipitates tellurium from a solution of tellurous acid in hydrochloric acid, while tellurium again precipitates antimony from its perchloride. The combinations of tellurium with antimony and with arsenic have the appearance of alloys, and differ in their properties entirely from sulphurets. The diversity of tellurium and antimony is also demonstrated by the existence of fully crystallised double sulphurets of tellurium with alkalies; while, on the other hand, tellurium is not capable of replacing sulphur in sulphur salts. In a word, the analogies between sulphur and tellurium would, perhaps, not have been considered obvious, did not selenium form a connecting-link between the two, offering, in many respects, great similarities with the former, and, in other respects, with the latter element. Both selenium and tellurium are precipitated from tellurous and selenious acids, by means of sulphurous acid, protochloride of tin, metallic tin, and several other metals. The difficulties arising in the separation of tellurium, selenium, and sulphur consist, therefore, in the analogies between tellurium and selenium and between sulphuric and selenic acids. The usual way of separating selenium from tellurium is based upon the insolubility of seleniate of barytes. But the difficulties in preparing selenic and telluric acids, and the slight solubility of the tellurate of barytes, make this process a very tedious one. A mixture of seleniate and sulphurate of barytes are sometimes separated by reducing the former in a current of hydrogen and dissolving the selenide in hydrochloric acid. I tried, some time ago, to find an easier method of determining

these elements, based upon the different way in which they are acted upon by cyanide of potassium.¹ Sulphur is dissolved when fused with this compound and not precipitated by hydrochloric acid. Selenium—as Mr. Crookes was the first to observe—is likewise dissolved and precipitated as such by any acid. Tellurium does not only not form a tellurocyanide when fused with cyanide of potassium, but takes the place of the cyanogen therein, forming telluride of potassium, which dissolves in water, with a purple colour, and is speedily decomposed, by the action of the air, into potash and metallic tellurium. This process, however, is imperfect, on account of a slight loss, which was first ascribed to the volatility of the elements at the temperature employed. The loss, however, occurs chiefly on the side of tellurium, which, of the three elements, is the least volatilis; and lately, while repeating the experiments, I observed that it was owing principally to part of the tellurium being oxidised and dissolved as tellurate of potash. It also became evident that, instead of melting the elements with cyanide of potassium, it sufficed to digest them with a solution of the salt. Sulphur and selenium are thus completely dissolved. A small proportion of tellurium forms tellurite of potash, and the rest remains in the metallic state. The separation of the elements was, therefore, conducted in the following manner:—A mixture of them, reduced to a fine powder, was boiled with a solution of cyanide of potassium in a water-bath for about eight hours. Tellurium was then collected on a filter. Selenium was precipitated in the filtrate by means of hydrochloric acid, and the second filtrate was mixed with some sulphite of soda, heated, and allowed to stand for twenty-four hours. The portion of tellurium which had been dissolved as tellurite of potash was thus completely precipitated. It was then added to the other portion collected on the filter, dried in the water-bath, and weighed. Selenium was determined in a similar manner, whilst the quantity of sulphur present was indicated by the difference in weight between the quantities employed and obtained anew. In this manner results of sufficient exactitude were arrived at, as for instance:—

	Grms. Employed.	Grms. Obtained.	Grms. Calculated.
Tellurium	1.190	1.172	1.188
Selenium	0.270	0.271	—
Sulphur	0.128	—	0.129

The same method may be employed for separating selenium from metals not soluble in cyanide of potassium. But if iron, copper, or other metals are present, which form soluble compounds with the re-agent, partly precipitated by hydrochloric acid, it is necessary to dissolve the mixture of the elements in acids, and to add a quantity of sulphuret of ammonium, sufficient to dissolve the selenium and tellurium. They must then be precipitated from this solution as sulphurets, and treated with cyanide of potassium, after the manner described. When sulphuret of selenium is acted upon by cyanide of potassium, the selenium is first dissolved, and a residue of sulphur remains behind, which disappears but slowly. The red modification of selenium is dissolved more easily than the black. Selenious acid cannot be reduced by being boiled with cyanide of potassium.

On the Production of Ethylamine by Reactions of the Oxy-ethers,² by M. CAREY LEA.

WHILE engaged in making a series of experiments on this subject I met with the paper of Juncadella (*Rép. de Chimie pure*, t. i. p. 273) and the observations of De Clermont (*Ibid.* p. 374) on the same subject. Finding that the subject had less novelty than I supposed, I merely offer here one or two of the results which I have obtained.

Nitrate of ethyl, C_2H_5O, NO_3 , heated in sealed tubes with chloride of mercurammonium $\left. \begin{matrix} H_3 \\ Hg \end{matrix} \right\} NCl$ for many hours in the water-bath, did not appear to react upon it. Kept for some time in a boiling saturated solution of chloride of calcium the tubes, although extremely thick green-glass combustion-tube of small calibre was used, exploded with great violence, shattering the vessel in which they were contained, although they had been wrapped in strong cloth.

Nitrate of ethyl, heated in a sealed tube with chloride of zincammonium $\left. \begin{matrix} H_3 \\ Zn \end{matrix} \right\} NCl$ in the water-bath, does not appear to act upon it.

Nitrate of ethyl, heated in the water-bath in a sealed tube with carbamate of ammonia $NH_4O, NH_2C_2O_2$, dissolves the salt. On cooling, radiated crystals form. The contents of the tube evaporated to dryness with excess of chlorhydric acid, and then exhausted with ether, to which a few drops of strong alcohol have been added, yielded a solution which gave a chamois-coloured precipitate with bichloride of platinum, consisting of chloroplatinate of ethylamine.

1185 gm. substance gave .0465 metallic platinum corresponding to 39.25 per cent; theory requires 39.29.

The product was but small. Probably portions remained undissolved by the ether. No doubt portions of di- and tri-ethylamine are also formed in the above reaction, in the same manner as in those of the halogen ethers with ammonia.

An Account of Guy's Hospital Well, by
WILLIAM ODLING, M.B., F.R.S.

IN 1858, it was determined to sink a deep well at Guy's Hospital, which should yield a quantity of excellent water, sufficient to supply the entire wants of the institution. The well, with its appurtenances, was planned and constructed by Mr. R. W. Mylne, C.E., F.R.S. It was commenced in the winter of 1858, and completed in the summer of the following year. Its total depth from the surface of the yard is 297½ feet; its depth down to the chalk, 197 feet; and, consequently, its depth into the chalk, 100½ feet. The level of the yard is two feet above Trinity high-water mark. The following table gives an enumeration of the principal strata gone through, with their respective thicknesses, in feet:—

	Feet.
Made ground	12
Peat	2
Gravel	24
London clay	63
Red and yellow mottled clay	24
Blue shelly clay	7
Red mottled clay	10
Sand and clay, with pebbles	15
Gray sand	44
Chalk	100½
	297½

¹ Read before the American Association for the Advancement of Science, at Newport, August, 1860.

² *Journal für Praktische Chemie*, vol. lxxi.

The water supplied by the well is bright and colourless to the eye, brisk and pleasant to the taste, and has a marked alkaline reaction to test-paper. Its temperature, when fresh from the well, was found to be 54° , that of the atmosphere being 65.5° F. When boiled, the water becomes slightly turbid, from the deposition of the carbonates of lime and magnesia, which the excess of carbonic acid, driven off by ebullition, formerly held in solution. When boiled down to half its bulk, the clear water filtered from the deposit does not afford any evidence of the presence of either lime or magnesia. It has still, however, a strong alkaline reaction, from the carbonate of soda which it contains. This filtered water, when slowly evaporated, yields a white, saline residue, in which crystals of chloride of sodium and sulphate and carbonate of soda may be readily distinguished by microscopic examination. The residue left by the evaporation of the water to dryness undergoes, when heated to redness, a slight charring, from the presence of a small proportion of organic matter, but does not evolve any ammoniacal vapour recognisable by delicate test-paper. In addition to the constituents already mentioned, the water was found to contain some potash-salt, minute quantities of phosphoric acid, iron, and alumina, and a scarcely-perceptible trace of nitric acid, but no ammonia. The specific gravity of the water at 60° F. is 1.00077 . Its total solid contents amount to 51.69 grains per gallon. This residue is composed of the following constituents, in the quantities indicated:—

Separate Constituents.	Grains per Gallon.
Chlorine	9.75
Sulphuric acid	9.24
Carbonic acid	6.90
Silicic acid	0.79
Lime	1.47
Magnesia	1.15
Soda	13.87
Sodium	6.32
Potash	0.66
Phosphate of iron and alumina	0.05
Organic matter	0.94
Total by summation	51.14
Total by direct experiment	51.69

A sufficient amount of sodium to combine with the whole of the chlorine was calculated as sodium, the remainder as soda. The phosphate of iron and alumina was determined as follows:—A portion of the water was evaporated to dryness, and the residue, after ignition at a low red heat to destroy the organic matter, treated with an excess of hydrochloric acid. This was expelled by evaporation, and the dried residue heated to about 250° , to separate the silica. The resulting mass was then treated with strong hydrochloric acid, and dissolved in hot water. A few drops of nitro-muriatic acid were next added to oxidise the iron, after which the solution was filtered. The filtrate was precipitated with a slight excess of ammonia, and the whole gently heated. After standing for a night, the pale ochrey precipitate was collected on a filter, washed, dissolved in hydrochloric acid, reprecipitated by ammonia, collected on a filter, washed, dried, ignited, and weighed. The ignited mass was then treated with a little carbonate of soda, to render it soluble, and dissolved in hydrochloric acid. Phosphoric acid, alumina, and sesquioxide of iron were detected in the resulting solution. There was more than sufficient iron and alumina present to carry down the whole of the phosphoric acid. The organic matter was

estimated by evaporating a portion of the water to dryness at 250° , then igniting the residue for some time at a low red heat, moistening it with water, treating the moist mass with carbonic acid for several hours, and then again drying at 250° . After this restoration of the carbonic acid, driven off from the lime and magnesia, the loss on ignition was considered to be organic matter. This method of determination, introduced by Dr. Clark, though not rigidly accurate, is, I believe, the best at present known. The other estimations were made in the usual manner, and do not call for any remark.

In the evaporated residue, the acid and basic constituents of the water seem to be combined with one another, somewhat in the manner indicated below:—

Combined Constituents.	Grains per Gallon.
Chloride of sodium	16.05
Carbonate of soda	12.36
Sulphate of soda	15.21
Sulphate of potash	1.33
Carbonate of lime	2.62
Carbonate of magnesia	2.37
Silica	0.79
Phosphate of iron and alumina	0.05
Organic matter	0.94
Total residue	51.72

It is observable, however, that the mode in which the different constituents are combined in the residue affords no information as to the mode in which they are combined in the water itself. It appears probable, indeed, that, in the water, every acid is united with every base, so as to form the greatest possible number of salts, the amount of each different salt formed being dependent conjointly upon the relative masses and affinities of its constituents. Even the arrangement of the different constituents of the residue, to form definite salts, is, to some extent, a conventional affair. Thus, in the above statement of results, the silica is placed apart, in accordance with ordinary custom. It was ascertained, however, that in the dried residue of the water, and still more in the considerably evaporated water, a portion of the silica existed in the state of silicate of soda, and, doubtless, the silica in the natural water was also present in that form. The magnesia, again, is set down as a mono-carbonate; whereas, neither in the water nor the residue does it really exist in that state, but in the former as bicarbonate, and in the latter as three-fourths carbonate. The water is but sparingly aerated. The total quantity of carbonic acid in a gallon was found to be, as a mean of two concordant determinations, 13.98 grains. Deducting the 6.90 grains of fixed carbonic acid, we have an excess of 7.08 grains, or just sufficient to convert the neutral carbonates into the state of bicarbonates, so that practically the water contains no free carbonic acid. There were also present in a gallon of the water 5.49 cubic inches of nitrogen, and 0.91 cubic inches of oxygen gas, measured at mean temperature and pressure. The low ratio of the oxygen to the nitrogen is probably due to a portion of the originally-dissolved oxygen having been expended in oxidation, during the percolation of the water through the earth. The water of which the above-described analysis was made was taken from the well on May 16th, 1860. Another sample was taken on July 23rd, 1860. The total residue in this second sample was found to be, as a mean of two experiments, 50.84 grains,¹ including 0.7

¹ Dr. Taylor examined the water in October, 1859, and found it to yield 47 grains per gallon of fixed residue.

grain of combustible matter. In the interval between the taking of the samples there had been more than 7½ inches of rainfall, which may possibly have had to do with the slight diminution in the amount of solid contents yielded by the water.

In the following table, the analytical results obtained with Guy's Hospital water are compared with those obtained with some other deep-well waters in London, to which allusion is about to be made:—

Constituents.	I.	II.	III.	IV.	V.	VI.
Chloride of sodium	16·05	12·7	19·04	10·53	13·92	14·30
Carbonate of soda	12·36	11·6	11·37	8·63	10·92	6·73
Sulphate of soda	15·21	24·25	20·27	13·14	10·75	15·69
Sulphate of potash	1·33	—	1·33	—	2·10	1·92
Carbonate of lime	2·62	6·18	2·74	3·50	4·14	5·39
Carbte. of magnesia	2·37	1·08	2·07	1·50	2·98	3·20
Silica	0·79	0·44	0·40	0·50	0·70	0·60
Phosphate of iron and alumina	0·05	0·43	0·97	traces	traces	traces
Organic matter	0·94	—	0·66	—	—	—
Total grs. per galln.	51·72	56·80	58·85	37·80	45·41	47·83

No. I. is my analysis of Guy's Hospital water.

No. II. is Mr. Graham's analysis of the water from Messrs. Combe and Delafield's well in Long Acre. In his analysis, the phosphoric acid was considered to be in combination with both iron and lime; so that the 0·43 grain, in the table, should be set down to phosphate of iron and lime, instead of to phosphate of iron and alumina.

No. III. is Mr. Dugald Campbell's analysis of the water of the Orange Street well, from which the fountains in Trafalgar Square are supplied. I have made a trifling alteration in the arrangement of his results, so as to make them comparable with my own.

No. IV. is Mr. Brande's analysis of the water of the Mint.

No. V. is my analysis of the chalk spring water, and

No. VI. my analysis of the sand spring water, from a deep well at Mr. Burnett's Distillery, in Vauxhall.

(To be continued.)

TECHNICAL CHEMISTRY.

On the Alloys of Copper and Zinc,
by FRANK H. STORER.

(Continued from page 24.)

This alloy readily admits of being rolled, either hot or cold, and may be subjected to the operations of hammering or drawing without detriment, while alloys containing only a few per cent. more copper can be rolled hot only when the sheets are raised to a very high temperature. Even then it is exceedingly difficult to obtain thin sheets without cracking their edges.

On the other hand, alloys containing somewhat less than 60 per cent. of copper cannot well be rolled hot, since, when the ingots are heated, the exterior—especially at the corners—is liable to become pasty before the centre is soft enough to admit of being rolled.

In the preparation of the alloy of 60 per cent. of copper, now so extensively used for sheathing vessels, under the name of Muntz's sheathing or yellow-metal, —also known as malleable brass,—it is the custom of founders to reserve a portion of the zinc which has been

weighed out for a charge, until the alloy in their pots or furnace has become sufficiently hot; the last portions of zinc are then added in small pieces, a sample of the alloy being tested after each such addition. This is done by dipping out a small portion of the melted mass and pouring it into a mould; a little ingot, five or six inches long by an inch or less in thickness, is thus obtained, which, after cooling, is broken on an anvil, and its fracture observed. If this does not exhibit a smooth and homogeneous surface, more zinc is added to the alloy. The accuracy with which an experienced workman can thus obtain the desired alloy is truly astonishing, the more especially since this homogeneous alloy is confined within very narrow limits.

It is stated by founders that the alloy of 60 per cent. of copper and 40 per cent. of zinc will present almost precisely the same homogeneity of fracture, whether the test ingot prepared from it be cooled slowly, by exposure to the air, or rapidly, by plunging it into cold water, while alloys containing either more or less than 60 per cent. of copper are liable to assume different structures, according as they are cooled with greater or less rapidity. Two ingots are, therefore, sometimes cast on each trial of the alloy, one of which is cooled in water and the other in air, for comparison. This double test is, however, deemed superfluous by skilful workmen.

I may remark, in this connection, that I have repeatedly obtained crystals, by the method of partial cooling, from portions of melted yellow-metal taken from the founder's pots at the moment it had afforded them a satisfactory test. These crystals are in no wise different from those obtained by myself from alloys of almost identical composition.

Although, as has been stated, the tendency to form fibres seems to have ceased at the alloy containing 60 per cent. of copper, I cannot but think that the limit of its influence is less clearly defined than the "test" of yellow-metal founders would seem to indicate. In the circumstances under which this test is applied, it is doubtless true that no fibres are formed; but it is a matter of experience with manufacturers of yellow-metal, that the texture of the large ingots from which the sheets of sheathing are rolled is no longer so homogeneous as that of the small test ingot; they affirm also that this texture may vary greatly, according to the conditions in which the ingot is allowed to cool. It is evident, therefore, that during the processes of hot and cold rolling, and of annealing, to which the alloy is subsequently subjected, its texture may undergo various changes; while it is certain that the comparative durability of the sheathing, when exposed to the action of sea-water, must in great measure depend upon its relative compactness. If it be open and porous, as would be the case if a trace of the fibrous structure were present, it is clear that the sheathing would soon be destroyed,—not only because the salt water would come in contact with its interior portions, but also since the individual crystalline fibres of the alloy would, doubtless, resist its action more completely than the amorphous matter attached to them or contained in their interstices; from this a galvanic action would be produced, which could not fail to promote the corrosion of the alloy.

¹ The temperature to which the alloy is brought before pouring is considered to be a point of much importance by founders. In their opinion, a very inferior alloy would be formed if it were not heated much more strongly than would be necessary to maintain it in the liquid state. Compare Bolley, *Zur Kenntnis der Moleculareigenschaften des Zinks*, Ann. Ch. u. Pharm., xcv. 302. This subject is evidently connected with the phenomena of tempering described in another part of this memoir.

One of the most common complaints against yellow-metal arises from a tendency which some specimens of it exhibit to become so friable, after an exposure of longer or shorter duration to sea-water, that the sheets may readily be broken in pieces, sometimes even between the fingers. Attention has recently been called to this subject by Bobierre (*Thèses présentées à la Faculté des Sciences de Paris. "Thèse de Physique: Des Phénomènes électro-chimiques qui caractérisent l'Altération, à la Mer, des Alliages employés pour doubler les Navires,"* p. 61. Nantes: Imp. Busseuil. 1858,) who would refer such cases more particularly to chemical conditions depending, as he thinks, upon the too great proportion of zinc which is used in the preparation of the alloy, yellow-metal, as well as to the changes of composition produced by hot-rolling.

I cannot agree with this conclusion. In my own opinion, the other alternative which Bobierre has suggested, namely, peculiar arrangement of the molecules of which the alloy is composed, furnishes the true explanation of the difficulty. It is, however, possible that at times, when the temperature of the reverberatory, in which the alloy is heated before passing to the rollers, is not properly regulated, zinc may be burned off from the exterior portions of the sheet, and that the alloy, richer in copper, which would thus be formed, may subsequently be pressed into the body of the sheet during the operation of rolling. An alloy destitute of homogeneity would result from this treatment, which could hardly be durable in any event. Instances of this sort must nevertheless be rare, for no part of the process is watched by the manufacturers more scrupulously than this.

It must also be borne in mind, that, of the enormous quantity of yellow-metal which is now used by the merchant vessels of Great Britain and of this country,—all of which is composed of 60 parts of copper and 40 parts of zinc, and rolled hot,—only a comparatively small portion passes into the friable condition to which I have alluded.

In most cases the alteration which the sheathing undergoes is gradual and regular, and the portion which remains after the wear of several years is still malleable. Indeed, the absolute amount of sheathing which becomes friable is entirely out of proportion with the annoyance to which it subjects ship-owners;¹ for it rarely happens, even in the worst instances, that more than one-third of the sheets upon a vessel become friable, the remainder being in good condition.

The friability is, therefore, a purely accidental occurrence, and by no means a necessary consequence either of hot-rolling or of the presence of 40 per cent. of zinc in the alloy, as has been implied by Bobierre (*Thèses*, p. 77). I am strongly of opinion that it might be in every instance entirely obviated by methodically annealing or tempering the sheets of alloy in such a manner that no fibres could form in them, and that their structure should be homogeneous.

Bobierre, in his very able thesis, to which I have already alluded, has urged that it would be well to discontinue the use of the alloy containing 60 per cent.

of copper, which admits of being rolled hot, and to substitute for it sheathing prepared from alloys containing about 66 per cent. of copper, which can only be prepared by a most laborious process of cold rolling. It is true that the fibres which I have described would in this case probably never be encountered. Still, there are several important objections to the proposition of M. Bobierre. Not only would the method of cold-rolling consume a much greater amount of time and labour, but it would be exceedingly difficult, if not impossible, to procure any alloy the composition of which could be maintained so nearly constant as is the case with yellow-metal.² It is not probable that a test like the one applied to this alloy could be found anywhere else in the whole series.

It is frequently stated in chemical text-books that yellow-metal is always prepared from "best-selected" copper, and one is led to infer that a metal of peculiar purity is alone used in its manufacture. This may once have been the case; but since the immense increase in the use of this alloy, it would no longer be practicable to obtain a sufficient quantity of copper of uniform character, or, indeed, of any one kind, from which to prepare the large quantity of sheathing which is used. In an establishment where expense would be a secondary consideration, as, for example, in a Government workshop, it might still be possible to prepare an alloy constantly from the same sorts of copper and of zinc, in which case, although the amount of zinc lost would probably be subject to considerable variations, one might, nevertheless, soon be able to control the process, and to prepare an alloy of the composition proposed by Bobierre, so that only trifling variations should occur in the composition of the product. But in ordinary practice manufacturers are compelled to make use of the most varied kinds of copper, not only because the supply of the best sorts is limited, but particularly from the fact, that, owing to the competition which exists between the various foundries,—or, rather, by force of the laws which regulate supply and demand,—they are obliged to re-melt larger quantities of old copper sheathing,³ the origin of which is unknown to them, and which may have been originally prepared from copper of inferior quality.

(To be continued.)

The Adulteration of Food Act in the City.

THE following extracts are taken from Dr. H. Letheby's "Quarterly Report on the Sanitary Condition of the City of London:"—

In the matter of this Act, I have had but four applications for analysis. These are one for milk, two for bread, and one for mustard. The milk was delivered in the ordinary way to a householder in Martin's Lane, Cannon Street, and it was adulterated with water. One of the samples of bread was said to have been purchased of Mr. Negus, 158, Bishopsgate Street: it was adulterated with rice and alum, and contained about 44 per cent. of water. The other sample

¹ Whenever the destruction of a portion of the sheathing requires that a ship be hauled up for repairs, it is customary, since this operation is an expensive one, to re-sheath the vessel entirely; for, as the friable sheets are interspersed among the others, and since all the sheets are somewhat worn, it would be bad economy to attempt any partial repairs. Of course the owner of the vessel regards the entire suit of sheathing as being worn out, and forms his opinion of the usability of yellow-metal in accordance with this view.

² An alloy of constant composition might, however, possibly be prepared, by adding known quantities of melted copper to determined volumes of molten yellow-metal which had been prepared by the usual method.

³ The popular notion that a better—"more compact"—product is obtained by re-melting any alloy, may also have some connection with this custom.

was said to have been purchased of Mr. Clunie, at No. 4, Finsbury Pavement: it was genuine bread, of good wheaten flour. The sample of mustard was said to have been obtained at Mr. Colman's, in Cannon Street; it consisted of mustard meal, wheaten flour, and turmeric; but considering that the proportion of wheaten flour was not excessively large, and that it is a common and necessary practice to dilute the meal of mustard, in order that it may be used as an agreeable condiment, I do not think that the mixture was an adulteration, and therefore I certified that it did not contain anything injurious to health.

I am very anxious that the practical working of this Act should be of advantage to the public, and should be brought fully and fairly into operation within the City. This, however, must be effected by the energy of the citizens themselves; for, however desirable it may be to expose the frauds and dangers of adulteration, the Act has not contemplated that the exposure should be brought about by the offices of the common informer. In every case, therefore, the public must initiate the inquiry by a fair and open purchase of the article suspected to be adulterated; and as far as my duties are concerned, as the Analyst appointed for the City, I shall endeavour so to conduct every analytical investigation, as, on the one hand to expose fraud, and on the other to protect the interests of the honest dealer. Whether it may be thought desirable, hereafter, to undertake an extensive series of systematic examinations of the various articles of food and drink, for the purpose of exposing the fraudulent practices of trade, is a matter that will require serious consideration. At present, however, the effectual working of the Act must rest entirely with the public; and with the view of indicating the right mode of proceeding, I have reproduced in the Appendix an abstract from my recent report, on the practical working of the Act.

Of the many classes of adulteration that deserve attention, there are none so serious in their consequences as those which affect the stamina and health of the people. The adulteration of bread, for example, with materials that lessen its nutritive value, is not merely a fraud on the pocket, but is also a fraud on the constitution of the consumer; for that which diminishes the flesh-forming constituents of so important an article of diet, must necessarily be a means of impairing the growth of the animal fabric, and of weakening the force of labour.

In some cases of adulteration, the practice of it has a far more extensive relation to the affairs of the community than at first sight appears. To take the sophistication of beer and porter as an instance: it is well known that the publicans, almost without exception, reduce their liquors with water after they are received from the brewer. The proportion in which this is added to the beer at the better class of houses is nine gallons per puncheon, and in second-rate establishments the quantity of water is doubled. This must be compensated for by the addition of ingredients which give the appearance of strength, and a mixture is openly sold for this purpose. The composition of it varies in different cases; for each expert has his own particular nostrum. The chief ingredients, however, are a saccharine body, as foots and liquorice to sweeten it; a bitter principle, as gentian, quassia, sumach, and terra japonica, to give astringency; a thickening material, as linseed, to give body; a colouring matter, as burnt sugar, to darken it; cocculus indicus, to give a false strength; and common salt, capsicum, copperas, and Dantzic spruce to produce

a head, as well as to impart certain refinements of flavour. In the case of ale, its apparent strength is restored with bitters and sugar-candy. Now, apart from the question of how much injury is done to the health of the consumer by this practice, or how large is the fraud on the pocket of the purchaser, it is a matter of interest to know what is the effect of it on the revenue. There are, I believe, about 6200 publicans and beer retailers in this metropolis. Supposing that each, on an average, has a sale for only six barrels of beer and porter per week, and that the degree of dilution is but to the extent of five gallons of water per barrel, in the aggregate there would have been used 186,000 gallons of water per week. This, if properly and honestly brewed into beer, would have required about 1722 quarters of malt; and as each quarter of malt pays a duty of 22s. 6d. to the revenue, there is a fraud of rather more than £1937 sterling per week, or nearly £100,800 per annum. In like manner almost every such fraudulent practice of trade has a wider relationship to the public interests than the mere act itself would imply. I will not, however, pursue this further at present; and, in leaving it, I venture to express a hope, that as the public interests are so largely concerned in the general application of this Act of Parliament, its execution will be faithful and vigorous.

Abstract of the Report on the Practical Working of the Act.—According to the provisions of the Act it is necessary that the different states of the investigation shall be conducted in a systematic manner.

First. In respect of the Purchasing of the Article.—In all cases the purchaser of an article suspected to be adulterated, shall, directly he has purchased it, and before he removes it from the shop, give notice to the dealer, or the person serving him, that he intends to have the article analysed; so that the dealer may have the opportunity of accompanying the purchaser to the analyst, or of securing the article in such a manner as to prevent it from being tampered with. This will be necessary, in order that proof may be furnished of the identity of the article examined.

Second. As regards the Analysis.—Wherever an analyst is appointed, any purchaser of an article of food or drink, shall be entitled, on payment to the analyst of a sum not less than two shillings and sixpence, nor more than ten shillings and sixpence, to have the article analysed, and to receive from him a certificate of the result thereof. Considering, however, that it is not easy or practicable to draw up a scale of charges for the various kinds of investigations contemplated by the Act, and that the fee named will rarely cover the expenses of the analysis, it will, I think, be expedient to fix an uniform charge, intermediate in the scale, say five shillings, for each analysis. This will prevent a great deal of complexity, and will, at the same time, show that the object of the charge is not to cover the expenses of the inquiry, but rather to guard the dealer against unnecessary annoyance and groundless complaint. The poor, however, who are the chief sufferers from adulteration, will, I fear, be unable to pay the fee named; and, unless something be done to assist them in the matter, they will be shut out, most completely, from the benefits of the Act. I would suggest, therefore, that you give to the analyst a discretionary power to conduct such inquiries for the poor without fee. It is necessary, however, that a complaint should first come to him from a poor person, and the complainant should

be entitled to leave with the analyst a sample of the suspected article, together with the name and address of the dealer; and if, in the judgment of the analyst, the matter is sufficiently serious to demand inquiry, he shall be at liberty to instruct the inspector to purchase a sample of the suspected article at the place named, and then to submit it to analysis free of charge.

Lastly. *In respect of the Certificate.*—I place before you the form of certificate, which, with your sanction, I will use. It is constructed so as to indicate the object of the inquiry, and to express the result of the analysis. It also expresses the opinion of the analyst—whether the article is adulterated or not, and whether the article is so adulterated as to be injurious to health. These indeed are the several points to which the Act has directed attention; and in giving expression to them, it is necessary on the one hand to be very explicit, so as to expose fraud, and on the other not so emphatic as to furnish a puff for the rivalries of trade.

PHARMACY, TOXICOLOGY, &c.

Remarks on Polygalic Acid, by WILLIAM PROCTER, Junr.

AMONG the questions proposed last year for investigation, the following was accepted by me:—

“What is the most eligible process for obtaining polygalic acid from senega? In what proportion does it exist in that root? And what are the forms for prescribing it as substitutes for the preparations of seneka?”

Seneka snake-root, as one of the most efficient American contributions to the “*Materia Medica*,” early attracted the attention of European pharmacists and chemists, and at least half-a-dozen analyses of it had been published more than twenty years ago, when it was very fully investigated by Quevenne,¹ in his inaugural essay sustained at the School of Pharmacy, at Paris.

According to this analysis, the constituents of seneka are as follows, viz.:—

Organic.	Inorganic.
Polygalic acid.	Carbonate, phosphate, and
Virgineic acid.	sulphate of potassa.
Pectic acid.	Chloride of potassium.
Tannic acid.	Carbonate, sulphate, and
Bitter yellow colouring matter.	phosphate of lime.
Gum, albumen, &c.	Alumina, magnesia, silica,
Cerin, and fixed oil.	and iron.

Polygalic acid, so called by Quevenne, was previously made known, in a less pure state, as *senegin*, by Gehlin. It is the active constituent of the root, although some influence may be due to the virgineic acid and colouring principle. It may also be noticed that both Dulong and Foenuille found malic acid in this root, united with lime and potassa.

It may not be out of place to state the characteristics of this substance, as it is certainly deserving of a better destiny than has thus far been accorded to it. Polygalic acid is a white, amorphous powder, without any tendency to crystallise, and is unalterable in the air. It is inodorous when pure, but is apt to retain traces of virgineic acid. Its taste, at first but slight, soon becomes pungent and acrid, and produces a painful constriction of the throat. When its dust is inhaled through the nostrils, irritation and violent sneezing occur. It is not

volatile; when burnt in a tube it affords no nitrogen, and is wholly combustible. Polygalic dissolves slowly in cold water, but readily in hot water, forming a solution which reddens litmus-paper, and froths strongly by agitation. It is soluble in boiling alcohol, but the larger part separates on cooling. It is soluble in diluted alcohol, but the solution has a tendency to deposit it on standing. It is absolutely insoluble in ether, and in acetic ether, the fixed and volatile oils. It is soluble in alkaline solutions, with the production of a greenish-yellow coloration. It is precipitated in its free state by subacetate of lead, and proto-nitrate of mercury, but by no other salts; various salts throw it down from solution when it is combined with an excess of potassa or soda. When heated with nitric acid it affords oxalic and picric acids, and sulphuric acid decomposes it with a rose and violet colour. The acid character of this substance is so slight that it will not decompose the carbonates, yet it forms amorphous salts, with alkaline and metallic bases. Quevenne suspected a close relationship between polygalic acid and saponin. More recently,² M. Bolley has resumed the inquiry, and believes that both saponin and polygalic acid, are glucosides, like salicin, resolved by hydrochloric acid into sugar and their derivative body *sapogenin*, and that their formula is the same.

Preparation of Polygalic Acid.—The particular object of this paper is the preparation and administration of this substance. The original process of Quevenne required precipitation by acetate of lead, and the action of sulphuretted hydrogen; but he afterwards greatly simplified the process, but retained some complications not necessary to obtain the acid sufficiently pure for medicinal use. The following process I have tried several times; and it has the merit of great simplicity:—

Take of senega, in powder (50 meshes to the inch) 10,000 grains; alcohol, ether, and water, of each a sufficient quantity. Moisten the senega with half-a-pint of a mixture of two parts of alcohol and one part of water, pack it in a large glass funnel; cover the surface with a piece of muslin, or paper, and pour on the same liquid until three pints have slowly passed by drops, or until the root is nearly exhausted. Evaporate this tincture to 12 fluid ounces, which removes nearly the whole of the alcohol, and then agitate it with repeated portions of ether, until the latter ceases to acquire colour, decanting each portion before adding the next, which requires about a pint and a-half. The syrupy liquid is now mixed with a pint and a-half of strong alcohol, and a half-pint of ether, well agitated several times, and set aside to repose. The polygalic acid being nearly insoluble in this menstruum, precipitates as a bulky, light, fawn-coloured precipitate. If on adding a small portion of the clear liquid to a similar mixture of ether and alcohol, no precipitate occurs, the whole of the polygalic acid capable of being thus thrown down has been separated. The supernatant liquid is now carefully decanted from the precipitate, which is thrown on to a filter and carefully washed with a mixture of two parts of alcohol and one of ether, till the adhering coloured liquid has all been displaced. The filter is then spread on an absorbent surface of paper, pressed strongly, the mass broken into small fragments, dried, and powdered. The resulting powder weighs 550 grains, 5½ per cent. of the weight of the senega; making allowance for wastage, senega may be said to contain 1/16th of its weight of this substance, and, consequently, 30 grains will equal an ounce of the root.

¹ Journ. de Pharm., 1836, p. 449.

² Annal. der Chemie, and Amer. Journ. of Pharm., vol. xxvii, p. 43.

In this state polygalic acid is not pure, but is sufficiently so for all medical purposes. It retains traces of colouring matter, and of some substances which are separated by the ether, but in very small amount. By re-dissolving it in hot water, and precipitating with the mixture of ether and alcohol, and afterwards dissolving it in hot alcohol, with animal charcoal, and filtering hot, it is obtained quite white.

The ethereal washing liquids first obtained were evaporated till no ether remained. The residue consisted of a dark amber-coloured fixed oil, admixed with crystals of virgineic acid, which formed a crust on its surface, and a dense aqueous solution of the same acid, which strongly affected litmus-paper.

The liquid from which the polygalic acid precipitated was allowed to stand a month, when the sides and bottom of the vessel were spangled with a crystalline matter, in small quantity. This substance, is soluble in water, when heated; less soluble in alcohol; gritty between the teeth; has no taste of senega; fuses by heat, blackens, and is almost entirely consumed. Concentrated SO_2 dissolves it with heat without the evolution of either ammonia or HCl . Its aqueous solution is not precipitated by either chloride of barium or nitrate of silver. A crystal placed on moistened litmus-paper produced a reddish spot. It is possible that this substance is an acid salt of malic or some analogous acid. The small quantity of ash it leaves is alkaline. The investigation was not pushed further.

The liquid, from which the crystals deposited, by evaporation, yielded an extract tasting strongly of senega.

As regards the pharmaceutical applications of polygalic acid, several experiments were made:—

1. Five grains of the acid in a fluid ounce of water, kept in a vial for two months, acquired a fetid odour, whilst a portion of matter precipitated, yet the taste of the solution was strongly that of senega.

2. Five grains of the acid were dissolved in an ounce of cold water, with five grains of bicarbonate of potassa, and kept the same length of time. This solution also spoiled, but retained its senega taste.

The best form of prescribing polygalic acid, in liquid form, is with syrup of gum arabic, to blunt its acrimony. It is necessary to dissolve it in a little hot water, and add it warm to the syrup of gum, and mix them. It may also be added to syrup of ipecacuanha, to increase its expectorant power. But the form best adapted to administer this principle is in pills and powders. In doses of one or two grains (which represent 15 to 30 grains of senega), the full effect of a tablespoonful and a fluid ounce of decoction of senega is obtained. Where a more gentle effect is desired, the dose must, of course be greatly reduced,—to $\frac{1}{4}$ or $\frac{1}{2}$ of a grain,—and can be associated with ipecacuanha, tartar emetic, and other medicine with which senega is given. When the form of powders is adopted, a mixture of powdered gum and sugar should always be added, to blunt the acrimony of the polygalic acid. Sugar of milk triturated with the acid, in the proportion of one grain to nine grains, is an eligible form for use.

In cough mixtures, when not contra-indicated or incompatible, the addition of an equal weight of bicarbonate of potassa facilitates its solution. It is not advisable to keep a permanent liquid preparation of polygalic acid, leaving that form for the official syrup, but it undoubtedly offers an admirable means of using senega in the forms above indicated, when the physician

can regulate the dose and associate it with other remedies in the nicest manner.—*Proceedings of the American Pharm. Association, 1859.*

Lead in the Calcareous Deposits of Kettles, by HENRY OSBORN, M.D.

IN making an examination of water for the presence of lead, it is seldom requisite to reduce the water by evaporation, but simply to test it by means of the ordinary tests, and should there be no indication of lead, we infer that the water is free from the poison. Whenever water is capable of acting upon lead, we generally find the earthy matter, which is held in solution, so small that no concretion takes place in the kettles or boilers; and the carbonic acid, which the water contains, exists in a free state, or nearly so. Until a very recent period I was induced to believe that whenever the carbonic acid held carbonate of lime in solution it was incapable of taking up the lead. I was aware, however, of the liability of such water to corrode the metal into holes, even when a sufficiency of sulphates, &c., were present to act as protecting agents. In carrying my investigations a step further, I found, by evaporating to dryness one gallon of water (which, on previously testing, did not appear to contain lead) and analysing the residue, a small quantity of lead was present. I then, on examining the calcareous deposits taken from a number of kettles and boilers, by treating the earthy matter with acetic acid, and found an appreciable quantity of carbonate of lead to exist in a few grains of the crust. The proportion, however, varied in different samples, owing to the length of the one-inch leaden pipes which transmitted the water from the main, and the length of time in which the water is permitted to rest in contact with the lead. The next point was to determine whether this accumulation of carbonate of lead, combined with carbonate of lime, organic matter, &c., was liable to be re-dissolved by boiling water in the encrusted kettles; but I failed to prove that the lead was re-dissolved, on applying the tests, without concentration. Hence, we may conclude that there is but little risk of being poisoned by such accumulations, though it is advisable to avoid the insoluble lead as much as possible, because the particles of the crustaceous matter may become detached by ebullition and suspended by the water. It is evident, then, that water may take up so small a quantity of lead as to escape detection by our most delicate colour-tests prior to evaporation; but, by boiling such water, the carbonic acid escapes, and minute quantities of carbonate of lead are daily thrown down, and may thus accumulate to a considerable extent.

The house in which I now reside is supplied with water from two sources,—one from a pump, and the other from the town water-works. The former is most delicious water, containing but a very small quantity of solid contents, and leaving no deposit in the kettles. Its free carbonic acid and purity, however, render it capable of forming carbonate of lead; and, if I were to permit its use for domestic purposes, my constitution would soon succumb to its enervating influence. The latter water, supplied from the town, is the water which yields the deposit now under consideration; but, as it passes through a small iron tube from the main, instead of lead, I find no insoluble carbonate of lead in the concrete matter in the kettles. Whilst engaged in making this investigation, my attention was drawn to a singular and simple method of preventing the incrustation from taking

place. I was informed that a schoolboy's marble, when placed in a kettle, would prevent the crust from forming, and the interior of the vessels never required cleansing. Being unable to account for the action of a marble in preventing such a formation, I was referred to a person who had kept a marble in her tea-kettle for five years. On inspection, I found a very little deposit, except in the spout of the vessel, which part only was thickly encrusted. On examining the marble, I found that it had increased in size, but the surface of it was perfectly smooth, and so hard that I could scarcely scrape it with a knife; but when cut it presented fine concentric layers, resembling the most compact calculus. When split asunder the marble was found in the centre, measuring three-quarters of an inch in diameter, and the concrete matter enclosing it, only one-eighth of an inch in thickness, which had been about five years forming. The interesting part of this formation is the resemblance which it bears to a calculus, and shows that an artificial calculus may be formed in a tea-kettle as well as in the bladder of the human body, though not composed of the same constituents.—*Medical Times*.

A Means of Detecting the Presence of Castor-oil in the Volatile Oils, by HARRY NAPIER DRAPER, F.C.S.L.

As a general rule, the volatile oils which are employed in pharmacy and perfumery are, at least in our markets, to be obtained pure; but it does occasionally happen that they are sophisticated with some fluid of low price. Turpentine of wood, alcohol, and the fixed oils are the most common adulterants. It is a very simple matter to detect alcohol, and the admixture of turpentine so much deteriorates the proper odour of the oil that but slight experience is required for its recognition. The insolubility of nearly all the fixed oils in alcohol, and the consequent facility of ascertaining their presence by simple treatment of the suspected sample with this re-agent is so well known, that this mode of adulteration is seldom attempted. Castor-oil forms, however, an exception to the general behaviour of this class of bodies in this respect. Soluble not only in alcohol, but in all menstrua which dissolve the volatile oils, its recognition by the means just pointed out would be impracticable, and its miscibility with alcohol can even be taken advantage of to introduce the latter into the volatile oil in such proportion as to imitate the natural fluidity of a pure specimen. Having recently had occasion to examine an oil which was suspected to be adulterated in this manner, it occurred to me that the reaction by which ricinus oil when treated with nitric acid gives rise to ceanthyllic acid ($C_{14}H_{13}O_3 + HO$) might be taken advantage of for its detection.

This body is a product of the oxidation of the castor-oil, and is formed when the warm oil is treated with an excess of nitric acid. A violent action ensues during which much nitrous acid is disengaged, and there is found floating in the acid liquid, when the residue is mixed with water, a soft, unctuous mass, which is, I believe, the palmino of Boudet, accompanied by fatty acids. If the acid liquid be now neutralised with carbonate of soda, so as to entirely remove the odour of nitrous acid, the smell of the ceanthyllic acid can be most clearly recognised. It is upon this fact that I have based the method of detecting the presence of castor-oil which is now to be described. The *modus operandi* is as follows:—Twenty drops of the suspected oil are placed in a porcelain capsule, and heated on a sand-

bath until the odour of the oil is no longer perceived. To the residue—if there be any—add five or six drops of nitric acid, and as soon as the action has subsided, dilute with a solution of carbonate of soda. If castor-oil be present, the odour of the ceanthyllic acid will now be at once perceived, and once smelled, is not likely to be mistaken for any other. As, however, it can hardly be compared to any other familiar odour, any one who has occasion to use this test will do well to make the experiment with castor-oil alone, and to acquaint himself with the peculiar smell.

In this way I have been able to detect the admixture of five per cent. of castor-oil in volatile oils, and have no doubt even a much smaller proportion could be recognised.—*Dublin Medical Press*.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures¹ (adapted to a Juvenile Auditory), on the Chemical History of a Candle; by M. FARADAY, D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.

LECTURE III. (Jan. 1, 1861.)

Products: Water from the Combustion—Nature of Water—A Compound—Hydrogen.

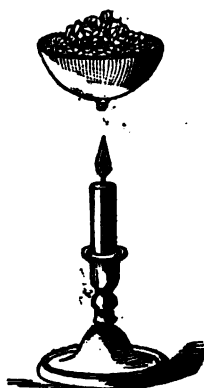
I dare say you well remember that when we parted we had just mentioned the word "products" from the candle. For when a candle burns we found we were able, by nice adjustment, to get various products from it. There was one substance which was not obtained when the candle was burning properly, which was charcoal or smoke, and there was some other substance that went upwards from the flame which did not appear as smoke, but took some other form and made part of that general current which, ascending from the candle upwards, becomes invisible and escapes. There were also other products to mention. You remember that in that rising current having its origin at the candle, we found that a part was condensable against a cold spoon, or against a clean plate, or any other cold thing, and part was incombustible.

We will first take the condensable part and examine it, and, strange to say, we find that that part of the product is just water—nothing but water. I last time spoke of it incidentally, merely saying that water was produced among the condensable products of the candle; but, to-day, I wish to draw your attention to water that we may examine it carefully, especially in relation to this subject, and also with respect to its general existence on the surface of the globe.

Now, having previously arranged an experiment for the purpose of condensing water from the products of the candle, my next point will be to show you this water; and perhaps one of the best means that I can adopt for showing its presence to so many at once, is to exhibit a very visible action of water, and then to apply that test to what is collected as a drop at the bottom of that vessel. I have here a chemical substance, discovered by Sir Humphry Davy, which has a very energetic action upon water, which I shall use as a test of the presence of water. If I take a little piece of it—it is called potassium, as coming from potash—if I take a little piece of it, and throw it in that basin, you see how it shows the presence of water by lighting up and floating about, burning. I am now going to take away the candle which has been burning underneath the vessel containing ice and salt, and you see a drop of water—a condensed product of the candle—hanging from the under surface of the dish. I will show you that potassium has the same action upon it as upon the water in that basin in the experiment we have just tried. See! it takes fire and burns in just the same manner. I will take another drop upon this glass slab, and when I put the potassium on to it you see at once, from its taking

¹ Reported verbatim by special permission.

fire, that there is water present. Now, that water was produced by the candle. In the same manner, if I put this spirit-



lamp under that jar, you will soon see the latter become damp from the dew which is deposited upon it—that dew being the result of combustion; and I have no doubt you will shortly see, by the drops of water which fall upon the paper below, that there is a good deal of water produced from the combustion of the lamp. I will let it remain, and you can afterwards see how much water has been collected. So, if I take a gas-lamp, and put any cooling arrangement over it, I shall get water,—water being likewise produced from the combustion of gas. Here, in this bottle, is a quantity of water—perfectly pure, distilled water, produced from the combustion of a gas-lamp—in no point different from the water that you distill from the river, or ocean, or spring, but exactly the same thing. Water is one individual thing, it never changes. We can add to it by careful adjustment, for a little while, or we can take it apart and get other things from it, but water, as water, remains always the same, either in a solid, liquid, or fluid state. Here again [holding another bottle] is some water produced by the combustion of an oil-lamp. A pint of oil, when burnt fairly and properly, produces rather more than a pint of water. Here, again, is some water, produced by a rather long experiment, from a wax candle. And so we can go on with almost all combustible substances, and find that if they burn with a flame, as a candle, they produce water. You may make these experiments yourselves; the head of a poker is a very good thing to try with, and if it remains cold long enough over the candle, you may get water condensed in drops on it; or a spoon, or ladle, or anything else may be used, provided it be clean, and can carry off the heat, and so condense the water.

And now—to go into the history of this wonderful production of water from combustibles, and by combustion,—I must first of all tell you that this water may exist in different conditions, and although you may now be acquainted with all its forms, they still require us to give a little attention to them for the present; so that we may perceive how the water, whilst it goes through its Protean changes, is entirely and absolutely the same thing, whether it is produced from a candle by combustion, or from the rivers or ocean.

First of all, water when at the coldest is ice. Now, we philosophers,—I hope that I may class you and myself together in this case,—speak of water as water, whether it be in its solid, or liquid, or gaseous state—we speak of it chemically as water. Water is a thing compounded of two substances, one of which we have derived from the candle; and the other, which we shall find elsewhere. Water may occur as ice; and you have had most excellent opportunities lately of seeing this. Ice changes back into water; and on our last Sabbath we had a strong instance of this change, by the sad catastrophe which occurred in our own house, as well as in the houses of many of you. Ice changes back into water when the temperature is raised: water also changes into steam when it is warmed enough. The water which we have here before us as ice, is in its densest state, and although it changes in weight, in condition, in form, and in many other qualities, it still is water; and whether we alter it into ice by cooling, or whether we change it into steam by heat, it increases in volume,—in the one case very strangely and powerfully, and in the other case very

largely, and strangely, and wonderfully. For instance, I will now take this tin cylinder, and pour a little water into it, and seeing how much water I pour in, you may easily estimate for yourselves how high it will rise in the vessel: it will cover the bottom about two inches. I am now about to convert the water into steam, for the purpose of showing to you the different volumes which water occupies in its different states of water and steam.

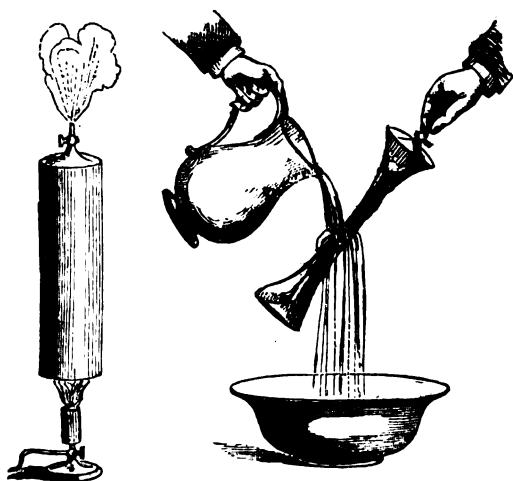
Let us now take the case of water changing into ice; we can effect that by cooling it in a mixture of salt and pounded ice, —and I shall do so to show you the expansion of water into a thing of larger bulk when it is so changed. These bottles [holding one] are made of strong cast-iron, very strong and very thick—I suppose they are the third of an inch in thickness; they are very carefully filled with water, so as to exclude all air, and then they are screwed down tight. We shall see that when we freeze the water in these iron vessels, they will not be able to hold the ice, and the expansion within them will break them in pieces as these [pointing to some fragments] are broken, which have been bottles of exactly the same kind. I am about to put these two bottles into that mixture of ice and salt, for the purpose of showing that when water becomes ice, it changes in volume in this extraordinary way.

In the meantime look at the change which has taken place in the water to which we have applied heat; it is losing its fluid state. You may tell this by two or three circumstances. I have covered this glass flask—in which water is boiling—over with a watch-glass. Do you see what happens? It rattles away like a valve chattering, because the steam rising from the boiling water sends the valve up and down, and forces itself out, and so makes it clatter. You can very easily perceive that that flask is quite full of steam, or else it would not force its way out. You see also that the flask contains a substance very much larger than the water, for it fills the whole of the flask over and over again, and there it is blowing away into the air; and yet you cannot observe any great change in the bulk of the water, which shows you that its change of bulk is very great when it becomes steam.

I have put our iron bottles containing water into this freezing mixture that you may see what happens. No communication will take place, you observe, between the water in the bottles and the ice in the outer vessel. But there will be a conveyance of heat from the one to the other, and if we are successful—we are making our experiment in very great haste—I expect you will by-and-by, so soon as the cold has taken possession of the bottles and their contents, hear a pop on the occasion of the bursting of the one bottle or the other, and, when we come to examine the bottles, we shall find their contents masses of ice partly enclosed by the covering of iron which is too small for them, because the ice is larger in bulk than the water. You know very well that ice floats upon water; if a boy falls through a hole into the water, he tries to get on the ice again to float him up. Why does the ice float? Think of that, and philosophise. Because the ice is larger than the quantity of water which can produce it, and therefore the ice weighs the lightest and the water is the heaviest.

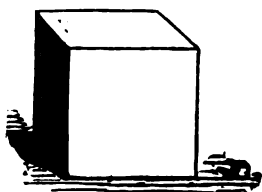
To return now to the action of heat on water. See what a stream of vapour is issuing from this tin vessel. You observe, we must have made it quite full of steam to have it sent out in that great quantity. And now, as we can convert the water into steam by heat, we convert it back into liquid water by the application of cold. And if we take a glass, or any other cold thing, and hold it over this steam, see how soon it gets damp with water; it will condense it until the glass is warm—if condenses the water which is now running down the sides of it. I have here another experiment to show the condensation of water from a vaporous state back into a liquid state, in the same way as the vapour, one of the products of the candle, was condensed against the bottom of the dish and obtained in the form of water; and to show you how truly and thoroughly these changes take place, I shall take this tin flask, which is now full of steam, and I shall close the top. We shall see what takes place when we cause this water or steam to return back to the fluid state by pouring some cold water on the outside. [The Lecturer poured the cold water over the vessel, when it immediately collapsed.] You see what has happened. If I had closed the stopper and still kept the heat applied to it, it would have burst the vessel; yet, when then the steam returns to water, the vessel collapses, there being a vacuum produced inside by the condensation of the steam. I show you these

changes for the purpose of pointing out that in all these occurrences there is nothing that changes the water into



another thing; it still remains water, and so the vessel is obliged to give way and is blown inwards, as in the other case, by the further application of heat, it would have been blown outwards.

And what do you think the bulk of that water is when it assumes the vaporous condition? You see that cube [pointing];



it is a cubic foot. There, by its side is a cubic inch: it is square, exactly the same shape as the cubic foot, and that bulk of water [the cubic inch] will make that bulk [the cubic foot] of steam, and the application of cold will contract that large quantity of steam into that small quantity of water. [One of the iron bottles burst at that moment.] Ah! There is one of our bottles burst, and here you see is a crack down one side an eighth of an inch in width. [The other now exploded, sending the freezing mixture in all directions.] This other bottle is now broken; although the iron was nearly half-an-inch thick, the ice has burst it asunder. These changes always take place in water; they do not require to be always produced by artificial means, we only use them here because we want to produce a small winter round that little bottle instead of a large one. But if you go to Canada, or to the North, you will find the temperature there out of doors will do the same thing as has been done here by the freezing mixture.

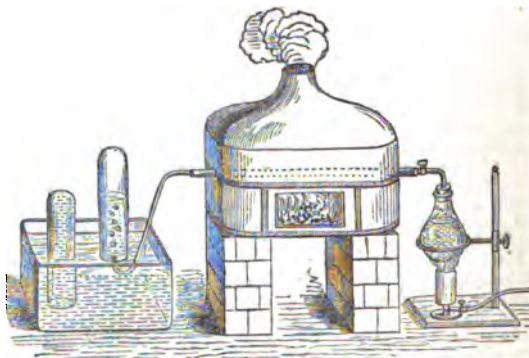
To return to our quiet philosophy. We shall not in future be deceived, therefore, by any changes that are produced in water. Water is the same everywhere, whether produced from the ocean or from the flame of the candle. Where, then, is this water which we get from a candle? I must anticipate a little, and tell you. It evidently comes, as to part of it, from the candle, but is it within the candle beforehand? No. It is not in the candle; and it is not in the air round about the candle which is necessary for its combustion. It is neither in one nor the other, but it comes from their conjoint action, a part from the candle, a part from the air; and this we have now to trace so that we may understand thoroughly what is the chemical history of a candle when we have it burning on our table. How shall we get at this? I myself know plenty of ways, but I want you to get at it from the association in your own minds of what I have already told you.

I think you can see a little in this way. We had just now the case of a substance which acted upon the water in

the way that Sir Humphry Davy showed us, and which I am now going to recall to your minds again by making an experiment upon that dish. It is a thing which we have to handle very carefully, for you see if I bring a little splash of water near this mass it sets fire to part of it; and if it set fire to a part, and there was free access of air, it would set fire to the whole. Now this is a metal—a beautiful and bright metal—which rapidly changes in the air, and as you know, rapidly changes in water. I will put a piece on the water, and you see it burns beautifully, making a floating lamp, using the water in the place of air. Again, if we take a few iron filings or turnings and put them in water we find that they likewise change. They do not change so much as this potassium does, but they change somewhat in the same way, they become rusty, and show an action upon the water, though in a different degree of intensity, to what this beautiful metal does; but they act upon the water in the same manner generally as this potassium. I want you to unite these different facts together in your minds. I have another metal here, and when we examined it with regard to the solid substance produced from combustion we had an opportunity of seeing that it burnt; and I suppose, if I take a little strip of this zinc and put it over the candle, you will see something half way, as it were, between the combustion of potassium on the water and the action of iron,—you see there is a sort of combustion. It has burnt, leaving a white ash or residuum, and here also we find that that metal has a certain amount of action upon water.

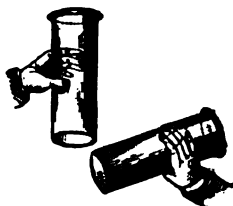
By degrees we have learned how to modify the action of these different substances, and to make them tell us what we want to know. And now, first of all, I take iron. It is a common thing in all chemical reactions, where we get any result of this kind, to find that it is increased by the action of heat; and if we want to examine minutely and carefully the action of bodies one upon another, we often have to refer to the action of heat. Now you know, I think, that iron filings burn beautifully in the air; and I am about to show you an experiment of this kind, because it will impress upon you what I am going to say about iron in its action on water. If I take a flame which I make hollow,—you know why, because I want to get air to it and in it, and therefore I make it hollow,—if I take a few iron filings, and drop them into the flame, you see how well they burn. That combustion proceeds from the chemical action which is going on when we ignite those particles. And so we proceed to consider these different effects, and ascertain what iron will do when it meets with water. It will tell us the story so beautifully, so gradually and regularly, that I think it will please you very much.

I have here a furnace with a pipe going through it like an



iron gun-barrel, and I have stuffed that barrel full of bright iron turnings, and the part that is so stuffed is put into the fire and is made red-hot. We can either send air through the barrel to come in contact with the iron, or else we can send steam from this little boiler at the end of the barrel. Here is a stop-cock which shuts out the steam from the barrel until we wish to admit it. There is some water in these jars, which I have coloured blue so that you may see what happens. Now you know very well that any steam I might send through that barrel, if it went through into the water in the form of steam, would be condensed; for you have seen that steam cannot remain as steam if it be cooled down; you saw it here [pointing to the tin flask] crushing itself into a small bulk, and causing the flask containing it to collapse.

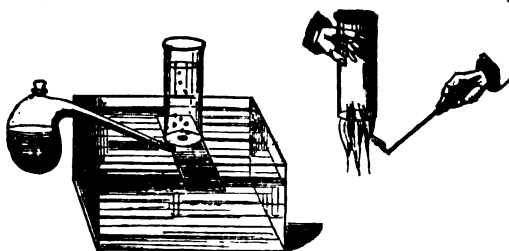
So that if I were to send steam through that barrel it would be condensed—supposing the barrel were cold,—it is, therefore, heated to perform the experiment I am now about to shew you. I am going to send the steam through the barrel in small quantities, and you shall judge for yourselves when you see it issue from the other end, whether it still remains steam. Steam is condensable into water, and when you lower the temperature of steam you convert it back into fluid water; but I have lowered the temperature of the gas which I have collected in this jar, by passing it through water after it has passed through the iron barrel, and still it does not change back into water. I will take another test and apply to this gas. (I hold the jar in an inverted position, or else I should lose my substance.) If I now apply a light to the mouth of the jar it ignites with a slight noise. That tells you that it is not steam, steam puts a fire out, it does not burn; but you saw that what I had in that jar burnt. We may obtain this substance equally from water produced from the candle flame as from any other source. When it is obtained by the action of the iron upon the aqueous vapour, it leaves the iron in a state very similar to that in which these filings were when they were burnt. It makes the iron heavier than it was before. So long as the iron remains in the tube and is heated, and is cooled again without the access of air or water, it does not change in its weight; but after having had this current of steam passed over it, it then comes out heavier than it was before, having taken something out of the steam, and having allowed something else to pass forth, which we see here. And now, as we have another jar full, I will show you something most interesting. It is a combustible gas; and I might at once take this jar and set fire to the contents, and show you that it is combustible; but I intend to show you more if I can. It is also a very light substance. Steam will condense; this body will rise in the air, and not condense. Suppose I take another glass jar, empty of all but air; if I examine it with a taper I shall find that it contains nothing but air. I will now take this jar full of the gas that I am speaking of, and deal with it as if it were a light body; I will hold both upside down, and turn the one up under the other; and that which con-



tains, or did contain, the gas obtained from the steam, what does it contain now? You will find it now only contains air. But look! Here is the combustible substance [taking the other jar] which I have poured out of the one jar into the other. It still preserves its quality, and condition, and independence, and therefore is the more worthy of our consideration, as belonging to the products of a candle.

Now, this substance which we have just got by the action of iron on the steam or water, we can also get by means of those other things which you have already seen act so well upon the water. If I take a piece of potassium, and make the necessary arrangements, it will produce this gas; and if I take a piece of zinc, I find, when I come to examine it very carefully, that the main reason why this zinc cannot act upon the water continuously as the other metal does, is because the result of the action of the water envelopes the zinc in a kind of protecting coat. We have learned in consequence, that if we put into our vessel only the zinc and water, they, by themselves, do not give rise to much action; and we get no result. But suppose I proceed to dissolve off this varnish,—this encumbering substance,—which I can do by a little acid; the moment I do that I get the zinc acting upon the water, exactly as the iron did, but at the common temperature. The acid in no way is altered, except in its combination with the oxide of zinc, which is produced. I have now poured the acid into the glass, and you would think I was applying heat to cause this boiling up. There is something coming off from the zinc very abundantly, which is not steam. There is a jar full of it; and you will find that I have exactly the same combustible substance remaining in

the vessel, when I hold it upside down, that I produced from the experiment with the iron barrel. This is what we get



from water, and this is the substance which is contained in the candle.

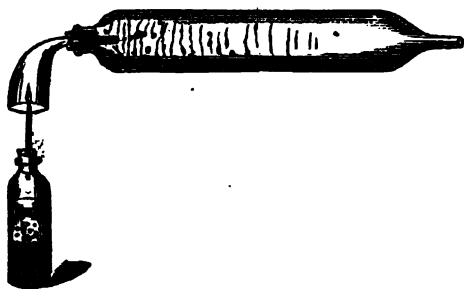
Let us connect these two points clearly and distinctly together. This is hydrogen—a body classed among those things which, in Chemistry, we call elements, because we can get nothing else out of them. A candle is not an elementary body, because we can get carbon out of it, we can get this hydrogen out of it, or at least out of the water which it supplies. And this gas has been so named hydrogen, because it is that element which, in association with another, generates water.* Mr. Anderson having now been able to get two or three jars of gas, we shall have a few experiments to make, and I want to show you the best way of making these experiments. I won't be afraid to show you, for I like you to make experiments, if you will only make them with care and attention, and the assent of those around you. As we advance in Chemistry we are obliged to deal with substances which are rather injurious if in their wrong places; the acids, and heat, and combustible things we use, might do harm if used carelessly. If you want to make hydrogen, you can make it easily from bits of zinc, and sulphuric, or muriatic acid. Here is what in former times was called the "philosopher's candle." It is a little phial



with a cork and a tube or pipe passing through it. And I am now putting a few little pieces of zinc into it. This little instrument I am going to apply to a useful purpose in our demonstrations, for I want to show you that you can prepare hydrogen, and make some experiments with it as you please, at your own homes. Let me here tell you why I am so careful to fill this phial nearly, and yet not quite, full. I do it because the evolved gas which, as you have seen, is very combustible, is explosive to a considerable extent, when mixed with air, and might lead to harm if you were to apply a light to the end of that pipe before all the air had been swept out of the space above the water. I am now about to put in the sulphuric acid. I have used very little zinc and more sulphuric acid and water because I want to keep it at work for some time. I, therefore, take care in this way to modify the proportions of the ingredients so that I may have a regular supply,—not too quick, and not too slow. Supposing I now take a glass and put it upside down over the end of the tube, because the hydrogen is light I expect that it will remain in that vessel a little while. We will now test the contents of our glass to see if there be hydrogen in it,—I think I am safe in saying we have caught some [applying a light]. There it is, you see. I will now apply a light

* *Idem*, "water," and *gennao*, "I generate."

to the top of the tube. There is the hydrogen burning. There is our philosophical candle. It is a foolish, feeble sort of a flame, you may say, but it is so hot that scarcely any common flame gives out so much heat. There is the flame and the heat of the flame, and you see it goes on burning regularly, and I am now about to put that flame to burn under a certain arrangement in order that we may examine its results and make use of the information which we may thereby acquire. Inasmuch as the candle produces water, and this gas comes out of the water, let us see what this gives us by the same process of combustion that the candle went through when it burnt in the atmosphere, and for that purpose I am going to put the lamp under this apparatus, in



order to condense whatever may arise from the combustion within it. In the course of a short time you will see moisture appearing here in the cylinder, and you will get the water running down the side, and the water from this hydrogen flame will have absolutely the same effect upon all our tests, being obtained by the same general process as in the former case. This hydrogen is a very beautiful substance. It is so light that it carries things up; it is far lighter than the atmosphere, and I dare say I can show you this by an experiment which, if you are very clever, some of you may even have skill enough to make. Here is our generator of hydrogen, and here are some soap-suds. I have an india-rubber tube connected with the hydrogen generator, and at the end of the tube is a tobacco-pipe. I can thus put the pipe into the suds and blow bubbles by means of the hydrogen. You observe how the bubbles fall downwards when I blow them with my warm breath; but notice the difference when I blow them with hydrogen. [The Lecturer here blew bubbles with hydrogen, which rose to the roof of the theatre.] It shows you how light a thing this must be in order to carry with it not merely the ordinary soap-bubble, but the larger portion of a drop hanging to the bottom of it. I can show its lightness in a better way than this; larger bubbles than these may be so lifted up; indeed, in former times balloons used to be filled with this gas. Mr. Anderson will fasten this tube on to our generator, and we shall have a stream of hydrogen here with which we can charge this balloon made of collodion. I need not even be very careful to get all the air out, for I know the power of this gas to carry it up. [Two collodion balloons were inflated and sent up, one being held by a string.] Here is another larger one made of thin membrane, which we will fill and send up; you will see they will all remain floating about until the gas escapes.

What, then, are the comparative weights of these substances? I have a table here which will show you the proportion which their weights bear to each other. I have taken a pint and a cubic foot as the measures, and have placed opposite to them the respective figures. A pint measure of this hydrogen weighs three-quarters of our smallest weight, a grain, and a cubic foot weighs one-twelfth of an ounce; whereas a pint of water weighs 8750 grains, and a cubic foot of water weighs almost 1000 ounces. You, therefore, see what a vast difference there is between the weight of a cubic foot of water and a cubic foot of hydrogen.

Hydrogen gives rise to no substance that can become solid, either during combustion or afterwards as a product of its combustion; but when it burns it produces water only, and if we take a cold glass and put it over the flame of it, it becomes damp, and you have water produced immediately in abundance; and nothing is produced by its combustion but the same water which you have seen the flame of the candle produce. It is an important point for you to consider that this hydrogen is the only thing in the world that by combustion gives such a substance as this as its sole product.

And now we must endeavour to find out some fresh proof of the general character and composition of this water, and for this purpose I will keep you a little longer, so that at our next meeting we may be better prepared for the subject. We have the power of arranging the zinc which you have seen acting upon the water by the assistance of an acid, in such a manner as to cause all the power to be evolved in the place where we require it. I have behind me a voltaic pile, and I am just about to show you at the end of this lecture, its character and power, that you may see what we shall have to deal with when we next meet. I hold here the extremities of the wires which transport the power from behind me, and which I shall use to act on the water.

We have previously seen what a power of combustion is possessed by the potassium, or the zinc, or the iron filings; but none of them show such energy as this. [The Lecturer here made contact between the two terminal wires of the battery when a bright flash of light was produced.] This light is, in fact, produced by a forty-zinc power of burning, it is a power that I can carry about in my hands through these wires at pleasure, although if I applied it wrongly to myself it would destroy me in an instant, for it is a most intense thing, and the power you see here put forth while you count five [bringing the poles in contact and exhibiting the electric light] is equivalent to the power of several thunder-storms, so great is its force. And that you may see what intense energy it has I will take the ends of the wires which convey the power from the battery behind me, and with it I dare say I can burn this iron file. Now this is a chemical power, and one which, when we next meet, I shall apply to water, and show you what results we are able to get.

[The Fourth Lecture will appear in our next Number.]

NOTICES OF BOOKS.

Etude sur l'Origine du Diamant. By M. D. Rossi, of Toulon. (Reprinted from the *Bulletin de la Société Scientifique de Draguignan*.) Draguignan: P. Grimbart. 1860.

This is an interesting little work upon the diamond, considered in a scientific point of view, and more especially as to its chemical origin in Nature.

In the first place, the author exposes the opinions of ancient and modern philosophers regarding the nature of the diamond. The opinion of Pliny, who considered it as *pure water solidified*, and that of Linnaeus, who called it *alumen lapidosum, pellucidissimum, solidissimum, hyalinum*, will suffice as examples of the extent of knowledge possessed by our ancestors on the nature of this precious stone.

When in later days it was found, by direct experiment, that the diamond was pure, crystallised carbon, it became interesting to endeavour to produce it artificially, and to account for its formation in Nature. Concerning the latter point, M. D. Rossi endeavours to refute the opinion professed by the late M. D'Orbigny, that "the diamond appears evidently to be the result of a crystalline transformation of vegetable *detritus*;" and to prove from geological and other deductions that it has been formed before any plants existed, by means of the natural carburets of hydrogen or other compounds of carbon existing on the surface of the globe.

He brings forward arguments, also, to prove that the diamond was not formed by fusion, remarking that we have never been able to fuse carbon, and by applying much heat, have only transformed it into graphite. According to our author, Sir David Brewster has shown that diamonds sometimes contain small cavities filled with gas.

On the other hand, M. Rossi details many experiments made with the view of forming the diamond artificially. Among them are some that certainly deserve repeating:—In favour of his opinion that the diamond may have resulted from the decomposition of some carbon compound existing before plants appeared upon the globe, M. Rossi cites an experiment made by Mr. J. Joyce, in which the latter endeavoured to fire a mixture of carbonic acid and

hydrogen by the electric spark. It was supposed that the oxygen of the carbonic acid would unite with the hydrogen and deposit pure carbon, perhaps in a crystalline state. "At the end of one operation," says M. Rossi, "it was imagined that diamonds could be distinguished with a magnifying-glass."

Many other experiments made with the same hopes turned out unsuccessful. Indeed, the late M. Cagniard de la Tour thought he had obtained some small, flat crystals of diamond by fusion, and M. Despretz by the action of electricity upon certain compounds of carbon. But M. Rossi brings forward another experiment in which diamonds appear really to have been picked up after the operation. It was performed by M. Gannal. Eight ounces of water and eight ounces of sulphide of carbon were introduced into a flask, and eight ounces of phosphorus in sticks were added; in four months' time some crystals were formed, and separated by pressing the liquid through chamois leather. The crystals (twenty in number) thus obtained, when submitted to examination, were found to scratch steel; no metal could act upon them; they were perfectly transparent, and possessed much brilliancy; finally, some of them appeared crystallised in dodecahedrons. They were not submitted to chemical analysis.

We should not do justice to M. Rossi's little work if we did not mention that it contains more than its title seems to indicate; the author having included an historical sketch of the diamond, and some remarks upon its physical and chemical properties, which well deserve perusal by those who feel interested in this mineral.

CORRESPONDENCE.

Restoration of Exhausted Manganese.

To the Editor of the CHEMICAL NEWS.

SIR,—My attention has been drawn to an enquiry, in a late number of the CHEMICAL NEWS, as to whether any practical means exist of recovering manganese.

Messrs. Tennant's mode has been alluded to, but there is also another patented by myself; the practical operation having been carried on for a whole summer.

The most marked features in my process are, the absence of expensive machinery; the economy of the operations; and an oxide of manganese produced containing 52 per cent. of peroxide—the same if required can be brought up to 70 per cent. of peroxide. Both of these products are almost free from iron.—I am, &c.

EDWARD HAEFFELY.

Farnworth, near Manchester.

Action of Iodine on Turpentine.

To the Editor of the CHEMICAL NEWS.

SIR,—Your correspondent, Mr. Dearden, is in error in supposing that no mention is made in Chemical works, of the action of iodine upon oil of turpentine; if he will refer to "Slater's Hand-book of Analysis," page 225, he will find a notice upon the subject.—I am, &c.

F. P.

Chemical Notices from Foreign Sources.

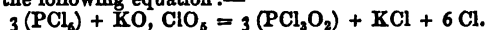
I. MINERAL CHEMISTRY.

Action of Hydrogen, Oxygen, and Chlorate of Potash on Perchloride of Phosphorus.—When pure, dry hydrogen, says M. Baudrimont (*Comptes Rendus*, t. li. p. 823), is passed through a glass tube heated to redness, into which the vapour of perchloride of phosphorus is passed at the same time, hydrochloric acid gas and liquid protochloride of phosphorus are formed. But the action of the hydrogen does not end

there: ordinary phosphorus is set at liberty, which becomes converted into the red amorphous, and a gas is disengaged which burns with a beautiful emerald green flame, and produces white vapours of phosphoric acid. This gas appears to be a mixture of phosphuretted hydrogen with an excess of free hydrogen. Although not spontaneously inflammable, it fumes in the air as soon as formed, and soon deposits a light yellowish film which appears to be solid phosphide of hydrogen. The author thinks that the hydrogen acts at once on both the chlorine and phosphorus of the PCl_5 .

At a dull red heat pure and dry oxygen also acts on vapour of PCl_5 , and during the reaction a beautiful phosphorescent light illuminates the tube. Considerable quantities of chlorine are evolved, and the tube becomes covered with downy flocculi of anhydrous phosphoric acid, while a liquid, coloured yellow by chlorine, condenses in the receiver. This liquid shaken up with mercury, and then distilled, is obtained colourless. It boils at 110° . It is decomposed by water, giving hydrochloric and phosphoric, without a trace of phosphorous acid; it is then chloroxide of phosphorus, PCl_3O_2 . The author considers the foregoing reaction the first example of the direct substitution of free oxygen for combined chlorine.

When three equivalents of PCl_5 , and one equivalent of KOClO_3 , both in powder, are mixed together, the mixture liquefies immediately, and the temperature rises a good deal. At the same time hypochlorous, or hypochloric acid gas, is evolved, which often explodes, but soon chlorine is produced. What remains in the vessel is pure chloride of potassium (quite free from phosphate) suspended in a liquid formed of the chloroxide of phosphorus holding chlorine in solution. The change is represented by the following equation:—



The formation of the explosive gas the author supposes to depend on the action of moisture on the PCl_5 , by which hydrochloric and phosphoric acids are formed, which act first on the chlorate of potash. The action of PCl_5 on KOClO_3 , the author considers another example of the substitution of oxygen for chlorine.

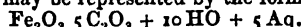
Biniodide of Potassium.—The same author (*Ibid.* p. 827) dissolved an equivalent of iodine in iodide of potassium, which, it is generally admitted, forms a biniodide of potassium. He then shook up the solution with sulphide of carbon, and found that the latter removed all the iodine, and left a colourless solution of KI. The removal of the iodine by a simple solvent, the author thinks, proves that it is not combined with the KI, but only held in solution.

II. ORGANIC CHEMISTRY.

New Vegetable Principle.—M. Ad. Chatin announces (*Ibid.* p. 810) the discovery of a colourless, neutral, nitrogenised and non-coagulable principle, which he says exists in a notable proportion in all vegetable tissues in process of formation, and which for the present he designates A. This matter exists in all vegetables, and is held in solution in a juice distinctly acid. Most vegetable acids and dilute mineral acids preserve it from change, but it is quickly turned brown by alkalies. M. Chatin was led to the discovery by observing that certain tissues which are always colourless in a fresh plant, are regularly coloured brown in old specimens; and that tissues in process of formation, and those which take the most active part in the phenomena of vegetation, show the colouration. He supposed, therefore, that the nourishing juices of vegetables must contain a principle which is colourless in living tissues, but which, on the death of the organs, and from other causes, undergoes a change indicated by the alteration of the colour, and is the exclusive cause of the brown appearance of autumnal and dead leaves in general. A, after it has turned brown, may be extracted from

autumn leaves by treating them with a mixture of ether and water, the former of which will dissolve the green matter and the latter the brown, but the author does not tell us how we may separate the original colourless A.

Oxalate of Peroxide of Iron.—Dr. Phipson (*Ibid.* p. 831) forms the above salt by dissolving ferric hydrate in a boiling solution of oxalic acid. By evaporation, emerald green oblique prismatic crystals are obtained, which are very soluble in water. When exposed to solar light, they blacken like chloride of silver; and, if they are then treated with water, they dissolve with a crackling noise, and leave a deposit of yellow oxalate of the protoxide. By a prolonged exposure to light all the green persalt is decomposed. The green crystals lose five equivalents of water at 100° C., and turn white; when decomposed by heat they lose ten more equivalents of water; for one equivalent of the peroxide they contain five equivalents of oxalic acid, hence their composition may be represented by the formula,—



Decolorisation of Iodide of Starch by Heat.—This, says M. Baudrimont (*Ibid.* p. 827), depends upon the volatilisation of the iodine, and the re-coloration of the liquid on cooling is caused by the re-solution of the vapour of iodine which will remain on the surface. This is proved by heating some iodide in an open capsule, and blowing on the surface, after which the solution does not become coloured again on cooling. But if the iodide be boiled in a small flask, from which the vapour cannot so easily escape, the solution does become coloured on cooling. Further, when heated in a sealed tube, without air, the solution does not decolorise.

MISCELLANEOUS.

Royal Institution.—The following Lectures will be delivered at the Royal Institution during the current week:—Tuesday, January 22, at 3 o'clock, Professor Owen "On Fishes." Thursday, January 24, at 3 o'clock, Professor Tyndall, "On Electricity." Friday, January 25, at 8 o'clock, Dr. G. C. Wallich "On the Nature of the Deep-sea Bed and the Presence of Animal Life at Vast Depths in the Ocean." Saturday, January 26, at 3 o'clock, Professor Frankland "On Inorganic Chemistry."

How to Keep the Water in Burst Pipes.—We suppose that many, probably most, of our readers have already had experience of the annoyance caused by the bursting of the water-pipes during the short interval of the last thaw, and are anticipating a similar inundation when the next comes. Our own experience of the evil has been of rather a severe kind. On one occasion the cistern exhausted itself wholly, and another partially, into our laboratory, and the damage caused led us to consider whether something could not be done to prevent the recurrence of a similar catastrophe, and the device we have adopted is one which it may be of use to some of our readers to be made acquainted with. Having ascertained the points at which the pipes showed signs of weakness, we caused the fractured portions to be cut away, and in place of these we substituted a piece of vulcanised india-rubbertubing, the diameter of the hollow core being a little less than the outer diameter of the lead pipe. The orifice being enlarged by stretching, about an inch and a-half of the end of the lead pipe was inserted, a little copper-wire binding caused the tube to embrace the pipe with such force that there was no risk of the pressure of the water forcing it off when the ice melted. The same operation being performed with the other end we had a continuous pipe, and can wait with tranquillity until the plumber shall be sufficiently at liberty to restore it in

a more permanent manner; and in the meanwhile having nothing to fear from the spontaneous irruption of the cool element into our house through irregular channels, or of our supply being cut off. With the view of preventing the future bursting of the principal pipes exposed to the cold, we propose to have the lead ones removed, and strong vulcanised india-rubber ones substituted for them. We need hardly say that for the frost to burst this is out of the question, since the elasticity of the material will yield to the expansive force of the water passing into the condition of ice. Whether such a substitution will be found to answer in a pecuniary point of view remains to be proved; but the difference must of necessity be trifling either way. Moreover, we have the satisfaction of knowing that we cannot have property of ten times its value, destroyed through its bursting—a contingency to which we should be liable at every thaw if we retained the old lead pipe.

ANSWERS TO CORRESPONDENTS.

. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

. All Editorial Communications are to be addressed to Mr. CROOKES and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the CHEMICAL NEWS, containing a copious Index, is now ready, price 12s., by post 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

D. Logan.—The numbers were sent by post as soon as the letter arrived.

Ozonized Cod-liver Oil.—A correspondent wishes to be referred to any article on, or account of, the preparation, nature, or properties of ozonized cod-liver oil.

G. F. R.—The subject of your paper is so purely theoretical, and unsupported by experiment, that we think it would be scarcely applicable for insertion in our columns. Amber-dust or fragments can readily be disposed of at the varnish-makers', amber varnish being largely used in photography.

F. C. H.—Received with thanks.

Books, &c., Received.—"Illustrated Catalogue of Chemical Apparatus," J. J. Griffin, Bunhill-row. "Infant Alimentation; or, Artificial Feeding, as a Substitute for Breast-milk, considered in its Physical and Social Aspects." By M. A. Baimes. A paper from the *Lancet* of January 12, 1861.

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THE CHEMICAL NEWS

VOL. III. No. 60.—January 26, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

An Account of Guy's Hospital Well, by
WILLIAM ODLING, M.B., F.R.S.

(Continued from page 37.)

Some of the constituents of these waters are highly interesting in a scientific point of view. The presence of potash in deep-well water was first ascertained in the case of the Artesian water of Grenelle, near Paris, which, moreover, is quite free from soda salts. The fixed constituents in a gallon of this water, as estimated by M. Payen,¹ are shown below:—

Constituents.	Grains per Gallon.
Chloride of potassium	0.763
Bicarbonate of potash	2.072
Sulphate of potash	0.860
Carbonate of lime	4.760
Carbonate of magnesia	0.994
Silica	0.399
Organic matter	0.015
Total residue	9.863

Mr. Graham was unable to detect potash in Messrs. Combe and Delafield's well, nor could Mr. Brande recognise its presence in the water of the Mint. It has, however, been found in the Trafalgar-square water, by Messrs. Abel and Rowney, by Mr. D. Campbell, and by Dr. Dupré and myself. Moreover, potash has been detected by several chemists in various other deep well waters of the London basin; there was no difficulty in recognising its existence in the Guy's Hospital water. I am quite unable to account for the irregularity of its presence in waters derived from the same strata, and at situations comparatively near to one another. It does not appear to depend upon the different depths of the borings.

The presence of phosphoric acid in deep-well water was first ascertained by Mr. Graham in his examination of the water of Messrs. Combe and Delafield's well in 1845. This acid has since been invariably found, when sought for, in the deep waters of London. Its presence is not surprising, for phosphoric acid is a compound of which minute quantities are very widely distributed in nature. It exists in the deep-well waters in variable, but always in very small, proportions. The amount of phosphoric acid which I found in Guy's Hospital water is considerably less than that which other chemists have found in the waters of other deep wells. The estimation, however, was made very carefully, and corroborated in several ways. When nitrate of silver is added to an aqueous solution of the fused residue of an evaporated deep-well water, an abundant precipitate is produced,

the yellow colour of which is at times very decided. This yellow colour has been considered to result from the phosphoric acid existing in the water. In my opinion, the quantity of phosphoric acid present is, in no case, anything like sufficient to account for the coloration, which, I believe, is really dependent upon the carbonate of soda. In adding nitrate of silver solution to the solution of an alkaline carbonate, whenever the drop of nitrate of silver is temporarily in excess, we always obtain a yellow precipitate, as is, indeed, well known. Phosphoric acid is also found in the water of shallow wells, or land-springs of the London gravel; but, in this case, it is, doubtless, derived from contamination of the water with products of animal decomposition, and is associated with obvious quantities of other compounds of similar origin,—ammonia, nitric acid, &c.

The source of the carbonate of soda which exists so uniformly, and to so considerable an extent, in the deep waters of London, has been the subject of much speculation, and is even now undecided. The general opinion at one time entertained was, that it resulted from the admixture, and mutual reaction, of infiltrated sea water, rich in soda salts, with proper chalk water, containing much bicarbonate of lime. The principal arguments in favour of this view were considered to be the following:—

Water from chalk wells, situated above the level of Trinity high-water mark,—the Watford water for instance,—is comparatively free from soda salts, which exist so largely in sea water; whereas, these salts are found to a considerable extent in the water of chalk wells descending greatly below high-water mark, such as the deep-well water of the London basin. Again, the high chalk water, though poor in soda salts, is rich in carbonate of lime, whereas, the deep chalk water is comparatively rich in soda salts, and poor in carbonate of lime; and this difference in character has been thought to indicate mutual decomposition of the soda salts of the sea, with the carbonate of lime of the proper chalk water, whereby carbonate of soda is produced in the deep-well water. Moreover, it has been stated that the proportion of soda salts in the deep-well water of London was gradually increasing, a circumstance which, it was conceived, could scarcely occur unless that water had some communication with the sea. The above view has been controverted, and, as I think, successfully, by Mr. Dugald Campbell.² He has shown that the alleged increase of soda salts in the deep-well water is a mistake; that the shallow chalk water contains an obvious amount of nitric acid which is not to be found in the deep-well water; and finally, that no admixture of proper chalk water and sea water could account for the composition of the deep-well water as a whole. The following table of the principal constituents of the different waters, calculated, No. I. from Mr. D. Campbell's analysis of the Watford water; No. II. from my own analysis of the Guy's Hospital water; and No. III. from Mr. Schweizer's analysis of the water of the English

¹ *Annales de Chimie et de Physique*, 3dme sér., t. i. p. 381.

² *Quarterly Journal of the Chemical Society*, vol. ix. p. 22.

Channel, illustrate Mr. Campbell's position. If the sodium of the deep-well water, for example, had been derived wholly from the sea, it ought to have been associated with at least 25.63 grains, instead of with only 9.75 grains of chlorine:—

Constituents.	I.	II.	III.
Chlorine	0.84	9.75	1366.75
Sulphuric acid . .	0.39	9.24	164.99
Carbonic acid . .	8.00	6.90	1.01
Calcium	7.71	1.05	29.86
Magnesium . . .	—	0.69	97.32
Sodium	0.77	16.61	744.69
Potassium . . .	—	0.54	28.03

In reference also to the above view, it is worthy of observation that the deep chalk water of Southampton, which appears to be contaminated to a considerable extent with sea water, is entirely free from carbonate of soda.¹ In addition to other constituents of the sea, this water contains 62.8 grains of common salt in a gallon, whereas the Trafalgar Square water contains only 19.0, and the Guy's Hospital water only 16.0. Mr. Campbell has also shown the improbability of a modification of the above view, namely, that the deep-well water results from an admixture of the proper chalk water with sea water, which has undergone a change by percolating through an immense bed of chalk.

In the present imperfect state of our knowledge, the most probable explanation of the source of the carbonated alkali in the deep water of London, seems to me to consist in recognising the decomposition of an alkaline silicate, chiefly silicate of soda, existing in the strata above the chalk. The presence of carbonated alkali in the deep water of Grenelle, which is free from salts of the sea, and the presence of salts of the sea in the deep water of Southampton, which is free from carbonated alkali, seem to negative the idea that the existence of carbonated alkali in certain deep-well waters is dependent in any way upon contamination with sea water. That it may be derived from alkaline silicates is rendered probable by the following circumstances, namely, that alkaline silicates are among the most common constituents of minerals, and are known to suffer slow decomposition by the action of water containing carbonic acid, rain-water for example;² and that carbonate of soda is not confined to the deep-water from the chalk, but exists also in the water of the porous strata intervening between the London clay and the chalk. In a preceding part of this communication, the analyses of two waters from Mr. Burnett's well at Vauxhall are given. This well is so constructed, that the water of the gray sand above the chalk can be obtained separately from the water of the chalk itself. No. v. is the analysis of the chalk water, and No. vi. that of the sand water. It will be observed that the general characters of the two correspond closely, the principal difference being that the chalk water contains more carbonate of soda, and less sulphate of soda, than does the sand water. This is a difference which may be readily explained, by supposing the sulphate of soda of the sand water to undergo a partial decomposition with the bicarbonate of lime, which characterises a pure chalk water. Moreover, it is quite possible that the formation of carbonated alkali, begun by the decomposition of alkaline silicates in the plastic clay and gray sand, may be continued by the decomposition of alkaline silicates in the chalk. My results

with the gray sand water are not singular; Mr. D. Campbell examined a specimen of well water from the plastic clay, and found it to correspond closely in its characters with deep-well water from the chalk. Messrs. Clarke and Medlock again, in their examination of the Westbourne Park well water, observe, "the almost complete identity of water from the sand and that from the chalk." I am inclined to think that the principal source of the deep-well water of London is rain water, a considerable proportion of which has drained through the highly porous sand formations beneath the mottled clay, into the chalk, which constitutes its principal reservoir. The porous cocene beds, intervening between the London clay and the chalk, have a very extensive superficial distribution for the reception of rainfall. On the south, they are found in Kent, at Woolwich, Blackheath, and Bromley, &c.; and in Surrey, beyond Croydon, and at the Addington Hills. To the north of London, they appear at the surface in parts of Hertfordshire, and of Middlesex, particularly in the neighbourhood of Bushey. This view of the source of the carbonate of soda in deep-well water does not account in any way for the presence of the chloride of sodium. It is not contended, however, that the well water is entirely free from sea-contamination, but only that sea-contamination is not the cause of its alkalinity. From the fact of portions of the chalk, and of the much more porous strata above it, cropping out as we proceed eastwards along the course of the Thames to the sea, it seems not improbable that the deep water of London should be contaminated to some extent with sea water. The deep water of the Paris basin, indeed, is free from sea salt; but then the direct distance of the chalk under Paris from the sea, the altered relations to the sea of the littoral chalk itself, which, in some parts eastwards, rests upon a bed of Kimmeridge clay, forming a portion of the sea-bottom, and the non-communication of any porous strata above the chalk with the sea, render the case of the Paris basin very different from that of London, as regards its liability to sea-contamination. Moreover, at Grenelle, the boring has pierced through the chalk, and entered the lower green-sand formation, from which, I believe, the supply of water is principally derived.

On the Detection of Phosphorus, by Dr. FRED. HOFFMAN.

THE author has instituted a series of more than 150 experiments with Mitscherlich's method for detecting phosphorus. He has mixed the phosphoric mass of matches in different preparations, with food cooked in various ways, with beverages commonly used, and with medicines in a liquid, soft, and plastic state, and kept the mixtures for a variable length of time. The presence of phosphorus, it will be remembered, is established by the phosphorescence imparted to the aqueous vapours on distilling the liquid or liquified mixture, and the distillation of the phosphorus is increased by the addition of sulphuric acid, chloride of sodium, sugar, &c., whereby the boiling-point is made to approach that of phosphorus.

In the place of Mitscherlich's apparatus, the author has constructed a more simple one, which is easily prepared and readily taken apart. It consists of an ordinary flask, connected with a receiving-bottle by means of a glass tube, which passes about 18 inches through a glass cylinder filled with cold water. A long, straight tube conducts the gaseous products from the

¹ Robson, *Quarterly Journal of the Chemical Society*, vol. iv. p. 7.

² A portion of the sand stratum overlying the chalk, obtained from the borings of Guy's Hospital well, was found to contain a considerable proportion of a readily-decomposable silicate of soda.

bottle. The lamp and flask are surrounded with dark paper, and about three-fourths of the glass cylinder. The operation is best performed in a dark room.

The phosphorescence of the liquid increases in intensity with the consistence of the liquid and the quantity of the phosphorus. The gas-bubbles are luminous, rise in the mixture, and apparently burn upon its surface with a bright flame. With the temperature the light is increased: a photosphere fills the flask, rises in the tube, and moves up and down within the cooled part. Sometimes only a column or a luminous ring appears stationary at the point where the vapours are cooled, and a luminous fog or sparks gradually sink into the receiver, or a sudden, frequently-repeated lightning is observed. If the heat is raised too high, or the cooling is insufficient, the luminescence passes through the long gas-tube, at the mouth of which the gases take fire, if the volatile oils from cruciferae (mustard, &c.) have been present.

Coffee, mustard, smoked meat, highly-seasoned food and beverages, and medicines containing odorous gums, resins, volatile oils, musk, castor, camphor, chlorine, &c., have the property of covering the odour of a small portion of phosphorus; minute quantities, which yet produce the symptoms of gastro-enteritis, may not be recognised by the odour in the contents of the stomach. The reaction is not prevented, except by the presence of much alcohol, volatile oils, and mustard. If the quantity of phosphorus be not too insignificant, the phosphorescence is observed, either momentary or constant, at the beginning of the distillation, and after the alcohol has passed over.

The reaction is not interfered with by the presence of ipecacuanha, tartar emetic, magnesia, hydrated oxide of iron, musk, castor, opium, albumen, neutral acid, or basic salts and double salts, volatile organic acids, chlorides, iodides and sulphides, and by free acids; but iodine, chloride and bichloride of mercury in considerable proportion, and metallic sulphides in the presence of free sulphuric acid, and particularly oleum cinæ (artemisise), interfere with or prevent the reaction.

Numerous experiments, by distilling the brain of various animals, blood, albumen, casein, fibrin, legumin, and other protein compounds, with diluted sulphuric acid, yielded not the least photospheric reaction.—*Archiv. de Pharm.*

On Combinations of Iodine with Iodide of Potassium, by B. PIFFARD.

HAVING been lately engaged in making a series of experiments for the purpose of determining whether iodine, when dissolved in a solution of iodide of potassium, is simply held in solution or chemically combined with it; and having come to the conclusion that it is a definite chemical compound, a short statement of some of the principal facts I have arrived at may not be out of place, as I see that an opposite view is occasionally held, from some remarks I read in the *CHEMICAL NEWS* for January 16, 1861.

In the first place, I prepared a saturated solution of iodide of potassium, and in it dissolved as much iodine as it would hold, which, according to Baup, is two equivalents;¹ and I also prepared another similar solution. and to it added several times its bulk of water, which precipitates one equivalent of iodine. From these two

solutions I then precipitated iodide of lead, by means of the acetate of lead.

Now, these iodides of lead, prepared in this manner, neither resembled one another nor the ordinary iodide of lead (PbI); for that precipitated from the first-named solution speedily turned from yellow to a deep-vermilion colour, and that from the second solution, after going through various shades of yellow, green, and blue, finally became nearly black. Both of these precipitates I separated from the liquid and dried without their undergoing any further change. This black iodide of lead has the following properties:—

1. It does not part from iodine at a temperature of 160° F., when even long continued—I have not tried a higher temperature—although uncombined iodine rises in vapour at ordinary temperature.

2. It does not part with its iodine to chloride of ammonium, nitrate of ammonia, or chloroform, in all of which uncombined iodine is soluble.

In the second place, I prepared a saturated solution of bromide of potassium, saturating it afterwards with bromine, and from it, with acetate of lead, precipitated a brilliant yellow bromide of lead; and from a similar solution of bromine in bromide of potassium, after diluting it with water, I precipitated with the acetate of lead, as before, a brickdust-coloured bromide of lead.

In the third and last place, through a saturated solution of chloride of potassium in water I passed a stream of chlorine gas, which was rapidly absorbed, and from it, when saturated, I precipitated with acetate of lead a reddish-brown chloride of lead.

By precipitating from a diluted solution of the above I did not find, however, that the colour of the precipitate was changed.

Now, I think that in all probability my two iodides of lead are different and definite combinations of iodine and lead, and that my bromides are also different and definite combinations of bromine and lead. Assuming this to be the case, and also that they are not produced by the action of free iodine and bromine on the ordinary iodide and bromide of lead,—which, after repeated attempts, I have not succeeded in doing,—I think I may safely come to the two following conclusions:—

1. That iodine and bromine combine respectively with iodide and bromide of potassium, with the aid of water.
2. That they each combine in two definite proportions.

TECHNICAL CHEMISTRY.

On the Alloys of Copper and Zinc, by FRANK H. STORER.

(Continued from page 38.)

Now, since many of the impurities of copper are eliminated when it unites with zinc, being thrown up on the surface of the melted metal as a crust or scum, which is removed by the workmen,¹ and as no two samples of copper contain the same kind or quantity of impurity, the amount of it removed, or, what is the same thing, the amount of copper left to unite with a given weight of zinc,² must continually vary.

¹ This explains the remark of Karsten (*loc. cit.* s. 385), that perfectly pure copper can take up from 1.5 to 2.5 per cent. more zinc than impure, and still afford a product of better colour, more tenacious, and more malleable.

² Much zinc also is lost in this case, both by alloying with the foreign metal and by uniting with any oxygen which may have been combined with the latter. As the workmen say, impure copper "burns up" a great deal of zinc.

¹ See Graham's "Elements of Chemistry," vol. I. p. 529.

From this it may easily be seen how very difficult, if not impracticable, it would be to obtain anything like constant results, unless some test were found by which the workman might ascertain, at least approximately, the composition of his alloy.³

That it is of the first importance that every alloy used for sheathing shall possess some one constant composition, so that it may endure equally well on all parts of the vessel, and that no galvanic action may occur between the metal of different sheets, is a point too obvious to be mentioned.

It might still be urged against yellow-metal, that the admixture of other metals—of kinds capable of mingling with it—which may have been derived from the copper used in its preparation, is a serious objection to its use. This is true, and the remark applies with equal or even greater force to all the alloys used for sheathing; it must always depend upon the condition of copper metallurgy. It would apply more forcibly, for example, to the low-grade brass which Bobierre has proposed to substitute for the ordinary sheathing-metal; for since the "test" by means of which founders are enabled to prepare the alloy (yellow-metal) of constant composition cannot be obtained if metals are present which are incapable of mixing in all proportions with this alloy, in which event small particles of the foreign substances would be found irregularly disseminated upon the fractured surface of the test ingot, manufacturers are enabled to ascertain at once whether or no a given sample of copper is suitable for the preparation of yellow-metal. Whenever it is found that satisfactory tests cannot be obtained, the copper is immediately rejected by the founders, and applied to some other use. No similar means of controlling the purity of the brass in question are known.

It does not appear—at all events, it has never been proved—that any serious injury results in practice from the use of the most varied kinds of copper, so long as they afford the desired *homogeneous* alloy containing 40 per cent. of zinc.

I do not wish to deny that hurtful impurities may at times occur in yellow-metal, in spite of the test to which I have so frequently alluded. Most probably, the very rare cases in which this alloy wears out irregularly, portions of the sheet being much corroded, while other parts are scarcely at all acted upon and have preserved their original colour and malleability, may be referred to the irregular diffusion of such impurities throughout the mass of alloy of which the sheet was formed;⁴ but their influence must be regarded as being of very slight importance in comparison with the changes of structure which have already been discussed.

The following is a list of the alloys which I have prepared. Crystals were obtained from all of them, with the exception, perhaps, of one or two of those lowest in the series, where the tendency to assume a pasty state during solidification renders the crystallisation obscure.

³ It must be well understood, however, that, as a rule, manufacturers of alloys of copper and zinc always use the best copper they can obtain, since it is generally more economical for them to do so. For the purer this metal is, so much the greater will be the total amount of alloy obtained by the use of a given weight of it, or, in other words, the loss, mentioned in the preceding notes, which would occur from the elimination of impurities, will be smaller.

⁴ It is not impossible that the small amount of silver which, as is well known, is precipitated from sea-water upon the metallic sheathing of ships, can accumulate to such an extent in yellow-metal which has been repeatedly used and re-melted, that the durability of the latter may be seriously impaired thereby. It is, however, equally probable, and perhaps more so, that the greater part of this silver is eliminated in the preparation of the alloy, when the old metal is fused with zinc.

The amount of copper contained in both the crystals and the portion of fluid alloy poured off from above them was determined directly by assay in each instance. The copper used in the preparation of the alloys was a very pure article from Lake Superior; it contained only a certain amount of suboxide of copper and traces of silver.

The zinc was from La Vieille Montaigne, containing as its principal impurity a small amount of lead. Both metals were granulated for convenience in weighing. The lead and other impurities have been neglected in stating my results.

Number of the Experiment.	Per cent. of Copper put into the Alloy.	Per cent. of Copper found in the Crystals.	Per cent. of Copper found in the portion of Alloy poured off.
1	99.38	99.14	98.68
2	96.50	95.57	97.40
3	94.38	95.06	95.04
4	94.38	94.30	93.82
5	91.38	90.77	91.49
6	88.38	88.86	88.78
7	85.38	86.10	85.74
8	79.38	83.72	83.61
9	82.38	82.89	82.52
10	79.38	80.34	80.73
11	76.38	77.32	78.92
12	73.38	75.03	74.27
13	74.38	75.71	74.94
14	72.38	74.52	73.61
15	70.00	71.48	71.68
16 ^a	—	—	69.20
17	67.40	68.93	67.37
18	65.94	67.12	—
19 ^b	—	66.94	—
20 ^c	—	65.56	—
21 ^d	—	—	62.30
22	60.29	61.16	61.99
23 ^e	—	60.92	61.36
24 ^f	—	60.77	60.88
25 ^g	—	60.42	60.24
26	56.69	57.82	58.66
27	53.09	54.22	54.45
28	51.00	52.62	52.11
29	49.00	49.51	49.55
30	47.00	49.43	48.26
31	45.00	47.49	46.79
32	43.00	44.41	43.63
33 ^h	—	43.64	44.01
34	41.00	41.48	42.19
35	39.00	38.57	38.77
36	37.00	38.09	37.71
37	35.00	35.33	36.71
38	33.00	32.98	32.79
39	27.31	29.67	29.07
40 ⁱ	—	29.07	—

In determining the amounts of copper contained in these alloys, I have employed a method which has been used for a long time by many of the copper assayers of this country, by whom it is highly esteemed. It is essentially the old Swedish method⁵ of precipitating the copper from its solution in acid, by means of metallic

⁵ The instances in which the amounts of metal used in preparing the alloy are not given refer to crystallised products which were obtained accidentally during the course of the research.

⁶ Yellow-metal, from founder's pots, said, from appearance of test ingot, to require a little more zinc.

⁷ Yellow-metal which had afforded a satisfactory test.

⁸ Vid. Bergman, "Physical and Chemical Essays," translated from the original Latin by E. Cullen, M.D. (London, 1788), vol. ii. p. 443. Described more fully by Kersten, in Karsten u. von Dechen's *Archiv für Mineralogie*, u. s. v., xii. 567; also by Kerl, in Bodemann's *Probirkunst von Bruno Kerl* (Clausthal, 1857), s. 216.

iron, with, however, several more modern modifications and improvements.* Since several of the details of this process have never, to my knowledge, appeared in chemical literature, I shall make no apology for describing it at length in this connection.

In choosing samples of the alloy for analysis, I have been careful to select those portions of the cup which were best crystallised, avoiding always the upper crust, which had lost zinc by volatilisation while cooling; from the sheets into which the fluid portion of the alloy was cast, I have taken pieces near the edge, which were free from any admixture of fragments of the crust.

(To be continued.)

On Coal Gas,¹⁰ by the Rev. W. R. BOWDITCH.

A distinguished Fellow of the Royal Society discovered coal-gas, when Rector of Crofton, about two miles from my present parish, and nearly all our knowledge of this complex substance is derived from the labours of chemists who have been, or are, Fellows of the Society. I feel assured, therefore, that an attempt to extend the knowledge of the reaction of coal-gas with various substances will be favourably received, and that the application to practice of the facts made known, will not render a memoir less acceptable to the Society which rewarded alike the abstract researches of Leverrier and the practical ones of Arnot.

Six years ago I introduced the use of clay into gas-works, for the purpose of improving the purification of coal-gas, and now,—after so long an experience, the purification of many hundreds of millions of feet of gas, and the use of many thousand tons of the refuse as manure,—I venture, for the first time, to submit the grounds upon which my process is based.

Coal-gas may conveniently be considered under the heads of carbon compounds required for the production of heat and light, which generate water and carbonic acid by their combustion; and sulphur and nitrogen compounds which are not necessary for heat and light, and ought to be removed from gas on account of the injurious nature of the substances produced by their combustion.

The former of these classes will be treated of incidentally; the latter class forms the principal subject of this paper. When speaking of *gas*, I always refer to that which has undergone the ordinary condensation of gas-works, wherefore no mention is made of the complex compounds removed by condensation.

When coal is distilled, its nitrogen is evolved in some forms of combination which are generally familiar, while others are almost unsuspected. Under certain conditions of distillation, much nitrogen leaves the retort and passes the condenser as ammonia or some of its salts. These are all removed from gas by clay, so that no trace of ammonia can be discovered after gas has passed through purifiers charged with an adequate quantity of clay, and with lime or some equivalent substance to remove sulphide of hydrogen. Clay is thus entitled to be classed with acids and some metallic salts as a purifier of gas, for these, of course, remove ammonia and its salts. But between clay and acids there is an important difference, in regard to the action which takes place upon the most valuable light-giving constituents of the gas; acids remove a large quantity of these, clay does not. We

have experimental proof that clay does not remove the valuable hydrocarbon vapours from gas, in the fact that strong spirit of wine digested upon foul clay for days, does not thereby become much more luminous than it was before being so treated. The very slight light-giving power which it has obtained is due to tar; for if the spirit be evaporated, and the tar so obtained be re-dissolved in fresh spirit, the same kind of flame will be obtained as before; whereas the addition of a small portion of coal-oil to spirit gives a flame of considerable illuminating power. To this I may add, that long and extensive experience shows that the employment of clay in the purifying process improves the light-giving power of gas, by removing substances which are not otherwise removed, and which, if allowed to be burnt with the gas, lessen its illuminating power. These light-damaging compounds are produced during the later portion of the distillatory process, as I have proved by experiment. The same retort was charged twice with the same weight of the same coal. The gas produced by one charge was purified by lime only, that produced by the other charge was purified by lime and clay. The illuminating power of the gas passing at each half-hour's end was determined, and it was found that the purification made no difference for the first three or four half-hours. About the middle of the charge, that purified by my process had slightly the advantage, and at the close the difference in favour of that purified by the addition of clay has been found as much as ten or twelve per cent. Thus it is shown that the compounds removed by clay from gas produced during the early stages of distillation—however objectionable on other accounts—do not lessen the light-giving power of gas, whereas those removed during the later periods of distillation reduce the light-giving powers considerably.

If conjecture be allowable, I would venture an opinion that cyanogen compounds, and other nitrogenised substances with which foul clay abounds, are those which lessen light. My own investigations lead directly to this inference, and, I think, explain an old Table by Dr. Henry in this sense. In the *Philosophical Transactions* for 1808, he shows that the gas produced from 12 lbs. of Cannel-coal contained, after purification, the following quantities of nitrogen:—

Hours from commencement of distillation.	100 measures of purified gas contain measures of nitrogen.	
$\frac{1}{2}$ an hour	20	Due chiefly to atmospheric air. Probably the time when ammonia was principally produced. Probably vapour of water was present in very small quantity, and cyanogen and related compounds were produced in increasing quantity.
1 hour	44	
3 "	5	
5 "	15	
7 "	15	
9 "	15	
10 $\frac{1}{2}$ "	20	
12 "	20	

Without assuming the absolute accuracy of these figures, we may regard them as valuable indicators, pointing, I think, in the direction I have ventured to conjecture.

A beautiful reaction furnishes experimental proof of the damage done to gas by acids. Clean deal sawdust is well moistened with pure sulphuric acid, diluted with five or six volumes of water, so that the sawdust may not be discoloured, and gas is passed through it in a slow stream. With rich gases, which give the light of from 20 to 25 sperm candles for a consumption of five feet an hour, the sawdust instantly changes to a most beautiful pink colour, and the tint gradually deepens until the whole becomes of a dark mahogany. With poor gases,

* For information concerning these I am indebted to Mr. G. J. Dickinson, chemist, of the copper works at Point Shirley, near Boston.

¹⁰ From the *Proceedings of the Royal Society*, vol. xi. p. 25.

which give the light of from 10 to 12 candles, this coloration is exceedingly faint at first, and deepens very slowly. The differences of coloration are so considerable and constant, that I have no doubt of the possibility of thus determining the value of gas as an illuminant. By using a standard acid, the same kind of sawdust, a uniform volume of gas, and the same sized U-tubes, notation of time and depth of colour would give a close approximation to the illuminating value of the gas. At all events, the sources of error are not greater than those of photometry in the hands of all but the most experienced, and the process is quite as close an approximation to truth as an ultimate analysis of gas, containing, as it does, impurities which render skill and precaution useless. A comparison of the analysis of coal-gas given in "Bunsen's Gasometry," with the substances now known to exist in gas, will convince us that at present we cannot attach any value to such analyses.

To determine the substances in gas which produce this coloration, some of its chief illuminating constituents were prepared and passed separately through the acid sawdust.

Olefiant gas made in the usual manner, and carefully purified, reddens the acid sawdust. Ether vapour does not affect it, and therefore need not be removed from the gas for this experiment.

Propylene, produced by passing the vapour of fusel-oil through a red-hot combustion-tube filled with cast-iron nails, but kept at so low a temperature that a small portion of oil passed over without decomposition, reddened the acid sawdust.

Commercial benzole, with the exception of one specimen, reddened the acid sawdust.

I have not yet had leisure to prepare and test acetylene.

The coloration of fir-wood, moistened by hydrochloric acid, has been mentioned by Williams as characteristic of pyrrole.

To show that the colour was produced by illuminating matter abstracted, some sawdust was treated with acid strong enough to char it slightly; and gas, which instantly reddened the clean sawdust and dilute acid, was passed first through the black and then through the clean acid sawdust. No colour was produced in the latter, though the flow of gas continued for an hour.

Hydrochloric may be substituted for sulphuric acid, so far as that gas colours sawdust moistened with it; but it is liable to a considerable disadvantage. If gas contain ammonia, the vapour of the acid unites with it in the tube before the gas comes into contact with the sawdust; and the result is a deposit of chloride of ammonium on the surface of the sawdust where the colour commences, which renders the observation less precise and easy. Olefiant gas likewise does not redden this acid sawdust, and therefore cannot be estimated by it.

Nitrogenised compounds in coal-gas present the greatest difficulty in the way of efficient purification, and the almost impossibility of obtaining them in a state fit for examination, renders their investigation laborious and unsatisfactory. Much nitrogen is contained in gas as cyanogen, which can be separated from the clay used in purification. Probably not much less exists as sulphocyanogen, which can be separated from the foul clay with ease; and the presence of further quantities in combination with sulphuretted hydrocarbons and tar, can be demonstrated. The bodies formed by this combination of elements are, I believe, unknown at present.

By placing clay in a purifier through which crude gas passes from the condenser of a gas-works, and treating the saturated clay with spirit, a solution is obtained, of a brown colour, which has no effect upon litmus, turmeric, or lead paper, which decolorises a solution of iodine, and from which nitrate of silver throws down a white or brownish-white precipitate, and acetate of lead a white precipitate. The aqueous solution possesses the same properties, and, like the solution in spirit, is always neutral. Litmus paper, immersed in either of the solutions, and exposed to the air, becomes quickly, strongly, and permanently reddened. Soluble sulphides have been tested for repeatedly with nitroprusside of sodium, as well as with acetate of lead, but have never been found; yet a sulphur compound exists in solution which possesses the power of forming a sulphide with metallic mercury. The spirit solution, digested on mercury, with occasional shaking, produces the black sulphide of mercury, while the aqueous solution, similarly treated, produces the red sulphide. Insoluble sulphides, however, exist in the foul clay, and evolve sulphide of hydrogen on the addition of an acid. These insoluble sulphides are oxidised rapidly by exposure of the clay to atmospheric action.

A solution of clay in spirit was treated with an excess of powdered acetate of lead, and the white precipitate filtered off. The brown filtrate was supersaturated with ammonia and filtered. The clear brown filtrate, diluted with twelve times its bulk of water, became milky, and with much difficulty was obtained clear by filtration. Part of the spirit was then distilled off, to ascertain whether it would bring over a volatile sulphur compound, but the spirit was quite free from sulphur. The remaining fluid was then acidified with nitric acid, which caused brisk effervescence and a strong smell of hydrocyanic acid. Nitrate of silver was added as long as it continued to produce a precipitate; the precipitate, dried and heated, gave off cyanogen, which burnt with its characteristic flame. The clear filtrate, slowly evaporated to dryness, left a pale yellow crystalline mass, which did not change colour by several days' exposure to light. Part of this, burnt in a porcelain crucible, gave off nitrous fumes, and left a considerable residue blackened by oxide of silver. Water was added to this residue, and the oxide of silver filtered off, and an abundant precipitate of sulphate of baryta obtained, with a salt of baryta. The remainder of the yellow salt was re-dissolved in water, with a view to separate a granular portion which was mixed with the more perfectly crystalline salt, but an accident unfortunately spoiled the remainder, and rendered any further progress impossible.

(To be continued.)

PHARMACY, TOXICOLOGY, &c.

*Report on Adulterations,
by the American Pharmaceutical Association.*

THE Committee upon the subject of Home Adulterations, appointed at the annual meeting of this Association, held in Boston last year, respectfully report:—

The subject of Home Adulterations is one worthy of the careful consideration of every member of society, whether pharmacist, physician, or consumer,—and all belong to the latter class,—because of the pernicious and often dangerous results which ensue from the use of

¹ This acid was of the same strength as that used in some gas-works.

articles mixed with deleterious substances, or deficient in power, owing to inert substances used to enhance the profit of the unscrupulous tradesman.

Your Committee feel deeply the responsibility of their duty; they feel that much ought to be said and done which they dare not take the responsibility, as individuals, of saying and doing. They trust that the time will come when State legislators, or the General Government, will cause such penalties to be affixed to the selling of adulterated articles that the business will cease to "pay," and that "honesty" will be practised because it is "the best policy." During the past year your Committee have had many articles of drugs, medicines, and food submitted to their examination, and they have felt more and more the necessity of some means being devised for checking this abominable practice, which, in the language of another, "is undermining the very foundation of trade,—viz., faith in commercial integrity."

We refer to our published report in our last year's "Proceedings" for the definition of an adulteration as given by Dr. Hassall, of London, who has done more than any other to draw public attention to the alarming facts of this species of fraud. He says an adulteration consists in the intentional addition to an article, for the purpose of gain or deception, of any substance or substances, the presence of which is not acknowledged in the name under which the article is sold. The change of an article entirely, and selling it under a false name in place of another, constitutes a *substitution*, and not an adulteration.

Thus briefly referring to the introductory of our last year's report, we would say that all the views we there express we still sustain. We have, during the past year, seen nothing to cause us to change our opinion as to the dangerous and hurtful tendency of this unlawful practice; on the contrary, all our experiences confirm us in the views we there express, and tend to convince us more fully that the public do not know the extent to which they are cheated, nor would any intelligent community submit to the imposition were they once fully aware of the danger and risk they are exposed to.

Adulterations are practised for three principal reasons, as briefly stated by the author of "Falsification of Food:—

1. For the purpose of making the substance more saleable, by improving its appearance by the addition of some body, either innocuous or otherwise.
2. To depreciate the quality by adding some substance which will diminish the *real*, without altering the *apparent* strength or general appearance. This is generally a very deadly fraud.
3. To depreciate quality and "extend" the quantity, by the addition of some simple substance, as water, or, if a solid body, as sand, gypsum, &c.

The matter of adulteration of articles of food is quite properly within the scope of a report on Home Adulterations; and your Committee propose entering somewhat largely into this part of the subject, trusting that the Association will grant them indulgence.

It is a sad and alarming fact that "death lurks in the pot;" that the community are exposed to injury, disease, and death, by the very food they live upon, and the tea, coffee, and water they drink. It is often urged in deference to many of these nefarious practices, such as the addition of some of the salts of copper and lead, or cocculus Indicus, to articles of daily consumption, that they are "used in *very small* quantities." We grant the statement to be true, and are deeply, we trust truly, grateful, as we ought to be, that we are not murdered

outright, but scientifically and handsomely killed by inches.

Who can estimate the results of the continued introduction into the system, day by day, and month by month, of these poisonous additions to articles of food? No substance, however small its daily use, which is decidedly poisonous and pernicious, can be used without producing, finally, disease, and even death. Any physician can tell you of cases where the seeds of painful and lingering disease have been planted by some slow, insidious poison. All of you are, doubtless, aware that disease often arises from the use of water through lead pipes; and who can say that of the many cases of "unknown disease," some of them may not have been caused by poison taken in the small but oft-repeated doses of the daily food.

We are aware that many—and among them even those of scientific reputation—scout the idea of any trouble arising from water through lead; but your Committee feel that it is of such great importance that they cannot pass it by without a brief notice. It is a question upon which scientific men have differed; but, with the present knowledge of the subject that we have, your Committee can but recommend, in every practical case, the use of some other substance than lead for the conveyance of water to be used for drinking purposes.

Where so large a possibility of injury exists, prudence would dictate an entire avoidance of the danger.

One of your Committee has been fully convinced of the great danger attending the use of water through lead pipe. He has known lead to be taken from a portion of the liver of a person who died with every symptom of acute lead poisoning, and who was considered by the attending physician to be a victim of lead disease.

The susceptibility of individuals to this poison varies very greatly; many persons apparently suffer no inconvenience, although subjected to precisely the same exposure which proves fatal to others.

One of the most striking cases of extreme susceptibility to this species of poisoning which has come to the knowledge of your Committee is the following:—

A man, aged thirty-five, who was a house-servant in the city at the time of his sickness, had never been exposed to the influence of lead, as far as could be ascertained, previous to entering his place of service. He was employed by a private family, consisting of persons both older and younger than himself, and who had drank the water through the lead pipe for several years, in fact, ever since the introduction of the water into the city, without the slightest apparent injury. The man referred to was from the country, and perfectly healthy and strong upon his arrival in the city; he was a fleshy and robust man, and never had noticed any indications of paralysis. He had been, in his situation as servant, exposed to lead poisoning in no other way than drinking the same water that the family used, only three months, before he was attacked with violent symptoms of lead paralysis, without the premonitory symptoms of colic. He was completely paralysed, excepting the muscles of respiration and the heart. Even the muscles of the eye-lids were useless, and the "blue line" upon the gums was apparent, which is the well-known and usual attendant upon cases of lead-poisoning. Under treatment of iodide of potassium he completely recovered.

If such serious results can occur, is not the matter one worthy the attention of those who have in charge the public health?

The singular mode in which the lead is sometimes introduced into the system also calls for care and watchfulness, to prevent the unknown, and, therefore, unsuspected danger.

One of the members of your Committee was called upon by a physician who had a patient, a farmer, suffering apparently from lead poison, to make an analysis of some cider. From what source the lead was taken was a mystery. The water used by the family was all drawn by an old-fashioned "well-sweep" and bucket; no possibility seemed to exist that lead could be the cause, and yet all the symptoms were those of a plainly-marked case of lead-poisoning. The attending physician finally thought of the cider which the farmer was in the habit of drinking, and, as a matter of extreme precaution, decided to have a portion of it analysed. From less than two quarts of that cider, metallic lead was obtained in substance sufficiently large to hold in the hand and prove its nature beyond question. This caused an examination of the cask of cider, and from the bottom of the cask, upon draining off the cider, was taken a pint bowlful of mixed sediment and white lead. This, of course, readily accounted for the farmer's lead disease. The cask used had formerly been filled with oil, and, in putting it together, the edges of the staves had been plentifully coated with lead, ground in oil, before driving the hoops, to prevent the possibility of leakage; afterwards, when used for cider, the lead was dissolved out, and the serious consequences followed.

These instances, taken from many, are given to show that those who speak and feel warmly on this subject of lead-poisoning have some reason for their opinions and feelings.

We are aware that this is not strictly an adulteration, but certainly, after all that has been said and written upon this subject, those who, being aware of the facts in the case, and who could avert the danger, go on deliberately using lead pipes for drinking-waters, place themselves in the situation of the man who sets spring guns and gets shot himself. When lead pipes are used in preference because they are "cheaper" than gutta percha, block tin, or iron, does not this constitute an adulteration of the water?

We are lead to speak of this subject of lead-water, perhaps going beyond the limits which the Association have laid down for our Committee, by having met with and known many instances where sad trouble has been produced by this often despised cause.

The next article we would ask your attention to is milk. During the past year it has been the duty of one of your Committee to analyse a large number of specimens of milk. As many of you are aware, there exists a statute in some States regarding the sale of adulterated and unwholesome milk, and by virtue of that statute an Inspector of Milk is appointed by the authorities.

During the past year a large number of specimens have been analysed at the request of the Inspector of Milk for the city of Boston. With but one single exception, every specimen was found to be adulterated, many of them very largely.

The plan adopted has been to compare the amount of food contained in any milk examined with that which, by the analysis of a very large number of specimens of pure cow's milk, was found to be the average amount. The amount of food being ascertained in any given specimen, it is easy, by comparison, to find the relative worth of the sample to the standard, and thus ascertain the dilution with water.

The amount of milk-sugar and salts was also estimated,

and the difference between the weight of the milk started with and the sum of the weights of the food, milk-sugar, and salts, gave the weight of the water in any sample analysed. The amount of water which any milk contained was not made the basis of comparison at all; the addition of water to milk increases in a much larger degree the worth of the milk as a food yielding fluid than it increases the *per centage* amount of water, and as the amount of food really constitutes the value of the milk, the analyses were placed on this alone.

Water is the principal adulteration of milk; but, to conceal this fraud and destroy the "skim milk" appearance, recourse must be had to several other adulterations, as the addition of colouring-matter, salt, &c., to increase the weight and improve the flavour, both of which it does, the latter quite remarkably.

Sometimes poor milk, from cows that should not be milked at all, owing to their condition, is mixed with that sent to market. It is only by microscopical examination that this unwholesome and unclean milk can be detected. Specimens of this kind have been met with during the past year.

From the experience one of your Committee has had, he does not hesitate to say that the adulteration of milk is largely and systematically carried on. Who can judge of the amount of sickness caused to children by the use of this unwholesome, and oftentimes poisonous food,—poisonous, because decomposition has already commenced.

Much of the sickness among young children in many large cities may be fairly attributed to the bad and unhealthy milk which they almost of necessity receive; and to the poorer classes, those less able by other reasons to contend with such a diet, the greater portion of this milk is given.

The milk oftentimes is not served to consumers until the second day after it is taken from the cow, owing to its being transported long distances, and when diluted with water the process of decomposition is much more rapidly set up.

*On Santonate of Protoxide of Mercury,
by Dr. JOHN MULLER, of Berlin.*

THE solutions of equal parts of protonitrate of mercury and santonate of soda, in distilled water, are mixed; after 24 hours the precipitate is separated by a filter, well washed with distilled water, dried at a moderate heat, and preserved in bottles excluded from the light.

The protosantonate of mercury is a whitish powder, somewhat crystalline under a magnifier, inodorous, and of a slight metallic taste, which is afterwards bitterish and very persistent. It is insoluble in water and alcohol, unaltered at 212°, charred at a high temperature, and decomposed into mercury and red oxide of mercury; lime-water decomposes it into soluble santonate of lime and black oxide of mercury.

A solution of sulphide of potassium decomposes it at 212°, forming black sulphide of mercury and soluble santonate of potassa. A warm solution of iodide of potassium yields with it greenish iodide of mercury and santonate of potassa.

Exposed with alcohol to the temperature of boiling water, a turbidity is produced on the addition of water, but no precipitate. Thereby the salt is distinguished from santonin, which, under the same circumstances, is precipitated. When treated with diluted nitric acid, protonitrate of mercury is dissolved and pure santonin left behind.

Other acids show the same behaviour as nitric acid, and alkalies decompose it like lime-water.—*Arch. der Pharm.*

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures (adapted to a Juvenile Auditory), on the Chemical History of a Candle; by M. FARADAY, D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.

LECTURE IV. (Jan. 3, 1861.)

Products: Water from the Combustion—Nature of Water—A Compound—Hydrogen.

I see you are not tired of the candle yet, or I am sure you would not be interested in the subject in the way you are. When our candle was burning we found it produced water exactly like the water we have around us; and by further examination of this water we found in it that curious body, hydrogen—that light substance of which there is some in this jar. We afterwards saw the burning powers of that hydrogen, and that it produced water. And I think I introduced to your notice an apparatus which I very briefly said was an arrangement of chemical force, or power, or energy, so adjusted as to convey its power to us in these wires; and I said I should use that force to pull the water to pieces, to see what else there was in the water besides hydrogen; because, you remember, when we passed the water through the iron tube, we by no means got the weight of water back which we put in in the form of steam, though we had a very large quantity of gas evolved. We have now to see what is the other substance present. That you may understand the character and use of this instrument let us make an experiment or two. Let us put together, first of all, some substances, knowing what they are, and then see what that instrument does to them. There is some copper (observe the various changes which it can undergo), and here is some nitric acid, and you will find that this being a strong chemical agent will act very much when I add it to the copper. It is now sending forth a beautiful red vapour; but as we do not want that vapour, Mr. Anderson will hold it near the chimney for a short time, that we may have the use and beauty of the experiment without the annoyance. The copper which I have put into the flask will dissolve: it will change the acid and the water into a blue fluid containing copper and other things, and I purpose then showing you how this voltaic battery deals with it; and in the meantime we will arrange another kind of experiment for you to see what power it has. This is a substance which is to us like water—that is to say, it contains bodies which we do not know of as yet, as water contains a body which we do not know as yet. Now this solution of a salt I will put upon paper and spread about, and apply the power of the battery to it, and observe what will happen. Three or four important things will happen which we shall take advantage of. I place this wetted paper upon a sheet of tinfoil, which is convenient for keeping all clean, and also for the advantageous application of the power; and this solution, you see, is not at all affected by being put upon the paper or tinfoil, nor by anything else I have brought in contact with it yet, and which, therefore, is free to us to use as regards that instrument. But first let us see that our instrument is in order. Here are our wires. Let us see whether it is in the state in which it was last time. We can soon tell. As yet, when I bring them together, we have no power, because the conveyers—what we call the electrodes—the passages or ways for the electricity—are stopped; but now Mr. Anderson by that [referring to a sudden flash at the ends of the wires] has given me a telegram to say that it is ready. Before I begin our experiment I will get Mr. Anderson to break contact again at the battery behind me, and we will put a platinum wire across to connect the poles, and then if I find I can ignite a pretty good length of this wire we shall be safe in our experiment. Now you will see the power. [The connection

was established, and the intermediate wire became red hot.] There is the power running beautifully through the wire, which I have made thin on purpose to show you that we have those powerful forces; and now, having that power we will proceed with it to the examination of water.

I have here two pieces of platinum, and if I lay them down upon this piece of paper [the moistened paper on the tinfoil] you will see no action; and if I take them up there is no change that you can see, but the arrangement remains just as it was before. But now see what happens: if I take these two poles and put either one or the other of them down separately on the platinum plates, they do nothing for me, both are perfectly without action; but if I let them both be in contact at the same moment, see what happens [a brown spot appeared under each pole of the battery]. Look here at the effect that takes place, and see how I have pulled something apart from the white—something brown; and I have no doubt, if I were to arrange this, and were to put one of the poles to the tinfoil on the other side of the paper, why, I get such a beautiful action upon the paper, that I am going to see whether I cannot write with it—a telegram if you please [the Lecturer here traced the word "juvenile" on the paper with one of the terminal wires]. See there how beautifully we can get our results.

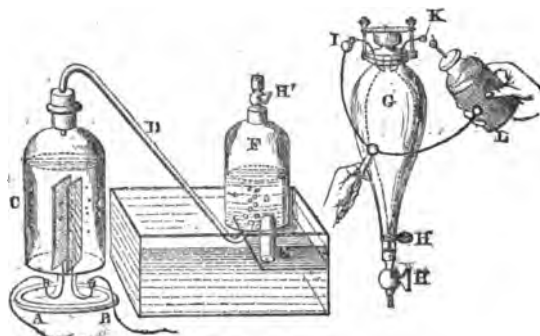
You see we have here drawn something, which we have not known about before, out of this solution. Let us now take that flask from Mr. Anderson's hands, and see what we can draw out of that. This, you know, is a liquid which we have just made up from copper and nitric acid, whilst our other experiments were in hand, and though I am making this experiment very hastily, and may bungle a little, yet I prefer to let you see what I do rather than prepare it beforehand.

Now see what happens. These two platinum-plates are the two ends (or I will make them so immediately) of this apparatus; and I am about to put them in contact with that solution, just as we did a moment ago on the paper. It does not matter to us whether the solution be on the paper or whether it be in the jar, so long as we bring the ends of the apparatus to it. If I put the two platinum in by themselves they come out as clean and as white as they go in [inserting them into the fluid without connecting them with the battery]; but when we take the power and lay that on [the platinum were connected with the battery and again dipped into the solution], this, you see [exhibiting one of the platinum], is at once turned into copper, as it were; it has become like a plate of copper; and that [exhibiting the other piece of platinum] has come out quite clean. If I take this coppered piece and change sides, the copper will leave the right hand side and come over to the left side; what was before the coppered plate comes out clean, and the plate which was clean comes out coated with copper; and you thus see that what copper we put into this solution we can also take out of it by means of this instrument.

Putting that solution aside, let us now see what effect this instrument will have upon water. Here are two little platinum-plates which I intend to make the ends of the battery, and this, (c) is a little vessel so shaped as to enable me to take it to pieces, and show you its construction. In these two cups (A and B), I pour mercury, which touches the ends of the wires connected with the platinum-plates. In the vessel (c), I pour some water containing a little acid (but which is put only for the purpose of facilitating the action, it undergoes no change in the process), and connected with the top of the vessel is a bent glass tube (D), which may remind you of the pipe which was connected with the gun barrel in our furnace experiment, and which now passes under the jar (F). I have now adjusted this apparatus, and we will proceed to affect the water in some way or other. In the other case, I sent the water through a tube which was made red hot; I am now going to pass the electricity through the inside of this vessel. Perhaps I may boil the water; if I do boil the water I shall get steam; and you know that steam condenses when it gets cold, and you will therefore see by that, whether I do boil the water or not. Perhaps, however, I shall not boil the water, but produce some other effect. You shall have the experiment and see. There is one wire which I will put to this side (A), and here is the other wire which I will put to the other side (B), and you will soon see whether any disturbance takes place. Here it is seeming to boil up famously; but does it boil? Let us see whether that which goes out is steam or not. I think you will soon see the jar

¹ Reported verbatim by special permission.

(F), will be filled with vapour, if that which rises from the water is steam. But can it be steam? Why, certainly not; because there it remains, you see, unchanged. There it is standing over the water, and it therefore cannot be steam, but must be a permanent gas of some sort. What is it? Is it hydrogen; is it steam; is it anything else? Well, we will

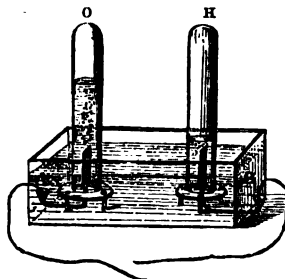


examine it. If it is hydrogen it will burn. [The Lecturer then ignited the gas collected, which burnt with an explosion.] It is certainly something combustible, but not combustible in the way that hydrogen is. Hydrogen would not have given you that noise, but the colour of that light when the thing did burn was like that of hydrogen; it will, however, burn without contact with the air. That is why I have chosen this other form of apparatus, for the purpose of pointing out to you what are the particular circumstances of this experiment. In place of an open vessel, I have taken one that is closed; (our battery is so beautifully strong, that we are even boiling the mercury, and getting all things right,—not wrong, but vigorously right); and I am going to show you that that gas, whatever it may be, can burn without air; and in that respect differs from a candle, which cannot burn without the air. And our manner of doing that is as follows:—I have here a glass vessel (G) which is fitted with two platinum wires (I, K), through which I can apply electricity; and we can put the vessel on the air-pump and exhaust the air, and when we have taken the air out we can bring it here and fasten it on to this jar (F), and let that gas into the vessel which was formed by the action of the voltaic battery upon the water, and which we have produced by changing the water into it,—for I may go as far as this, and say we have merely, by that experiment, changed the water into that gas. We have not only altered its condition, but we have changed it really and truly into that gaseous substance; and all the water is there which was decomposed by the experiment. As I screw this vessel (G, H) on here (N), and make the tubes well connected, and when I open the stop-cocks (H, N), if you watch the level of the water (in F), you will see that that gas will rise. Now, I will close the stop-cocks, as I have drawn up as much as that vessel can hold, and being safely conveyed into that chamber, I will pass into it an electric spark from this Leyden jar (L), and the vessel, which is now quite clear and bright, will become dim. There will be no sound, for the vessel is strong enough to confine the explosion. [A spark was then passed through the jar, when the explosive mixture was ignited.] Did you see that brilliant light? If I again screw the vessel on to the jar, and open these stop-cocks, you will see that the gas will rise a second time. [The stop-cocks were then opened.] Those gases [referring to the gases first collected in the jar, and which had just been ignited by the electric spark] have disappeared, as you see: their place is vacant, and fresh gas has gone in. Water has been formed of them; and if we repeat our operation [repeating the last experiment], I shall have another vacancy, as you will see by the water rising. I always have an empty vessel after the explosion, because the vapour or gas into which that water has been resolved by the battery, explodes under the influence of the spark, and changes into water; and by-and-by you will see in this upper vessel some drops of water trickling down the sides and collecting at the bottom.

We are here dealing with water entirely, without reference to the atmosphere. The water of the candle had the atmosphere helping to produce it; but in this way it can be produced independently of the air. Water, therefore, ought to

contain that other substance which the candle takes from the air, and which, combining with the hydrogen, produces water.

Now, you saw that one end of this battery took hold of the copper, extracting it from the vessel which contained the blue solution. It was effected by this wire; and surely we may say if the battery has such power with a metallic



solution which we made and unmade, may we not think that it is possible that it can split asunder the component parts of the water, and put them into this place and that place? Suppose I take the poles—the metallic ends of this battery—and see what will happen with the water in this apparatus (Fig. 2) where we have separated the two ends far apart. I place one here (at A), and the other there (at B), and I have little shelves with holes which I can put upon each pole, and so arrange them that whatever escapes from the two ends of the battery will appear as separate gases; for you saw that the water did not become vaporous, but gaseous. The wires are now in perfect and proper connection with the vessel containing the water, and you see the bubbles rising; let us collect these bubbles and see what they are. Here is a glass cylinder (O), I fill it with water and put it over one end (A) of the pile, and I will take another (H) and put it over the other end (B), of the pile. And so now we have a double apparatus, with both places delivering gas. Both these jars will fill with gas. There they go, that to the right (H) filling very rapidly; the one to the left (O) filling not so rapidly; and though I have allowed some bubbles to escape, yet still the action is going on pretty regularly, and were it not that one is rather smaller than the other, you would see that I should have twice as much in this (H), as I have in that (O). Both these gases are colourless; they stand over the water without condensing; they are alike in all things—I mean in all apparent things; and we have here an opportunity of examining these bodies and ascertaining what they are. Their bulk is large, and we can easily apply experiments to them. I will take this jar (H) first, and will ask you to be prepared to recognise hydrogen.

Think of all its qualities—the light gas which stood well in inverted vessels, burning with a pale flame at the mouth of the jar, and see whether this gas does not satisfy all these conditions. If it be hydrogen it will remain here while I hold this jar inverted. [A light was then applied and the hydrogen burnt.] What is there now in the other jar? You know that the two together made an explosive mixture. But what can this be which we find as the other constituent in water, and which must therefore be that substance which made the hydrogen burn? We know that the water we put into the vessel consisted of the two things together. We find one of these is hydrogen: what must that other be which was in the water before the experiment, and which we now have by itself? I am about to put this lighted splinter of wood into the gas. The gas itself will not burn, but it will make the splinter of wood burn. [The Lecturer ignited the end of the wood and introduced it into the jar of gas.] See how it invigorates the combustion of the wood, and how it makes it burn far better than the air would make it burn, and now you see by itself that every other substance which is contained in the water, and which, when the water was formed by the burning of the candle, must have been taken from the atmosphere. What shall we call it, A, B, or C? Let us call it O—call it "Oxygen;" it is a very good distinct-sounding name. This, then, is the oxygen which was present in the water, forming so large a part of it.

We shall now begin to understand more clearly our

experiments and researches; because when we have examined these things once or twice we shall soon see why a candle burns in the air. When we have in this way analysed the water—that is to say, separated, or electrolysed its parts out of it, we get two volumes of hydrogen, and one of the body that burns it. And these two are represented to us on this diagram, with their weights also stated, and

1 Hydrogen	8 Oxygen	Oxygen 88.9
	9	Hydrogen 11.1
		Water 100.0

we shall find that the oxygen is a very heavy body by comparison with the hydrogen. It is the other element in water.

I had better, perhaps, tell you now how we get this oxygen abundantly, having shown you how we can separate it from the water. Oxygen, as you will immediately imagine, exists in the atmosphere; for how should the candle burn to produce water without it? Such a thing would be absolutely impossible, and chemically impossible without oxygen. Can we get it from the air? Well, there are some very complicated and difficult processes by which we can get it from the air; but we have better processes. There is a substance called the black oxide of manganese; it is a very black-looking mineral, but very useful, and when made red hot it gives out oxygen. Here is an iron bottle which has had some of this substance put into it, and there is a tube fixed to it, and a fire ready made, and Mr. Anderson will put that retort into the fire, for it is made of iron, and can stand the heat. Here is a salt called chlorate of potassa, which is now made in large quantities for bleaching, and chemical and medical uses, and for gunpowder and other purposes. I will take some and mix it with some of the oxide of manganese (oxide of copper, or oxide of iron would do as well), and if I put these together in a retort far less than a red heat is sufficient to evolve this oxygen from the mixture. I am not preparing to make much, because we only want sufficient for our experiments; only, as you will see immediately, if I use too small a charge the first portion of the gas will be mixed with the air already in the retort, and I should be obliged to sacrifice the first portion of the gas because it would be so much diluted with air; the first portion must, therefore, be thrown away. You will find in this case that a common spirit lamp is quite sufficient for me to get the oxygen, and so we shall have two processes going on for its preparation. See how freely the gas is coming over from that small portion of the mixture. We will examine it and see what are its properties. Now, in this way we are producing, as you will observe, a gas just like the one we had in the experiment with the battery, transparent, undissolved by water, and presenting the ordinary visible properties of the atmosphere. (As this first jar contains the air, together with the first portions of the oxygen set free during the preparation, we will carry it out of the way, and be prepared to make our experiments in a regular, dignified manner.) And inasmuch as that power of making wood, wax, or other things burn, was so marked in the oxygen we obtained by means of the voltaic battery from water, we may expect to find the same property here. We will try it. You see there is the combustion of a lighted taper in air, and here is its combustion in this gas [lowering the taper into the jar]. See how brightly and how beautifully it burns;—you can also see more than this,—you will perceive it is a heavy gas, whilst the hydrogen would go up like a balloon, or even faster than a balloon, when not encumbered with the weight of the envelope. You may easily see that although we obtained from water twice as much in volume of the hydrogen as of oxygen, it does not follow that we have twice as much in weight; because one is heavy and the other a very light gas. We have means of weighing gases or air; but without stopping to explain that, let me just tell you what their respective weights are. The weight of a pint of hydrogen is three-quarters of a grain; the weight of the same quantity of oxygen is nearly twelve grains. This is a very great difference. The weight of a cubic foot of hydrogen is one-twelfth of an ounce; and the weight of a cubic foot of oxygen is one ounce and a third. And so on we might come to masses of matter which may be weighed in the balance, and which we can take account of as to hundred-weights and as to tons, as you will see almost immediately.

Now as regards this very property of oxygen supporting combustion, which we may compare to air, I will take a

piece of candle to show it you in a rough way,—and the result will be rough. There is our candle burning in the air: how will it burn in oxygen? I have here a jar of this gas, and I am about to put it over the candle for you to compare the action of this gas with that of the air. Why, look at it; it looks something like the light you saw at the poles of the voltaic battery. Think how vigorous that action must be! And yet during all that action nothing more is produced than what is produced by the burning of the candle in air. We have the same production of water; and the same phenomena exactly, when we use this gas instead of air, as we have when the candle is burnt in air.

But now we have got a knowledge of this new substance, we can look at it a little more distinctly, in order to satisfy ourselves that we have got a good general understanding of this part of the product of a candle. It is wonderful, you see, how great the supporting powers of this substance are as regards combustion. For instance, here is a lamp which, simple though it be, is the original, I may say, of a great variety of lamps which are constructed for divers purposes,—for lighthouses, microscopic illuminations, and other uses; and if it was proposed to make it burn very brightly, you would say, "If a candle burnt better in oxygen, will not a lamp do the same?" Why, it will do so. Mr. Anderson will give me a tube coming from our oxygen-reservoir, and I am about to apply it to this flame, which I will previously make burn badly on purpose. There comes the oxygen: what a combustion that makes! But if I shut it off, what becomes of the lamp? [The flow of oxygen was stopped, and the lamp relapsed to its former dimness.] It is wonderful how, by means of oxygen, we get combustion accelerated. But it does not affect merely the combustion of hydrogen, or carbon, or the candle; but it exalts all combustions of the common kind. We will take one which relates to iron for instance, as you have already seen iron burn a little in the atmosphere. Here is a jar of oxygen, and this is a piece of iron wire; but if it were a bar as thick as my wrist, it would burn the same. I first attach a little piece of wood to the iron, I then set the wood on fire, and let them both down together into the jar. The wood is now alight, and there it burns as wood should burn in oxygen; but it will soon communicate its combustion to the iron. The iron is now burning brilliantly, and will continue so for a long time. As long as we supply oxygen, so long can we carry on the combustion of the iron, until the latter is consumed.

We will now put that on one side, and take some other substance; but we must limit our experiments, for we have not time to spare for all the illustrations you would have a right to, if we had more time. We will take a piece of sulphur: you know how sulphur burns in the air; well, we will put it into the oxygen, and you will see that whatever can burn in air can burn with a far greater intensity in oxygen, leading you to think that perhaps the atmosphere itself owes all its power of combustion to this gas. The sulphur is now burning very quietly in the oxygen; but you cannot for a moment mistake the very high and increased action which takes place when it is so burnt, instead of being burnt merely in common air.

I am now about to show you the combustion of another substance—phosphorus. I can do it better for you here than you can do it at home. This is a very combustible substance, and if it be so combustible in air, what might you expect it would be in oxygen? I am about to show it to you not in its fullest intensity, for if I did so we should almost blow the apparatus up; I may even now crack the jar, though I do not want to break things carelessly. You see how it burns in the air. But what a glorious light it gives out when I introduce it into oxygen! [Introducing the lighted phosphorus into the jar of oxygen.] There you see the solid particles going off which cause that combustion to be so brilliantly luminous.

Thus far we have tested this power of oxygen and the high combustion it produces, by means of other substances. We must now, for a little while longer, look at it as respects the hydrogen. You know when we allowed the oxygen and the hydrogen derived from the water to mix and burn together we had a little explosion. You remember also that when I burnt the oxygen and the hydrogen in a jet together, we got very little light but great heat; I am now about to set fire to oxygen and hydrogen mixed in the proportion in which they occur in water. Here is a vessel containing one volume of oxygen and two volumes of hydrogen. This mixture is exactly of the same nature as

the gas we just now obtained from the voltaic battery; it would be far too much to burn at once; I have therefore arranged to blow soap bubbles with it and burn those bubbles, that we may see by a general experiment or two how this oxygen supports the combustion of the hydrogen. First of all we will see whether we can blow a bubble. Well, there goes the gas [causing it to issue through a tobacco-pipe into some soap-suds]. Here I have a bubble. I am receiving them on my hand, and you will perhaps think I am acting oddly in this experiment, but it is to show you that we must not always trust to noise and sounds, but rather to real facts. [Exploding a bubble on the palm of his hand.] I am afraid to fire a bubble from the end of the pipe because the explosion would pass up into the jar and blow it to pieces. This oxygen then will unite with the hydrogen, as you see by the phenomena, and hear by the sound, with the utmost readiness of action, and all its powers are then taken up in its neutralisation of the qualities of the hydrogen.

So now I think you will perceive the whole history of water with reference to oxygen and the air, from what we have before said. Why does a piece of potassium decompose water? Because it finds oxygen in the water. What is set free when I put it in the water, as I am about to do again? It sets free hydrogen, and the hydrogen burns; but the potassium itself combines with oxygen; and this piece of potassium, in taking the water apart,—the water, you may say, derived from the combustion of the candle,—takes away the oxygen which the candle took from the air, and so sets the hydrogen free; and even if I take a piece of ice, and put a piece of potassium upon it, the beautiful affinities by which the oxygen and the hydrogen are related are such, that the ice will absolutely set fire to the potassium. I show this to you to-day, in order to enlarge your ideas of these things, and that you may see how greatly results are modified by circumstances. There is the potassium on the ice, producing a sort of volcanic action.

It will be my place when next we meet, having pointed out these anomalous actions, to show you that none of these extra and strange effects are met with by us—that none of these strange and injurious actions take place when we are burning, not merely a candle, but gas in our streets, or fuel in our fire-places, so long as we confine ourselves within the laws that Nature has made for our guidance.

[The Fifth Lecture will appear in our next Number.]

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 8, 1861.

E. W. BINNEY, F.R.S., F.G.S., &c., Vice-President,
in the Chair.

Mr. RANSOME exhibited and explained the means of obtaining a photograph, which, although appearing a confused series of light and shade, yet, when reflected from a polished cylinder, was a beautiful picture.

Dr. CRACE CALVERT brought under the notice of the Society an interesting communication made to him by Professor Arnaudon, of Turin, to the effect that oxalate of ammonia completely modifies the action of yellow prussiate of potash when mixed in solution with a salt of peroxide of iron. Thus, if oxalate of ammonia be added to this metallic salt, it will give no prussian blue when a solution of yellow prussiate of potash is added; but on the addition of an acid, prussian blue is immediately produced. The knowledge of this fact may be interesting to calico printers, as it will give them the means of easily producing prussian blue on their fabrics. To attain this desirable end, the printer will simply have to pad his fabric through a mixture of persalt of iron and oxalate of ammonia, dry, and paint an acid where he wishes to produce the blue.—After Dr. Calvert had made a few more remarks on this subject, he stated that he had lately examined several varieties of Snuffs, which he had found to be more or less impregnated with lead compounds, especially the black rappee, and he found on further investigation that the presence of lead was due to the

corrosive action of the snuff upon the lead foil used for packing it. He also stated that it was his intention to examine several other substances usually packed in lead foils, and that he would lay the results of his observations before the Society, as he thought it highly desirable to make the public aware of such sources of injury and to induce manufacturers to adopt means to avoid inflicting this serious evil on their consumers. Dr. Calvert concluded by stating that he had been engaged for the last few months in investigating the action of the Manchester Waterworks water on various kinds of leaden pipes, and that he was arriving at such results as would show the necessity for serious consideration on the part of the inhabitants of this city with respect to the evils arising from the introduction of the water into their dwellings through leaden pipes.

Being requested by the Chairman to give his opinion, Dr. ANGUS SMITH said that he had never found any Manchester water which had passed through lead pipes to be entirely free from lead. At the same time, the quantity is in almost all cases so small that, as far as we know, it can produce no bad effects, and is practically equal to nothing. There is, however, a great difficulty in knowing what is hurtful; medical men had not settled the point. Persons said to be suffering from lead paralysis were known to have taken water with as little as one-hundredth of a grain of oxide of lead per gallon, whereas it was considered generally not to be hurtful until it contained one-fortieth of a grain. We have little idea of the extreme susceptibility of some persons, and it is better to avoid lead as much as possible. Short lead pipes may be used without fear in Manchester, especially if the water which has stood over night be thrown away. Long lead pipes should be avoided, and lead cisterns are extremely dangerous, especially with soft waters, including Manchester water. (Here instances were given.) Soft water dissolves lead more readily than many hard waters, and if the hardness be due only to the earthy carbonates, the lead becomes coated instead of being dissolved. But if the hardness be due to chlorides or nitrates, the water dissolves lead much more rapidly than pure water (referring to what he had written on this). He gave an instance of water from a cesspool obtaining, by oxidation of its impurities in a porous soil, much nitric acid, which, along with the chlorides always found in such cases, caused the adjacent water, when drawn up by a lead pump, to have a very strong taste of lead salts. It is remarkable that this water was drunk for some years, but ultimately caused the death of two or three persons. It is a mistake to suppose that pure water dissolves lead more than all impure waters. Some very pure natural waters dissolve lead simply because they contain chlorides, although in small quantities. Such waters sometimes come from clay slates and similar formations. As to peaty water, it was of two kinds, occasionally acid, with some action on lead, but in most cases alkaline, the peaty matter not dissolving lead. Some lead pipes were more easily affected than others. Dr. Smith gave an instance of a lead pipe, nearly an inch in thickness, with holes pierced through the sides in various places by the action of the water; others are much more equally corroded. He had spent a long time in obtaining a suitable coating for lead to protect the water from its action, and had not quite succeeded; but he had given his results to a friend, who had gone further, and, having obtained great success, patented the process. However, he was told that no one would buy lead pipes of the kind, as they cost half-a-crown per cwt. more than the ordinary ones. People complain of evils which they refuse to escape from. It is, however, better to avoid lead than to avoid pure water because of its action on lead.

A paper was read by Mr. EDWARD HULL, F.G.S., "On the Nature and Objects of Geological Surveys, with Special Reference to the Progress of the Geological Survey of

Lancashire and Cheshire." After describing the various sources, both natural and artificial, on which the geological surveyor depends for his conclusions, and according to which he is enabled to trace the boundaries of the formations, the author went on to observe that so generally is the value of such surveys recognised, that the Governments of nearly all the most civilised nations had undertaken their support. Amongst others, France, Belgium, Germany, Russia, the States of North America, the British Colonies, as Canada, New Columbia, India, Australia, and New Zealand. The Author then explained some details regarding the maps of Lancashire so far as they had been completed by the Geological Survey of Great Britain. Specimens of these maps, both of the one-inch and six-inch scales, were exhibited to the Society in the course of the evening.

MICROSCOPICAL SECTION.

December 17, 1866.

Letters were read from Sir LEOPOLD MCCLINTOCK, Mr. J. W. READ, of the Admiralty, and Dr. WALLICH, who accompanied the former in the *Bull Dog*, in the late expedition to the North Seas. Dr. Wallich kindly presented to the Section a few copies of his pamphlet on "Life in the Deep Sea," now circulating amongst the members.

A Letter was also read from Captain M. F. MAURY, of the U.S. Navy, promising to supply envelopes for soundings amongst the sperm whalers and other vessels trading to the Pacific Ocean, &c.

Specimens of incrustations from the boilers of the steamer *Edinburgh*, trading from Glasgow and Liverpool to New York; from the steamer *Rhone*, from Liverpool to Venice, Trieste, &c.; and from the steamer *Minho*, from Liverpool to Lisbon and Oporto, were received from Mr. W. A. Hayman, of Liverpool. The incrustations are as hard as marble, breaking with a crystalline fracture, and showing, by different coloured strata, the crust obtained from harbours and from the open sea. Mr. Dale stated that the component parts of the incrustations are sulphates of lime, magnesia, &c.; he recommended maceration in bicarbonate of ammonia to obtain calcareous shells, and in weak acids of muriate of barytes to obtain silicious shells. Various members took specimens for examination.

A Letter was read from Captain ANDERSEN, of the Cunard steamer *Canada*, from Liverpool to New York, accompanying specimens of the soundings taken during his last voyage across the Atlantic. Captain Andersen was kind enough to send the soundings by post from Queenstown, by which means they arrived just before the meeting.

Mr. W. H. HEYS, of Hazel Grove, exhibited his newly invented Kaloscope, by means of which he obtains refracted and reflected light of different colours at the same time upon objects under the microscope, producing beautiful effects in some cases.

NOTICES OF BOOKS.

On Coal Gas. By the Rev. W. R. BOWDITCH, B.A., F.C.S. Van Voorst. 1866.

THE substances obtained by the destructive distillation of vegetable matters, such as coal, peat, and wood, and not only the products proper, but the by-products, and even the impurities, have received, within the last few years, and are still constantly receiving, the most extensive and varied applications. Mr. Bowditch, of Wakefield, has given us some interesting details

concerning the purification of the most important of all these products of destructive distillation, namely, illuminating gas. The amplitude of the subject may be guessed from the fact that Mr. Bowditch's remarks, originally given in the form of a lecture delivered before some directors and managers of gas-works, though occupying about sixty pages, relate only, or at all events chiefly, to the detection and removal of one particular impurity invariably present in coal-gas. The presence of bisulphide of carbon in coal-gas had been ascertained before, but the author of the little work before us has shown us how to secure its absence by a happily-devised plan, at once simple and cheap, a great contrast to the tedious and costly processes which had been previously proposed. We intend to give some account of Mr. Bowditch's various experimental results, premising, however, that our remarks will also refer to the specifications which the author has given of his patent processes "for improvements in the purification of coal-gas," and to the corroborative testimony of several chemists who have reported upon the efficiency of his methods. As we proceed we shall point out one or two discrepancies between these different accounts which we have not been able to reconcile.

The book opens with a notice of Dr. Clayton's discovery of coal-gas (termed by him "Spirit of Coals"), in the year 1739. Next, we are told what substances are found in the gas which passes to the purifiers of a gas-works, after adequate condensation, these substances being divided into three classes,—illuminating substances, diluents, and impurities. The author says, "Of the impurities, I omit sulphuretted hydrogen and carbonic acid, because they are removable by the simplest means; and, except in the case of accident, cannot be found in gas supplied to consumers without most culpable negligence, or still more culpable parsimony." This is no doubt true as a general rule, but, unfortunately, in small country towns where the consumption of gas is not large, the former impurity may be often detected.

Ammonia is the first impurity to which the author devotes special notice. After stating that no impurity is more easily removed from coal-gas than ammonia, he goes on to describe the methods adopted:—"Where attempts are made to remove ammonia, the substances employed to that end are,—water, acids taken up by sawdust, metallic and earthy salts, and clay. I will say a word or two upon the advantages and disadvantages of each." He then states that water dissolves considerable quantities of the illuminating constituents of gas, and stops others by a mechanical action. "I am acquainted with a gas-works where five scrubbers are employed to wash the gas, four being placed between the condenser and the purifiers, and one between the purifiers and the gas-holders. Wigan coal is used and yet the illuminating power of the gas is but twelve candles. I asked the intelligent manager if he did not find a considerable quantity of hydro-carbon vapours removed even by the last scrubber, and he admitted that a considerable quantity could generally be obtained from the surface of a small well through which the water flowed from this scrubber on its passage to the store-tank." The hydro-carbons thus dissolved or mechanically separated by the water are, in reality, those of highest value, so far as illuminating power is concerned. Among them may be mentioned olefant gas, propylene, and other members of the same series as well as those hydro-carbons which present the composition and vapour density of the alcohol radicals (propyle, &c.) and which, if not identical with these bodies are isomeric with them. Thus it would seem to be with good reason, that Mr. Bowditch condemns the use of water as a purifying agent. And not only does it impoverish the gas, but it fails to separate the tar and sulphuretted carbon compounds present. Acids, and it would seem, metallic salts also present the same dis-

advantages as water. As illustrations of the disadvantage attendant upon the use of acids, the following instructive experiments are described. If through a tube containing deal sawdust, moistened with sulphuric acid, which has been diluted with six times its weight of water, gas be passed, a pink colouration, gradually, deepening in intensity, will be observed. With a gas giving the light of 14 candles for a consumption of 5 feet per hour, the colour is produced with moderate rapidity, but is not intense; and it is found that the better the gas the more intense is the colouration, and the more quickly does it take place. Mr. Bowditch has experimented with olefant gas, propylene and benzole, and in each case finds that if only a sufficient quantity of acidified sawdust be employed, the vapour of these valuable illuminating ingredients of gas, while they redden the sawdust are completely absorbed by it. He proposes the employment of the acidified sawdust as a method of ascertaining the light-giving power of different specimens of gas, by means of the depth of colour produced. The rationale of this absorption may be as follows. It is quite possible that the large surface of dilute sulphuric acid may exert the same absorbing effect on olefant gas and its homologues and on benzole which strong sulphuric acid under ordinary conditions does. If so, the sawdust after being used should yield the acids commonly known as sulphethylic, sulphopropylic, and sulphobenzolic.

Mr. Bowditch says, "Clay removes ammonia and its salts as completely as acids do. In this respect it is merely their equal, and if this were the limit of its power, it would not be entitled to any preference. *But it possesses properties and powers peculiar to itself, and exerts a beneficial influence which nothing else known to chemists can exert.*" Mr. Bowditch then relates a most important experiment. He takes gas which contains no sulphuretted hydrogen and which does not blacken lead-paper, passes it through a cold purifier containing clay, and then shows how powerful a darkening effect is produced on the test-paper then. Of course nothing is easier than to remove, by the ordinary lime process, the sulphuretted hydrogen now present, in which form the sulphur, previously existing as bisulphide of carbon, bisulphide of ethylene, &c., now appears in virtue of the action of the clay. The words which we have italicised above, where Mr. Bowditch states that clay is the only material capable of changing bisulphide of carbon and other similar sulphur compounds into the easily-removable sulphuretted hydrogen, though they re-appear frequently in different parts of the work before us, and are affirmed with emphasis, yet in his specification, filed before the publication of the work on gas, the same virtues are assigned to other substances, and especially to slaked lime at particular temperatures. More than this, the latter part of the book itself contains a complete account of the energetic action of monohydrate of lime upon the various injurious sulphur compounds of gas and we have seen the opinions and testimonials of Drs. Brande, Letheby, and Frankland, and of Messrs. Warrington and Keates, all of whom speak in the highest terms of the practical efficiency of that process invented by our author in which monohydrate of lime is employed at temperatures varying between 300° and 400° F., while we look in vain for a syllable concerning the process in which clay is employed, and to the description of which by far the greater part of Mr. Bowditch's lecture, and also of his specification is devoted.

The author finds that all the sulphur of gas appears as sulphuretted hydrogen when the gas is conducted over hydrate of lime at the particular temperatures specified above. He suggests applications both of the cold clay and of the hot lime process for domestic use, and gives all the necessary details for their employment on the large scale. The expense is quite trifling, and becomes quite unworthy of consideration when the enormous benefits

conferred by the adoption of the plan are taken into account. The lime or clay-process may be most advantageously adopted in the purification of coal-oils. The vapour of these liquids is to be passed over hydrate of lime heated to about the boiling-point of the liquid used, and the product afterwards washed with a dilute alkaline solution, to remove the dissolved sulphuretted hydrogen produced in the process.

The author makes the startling assertion that "naphthalin is a hurtful, sulphur-bearing, non-illuminating compound." We think, however, that we should like to have further experimental proofs of this statement than Mr. Bowditch affords. His explanation of the offensive state of the earth in contiguity with the gas-mains in streets is worthy of consideration, and is most probably correct. The sulphuretted hydrogen which blackens the London soil "results from the sulphur-compounds which the clay (in the soil) breaks up and re-arranges."

We hardly think Mr. Bowditch's method of detecting bisulphide of carbon in coal-gas (p. 35) an infallible one, for this sulphur-compound is not the only liquid possibly present in gas, which, while soluble in alcohol, is precipitated to the bottom of the containing vessel on the addition of six or seven volumes of water, a cloudiness or turbidity being at first produced. A confirmatory test should certainly be applied. The detection of sulpho-cyanogen compounds in coal-gas is not new, and it has long been known that bisulphide of carbon, in the presence of water, ammonia, and sulphuretted hydrogen (or of sulphide of ammonium in solution), yields a large quantity of sulpho-cyanide of ammonium.

In a volume such as that to which we are now directing our readers' attention, and which contains the history of a most important discovery, we are sorry to find some errors and oversights which the assistance of a person tolerably versed in the great principles of chemical science, in reading the proof sheets, would have enabled the author to avoid. At the same time, of course, we cannot but perceive how extensive and clear are Mr. Bowditch's views on the great practical questions on which he treats. It is likely that the book will soon reach a second edition, and it would undoubtedly be improved by one or two alterations in such points as the following. On page 19, the formula HCS_2 in accordance with the old and usually-received notation, is assigned to sulpho-carbonic acid, while on page 38, in an extract from a paper by Dr. Hofmann, the formula given to xanthate of potash in the original memoir, viz. $\text{K}(\text{C}_2\text{H}_3\text{O})\text{CS}_2\text{O}$, is passed over without notice, although the value assigned to the symbols C and S is not the same in both cases. We have not the original paper at hand, but we can hardly imagine that its author could have identified the terms "xanthate" and "sulpho-carbonate" as we find to be the case on page 38. An extract from Dr. Thompson's evidence on the Metropolitan Gas Bill is given on pages 47 and 48, and one of his erroneous statements is there corrected. But another passes unnoticed, for the statement that 94 grains of bisulphide of carbon contain or correspond to 4 grains of sulphur, remains unaltered, although it does not require a very profound knowledge to see that they correspond to exactly double this amount.

We have been given to understand that Mr. Bowditch's process is highly appreciated by the great gas companies, and will be at once adopted. We trust, however, that in the present case no attempt will be made to overthrow the validity of this gentleman's patent. We have heard with regret that the larger gas companies possess a common fund devoted solely or mainly to contest patent rights, and consequently, in many cases, to get rid of just claims. We can scarcely imagine that they would be successful in combating Mr. Bowditch's rights, for we are compelled to regard his discovery as totally new in theory, as it is admirably successful in practice.

CORRESPONDENCE.

Manufacture of Sulphuric Acid.

To the Editor of the CHEMICAL NEWS.

SIR,—While absent from England, I have heard that one of your correspondents has inquired for an apparatus to produce from one to seven tons of sulphuric acid per week.

Having devoted several years to gradually bringing to practical perfection an apparatus for this purpose, totally unlike the usual leaden chambers, and seeing great advantages in it, I shall be happy to communicate further with the party, either through your columns or (what would, perhaps, be less like an indirect advertisement) privately by letter, if he thinks well.

The basis of my system is the drawing the gases through coarse, rounded silicious sand, or equivalent material, in stoneware columns, all being put together with a cement which is indestructible by the acids used. The general principle has attracted the notice of several inventors and manufacturers; possibly, in some cases, independently of my patents; but its useful application requires arrangements which have not been so readily seen.

The great benefits of this system in my application of it are, that it presents a means of effecting the requisite chemical actions with great rapidity, or in a very small space, and very perfectly; and that the apparatus is indestructible, not needing the usual heavy expenses for repairs and renewals, nor any of the usual care lest it be destroyed by negligent working with too much nitre, or by lying unused for any time or at any intervals with the acids in it. It is well known that the usual leaden chambers, and their coke apparatus, or "scrubbers," are greatly at fault in meeting any of the aforesaid requirements.—I am, &c.

W. PETRIE.

Nouvelle Montagne Zinc Works, Engis, near Liege, Belgium.

The Adulteration of Food Act in the City.

To the Editor of the CHEMICAL NEWS.

SIR,—It is with feelings of surprise I read in the CHEMICAL NEWS the report of Dr. H. Letheby on Mustard. He admits the mustard was adulterated with flour and turmeric. Now, what is the intention of a manufacturer, when he adulterates any article, but to lessen the cost of producing the article in question? Now, here is a large manufacturer reducing the mustard with flour, and bringing up the colour with turmeric, both articles lower in price than the article vended.

The grocer is mulcted in the penalty of £100 if he mixes chicory with coffee, and yet chemists admit that chicory in itself does not contain anything injurious to health.

Surely the Act, and also the analysis, is a farce, for the only question for the manufacturer will be,—This article costs me 6d. per pound; I want something to adulterate it, but I must find some harmless substance to adulterate it with, so that it may be certified that the article is not injurious to health.—*Circumspice!*—I am, &c.

GEORGE J. JOHNSON.

A Curious Fact connected with Sounds, Smells, and Tastes of Various Intensities.

To the Editor of the CHEMICAL NEWS.

SIR,—It is very remarkable that faint sounds are different in kind, if human judgment is correct in this particular, to strong ones of the same origin, and that the same thing may be said of every taste and smell. A gentle rap with the knuckle upon a plate of iron produces a sound apparently different in kind to that which arises from a more

forcible stroke, similarly given, and this experience characterises every description of sound. And so the faint smell of a rose, and the taste of diluted brandy and gin, appear respectively to be different in kind from the strong smell and flavours. These are illustrations, not, I apprehend, the most striking, of a universal law.

It may properly be thought and inquired what is the criterion of difference in kind and degree? and this is a question to which it is impossible, in the present state of science, to give a satisfactory reply. If the production of sound is ever thoroughly understood, it will be possible to say when various sounds differ sufficiently to allow of a human distinction into kind and degree, and to refer to the judgment before this is simply to permit every individual to assume—seeing that the proof is impossible—whichever view he pleases. The former remark is of course only applicable to those sounds which are near to the division into kind and degree, inasmuch as, which applies to the two other sensations, and the optical sensation of black—there are sounds which it is exceedingly probable, if not certain, differ in kind, and may be extended to colour, taste, and smell of every description.

I am, &c. J. ALEXANDER DAVIES.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

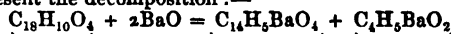
Phenomena Attending the Amalgamation of Zinc, Cadmium, and Iron.—M. J. Regnault has observed that the amalgamation of zinc with mercury is always attended with a diminution of temperature. (*Comptes-Rendus*, t. xli. p. 778.) Thus, when zinc and mercury at 10° C. are mixed in a tube, a thermometer placed in the mixture sinks to 7.5°. The amalgamation of iron is also attended with a condensation of heat. But when cadmium is amalgamated the temperature rises—a mixture of the two metals at 10° rising to 13°. The above facts, the author says, explain how it happens that amalgamated zinc is more electro-positive than zinc alone, and amalgamated cadmium more electro-negative than cadmium itself. Zinc becomes electro-positive because it fixes heat in combining with mercury; and cadmium becomes electro-negative because it parts with heat under the same circumstances. Amalgamated soft iron and steel also was found to be electro-positive relatively to the non-amalgamated metal. Tempered steel is proved by experiment to be electro-positive relatively to the same steel untempered, and, applying the above results, the author supposes that the latent heat of tempered steel is less than that of untempered, and this, he thinks, may be the cause of its remarkable mechanical properties.

Influence of Pressure on Chemical Action.—M. Favre has found (*Comptes-Rendus*, t. xli. p. 1027) by experiment that when hydrogen is generated from zinc and diluted sulphuric acid in a vessel hermetically closed, the action of the sulphuric acid diminishes as the pressure increases. This he ascribes to the adhesion of hydrogen to the surface of the metal, which protects it from the action of the acid. The author found also that the electrolysis of water by four of Bunsen's elements with platinum electrodes was not at all affected by a pressure of 70 or 80 atmospheres. No ozone was noticed in the gas which had submitted to the pressure, but when the pressure was removed the liquid from which the gas had been disengaged was found to retain a small quantity in solution, and this, when collected, was found to blue ozone paper and bleach sulphide of lead.

II. ORGANIC CHEMISTRY.

Decomposition of Ethers by Anhydrous Alkalies.—MM. Berthelot and de Fleurieu have studied the action of anhydrous alkalies on compound ethers (*Comptes-Rendus*, t. li. p. 1020), and find that when so acted on they split up

and form an acid and a compound corresponding to alcohol in which the equivalent of water is replaced by an equivalent of the alkali: for instance, they took 4 grammes of pure benzoic ether and 6 grammes of anhydrous baryta and placed them in a perfectly dry tube, which they then sealed and kept at a heat between 150° and 180° C. for five hours. After this time the tube was opened, and it was found that the benzoic ether had completely disappeared and no gas had been produced. The contents of the tube, when heated on a water bath, gave off no volatile matter, nor did they yield up anything to anhydrous ether. After being completely deprived of ether, water was added, and the mixture was rapidly distilled; immediately alcohol was obtained, the amount of which approached very nearly to the theoretical quantity. The liquid remaining in the retort was then treated with carbonic acid to remove the excess of baryta, and afterwards evaporated, by which means perfectly pure benzoate of baryta was obtained. The following equation will represent the decomposition:—



Benzoic Ether.

Benzoate of Baryta. Alcoholate.

In performing the above experiment care must be taken to manage the temperature, for if it should pass above 200° C. the benzoic ether is destroyed, and various products are formed which complicate the results.

When acetic ether is acted on by baryta, the temperature must be higher and more prolonged. Maintained at 200° C. for thirty hours, the results correspond exactly with the foregoing.

Acetic ether and lime exposed to 250° C. for a long time yielded no gas nor formed any secondary products. The mass contained neither free alcohol nor ordinary ether, but, under the influence of water, it was immediately resolved into alcohol and acetate of lime. In the presence of anhydrous alcohol the results were perfectly similar. With formic and oxalic ethers and baryta the results were similar to the above, but not quite so perfect, for some traces of gas and secondary products were obtained. The authors conclude that the action of anhydrous alkalis on ethers is comparable to that of the hydrated alkalis, the elements of the anhydrous alkali replacing the elements of water in the regenerated alcohol.

III. TECHNICAL CHEMISTRY.

Manufacture of Ice.—Although there is no lack of ice at the present time, it is worth knowing how to obtain it by a very simple process in the hottest summer. M. Carre (*Cosmos*, t. xvii. p. 686) takes two strong iron retorts (quicksilver bottles, they say, will answer), into one of which he puts a saturated solution of ammonia. When the two are connected, he places the empty one in a vessel of water, and the other over a fire. The heat drives over ammonia which is condensed by pressure in the previously empty retort. When the ammoniacal gas is liquefied, the one retort is removed from the fire and allowed to cool, whereupon the ammonia in the other rapidly assumes the gaseous form, abstracting so much heat from the water by which it is surrounded as to freeze a considerable quantity. The experiment is repeated without disconnecting the retorts. The cost of ice made in this way is said not to exceed a centime per kilogramme—or about fivepence a hundredweight.

MISCELLANEOUS.

Royal Institution.—The following Lectures will be delivered at this Institution during the ensuing week:—Tuesday, January 29, at 3 o'clock, Professor Owen "On Fishes." Thursday, January 31, at 3 o'clock, Professor Tyndall "On Electricity." Friday, February 1, at 8 o'clock, Rev. Alexander J. D. D'Orsey "On the Study of the English Language as an Essential Part of a University Course." Saturday, February 2, at 3 o'clock, Dr. E. Frankland "On Inorganic Chemistry."

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the *CHEMICAL NEWS*, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 6d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part I. of the present Volume will be ready for delivery with the Magazines for February.

*. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

*. All Editorial Communications are to be addressed to Mr. CROOKES and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the *CHEMICAL NEWS*, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

Tin Solder may be made by melting together equal weights of tin and lead. A more fusible solder may be formed of 3 equivalents of tin to one of lead; this will melt at 367° Fahr.

B. P.—Your article is received with many thanks. Can you favour us with the specimen and the continuation article referred to in your note?

W. W.—A full abstract of M. Caron's article on the reduction of calcium was given in our first volume. The reference to the original paper accompanied it.

Manufacture of Glue on the large scale.—A correspondent is desirous of knowing the best process for the above manufacture.

D. Clifton.—No fuller account was published than the one which appeared in our columns. If we are able to hear of further details we will give them insertion.

J. D. P.—We have decided to stitch the numbers in future before issuing them, this will get over the difficulty of the two loose pages which the recent increase of size has rendered necessary.

Edgar.—The red colouring matters named are slight modifications of the same body,—the violet colours are the same; the tints of each differ slightly according to method of preparation. Aniline can be made on the small scale by the method recommended in the article by M. Kopp. Methylated spirits will do in most cases where alcohol is mentioned—not in all.

W. E. R.—We will give the prices next week.

R. J.—Gerhart's continuation of Berzelius' "Chemistry" is a very good work for the advanced student. The new edition of Fownes' "Chemistry," which is shortly to appear, will contain a good account of the different systems of chemical nomenclature.

THE ADULTERATION OF FOOD.

Now ready, price 6d., by post 7d.

A TABLE (reprinted from the *CHEMICAL NEWS*, and Revised by the Author,) showing the more important Articles of FOOD or DRINK, and the Substances employed for Adulterating them. Especially suitable for Museums, Schools, Mechanics' and other Institutions, as well as for Private Reference.

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THE CHEMICAL NEWS

VOL. III. No. 61.—February 2, 1861.

THE ADULTERATION OF FOOD ACT.

OUR attention has been drawn to advertisements inviting applications for the appointment of Analyst, under the Adulteration Act, from *medical men* who are willing to take the fees receivable under the Act as their only remuneration. Such an advertisement, we imagine, could only have emanated from individuals who had entirely misunderstood the Act, or who were anxious to render it as inoperative as possible. In the first place, it is not at all necessary that the Analyst should be a medical man. All that the Act requires is, that he should possess "competent medical, chemical, and microscopical knowledge." But the amount of medical knowledge required, in order to know that a poisonous adulteration would be detrimental to health, is extremely small, while the chemical knowledge and experience required to detect the adulteration may be very great. We hold the medical profession in great respect, but, at the same time, we may safely assert that, while every competent chemist possesses sufficient medical knowledge, very few medical men have the chemical skill necessary for effectually carrying out the purposes of the Act. And what would be the effect of appointing an incompetent chemist? A timid Analyst would of course be anxious to err on the safe side. Uncertain about the composition of a substance, he could carefully guard himself by certifying that it "contained nothing detrimental to health," and then no proceedings could be taken, nothing in the Act of Parliament requiring him to state what is the actual composition of the article brought to him for analysis; and, even if he accepted the highest fee allowed by the Act as his only remuneration, he might almost be excused from troubling himself to ascertain it. The framers of the Adulteration Act well understood that the fees they made receivable were very insufficient rewards for the performance of analyses. The Act, therefore, expressly states that the districts appointing an Analyst may pay him "such salary or allowances as they may think fit,"—clearly, in addition to, or in place of the fees. Nor would such a payment out of the rates be at all inconsistent with ordinary practice, as some seem to think. The Analyst must be regarded as a sanitary detective policeman, who, like every other policeman, is employed as much to prevent as to detect crime; and if the knowledge that one had been appointed in a district should tend, as it probably would, to prevent adulteration, the ratepayers would be the gainers by the appointment.

But, in any case, we hope we shall not have to record that any gentleman has expressed his willingness to accept the office for the mere sake of the fees. If he did, the least he could do would be to exact the highest, which, in all probability, would prevent him from receiving any. Supposing, however, articles should be brought, for what analysis is ten shillings and sixpence an adequate remuneration? It is clear that only the most superficial examinations would be made, the most

carefully-guarded certificates given, and the object of the Act entirely defeated.

But the more we consider the Adulteration Act, the more valueless does it appear. We may take one glaring illustration. A few years ago an ingenious Frenchman came to London with a secret, which quite enchanted the workhouse authorities, who allowed him to exhibit his skill. He showed them that it was possible to make a sack of flour into nearly twice the number of loaves they had made before, the individual loaves weighing exactly the same. The secret was the addition of a few pounds of rice, which, when made into dough, would, of course, hold a large quantity of water. Whether this sort of bread-and-water diet has been generally adopted for paupers we do not know, but certain it is that the Frenchman's secret has become extensively known to the baking trade, and one of the four articles taken to Dr. Letheby for analysis was some of this bread, which contained 44 per cent. of water. Now, for such a fraud as this the Adulteration Act provides no remedy, it not being, in the sense in which the words are used in the Act, an adulteration injurious to health. But it is a cheat of the most flagrant kind, and deserves the most summary punishment, unless, indeed, the bakers who practise it can be made to understand that they must regulate the price of bread as much by the rates of the Water Companies as the price of corn in Mark Lane.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Action of Citric and Tartaric Acids upon certain Metallic Sulphides, by FREDERICK FIELD.

MR. SPILLER, in a paper upon various substances which influence and modify chemical reactions,¹ gave us some very curious and interesting facts regarding the non-precipitation of sulphuric acid by a salt of baryta in the presence of citric acid or a citrate. After a detailed account of his experiments upon this particular head, Mr. Spiller extended his observations to the influence which citric acid or its salts exerted upon the sulphides and oxides of various metals, proving that many of the latter are not precipitated from their solutions by means of potash if a citrate is present; and, further, that the sulphide of manganese, and even of arsenic, are freely soluble in citrate of soda. As regards the metals in the group comprising iron, nickel, cobalt, manganese, and zinc, the author of the paper referred to, tells us that, with the exception of manganese, their precipitation as sulphides is not effected in the presence of a citrate. The action of citric acids upon these sulphides is not especially noticed.

The following experiments may be interesting, as exemplifying in a very remarkable degree the fact pointed out by Rose, many years ago, that the sulphides

¹ Quarterly Journal of the Chemical Society, vol. x. p. 110.

not precipitated from their slightly-acid solutions by sulphuretted hydrogen are, however, nearly insoluble in dilute acids. This is especially the case with nickel and cobalt, which may be separated from manganese, and even from iron by taking advantage of this peculiarity. The sulphides thrown down by sulphide of ammonium are thoroughly washed and digested in very dilute hydrochloric acid, which dissolves all the manganese, with traces of nickel and cobalt. The precipitate caused by adding sulphide of ammonium to the acid solution is slightly blackish, but after another digestion in acid of equal strength, small black flakes remain undissolved, and the addition of the alkaline sulphide now precipitates the pure, flesh-coloured sulphide of manganese. When iron has to be separated, the method is equally advantageous, although we have necessarily no guide, as far as the colour of the precipitate is concerned, to judge of the correctness of the results.

When sulphide of cobalt is digested in a strong solution of tartaric acid in the cold, little, if any, is dissolved; but, after prolonged boiling, the liquid, on evaporation, has a very faint-pink colour, and becomes brown upon the addition of sulphide of ammonium. Citric acid acts in the same manner, but perhaps with rather more energy, the colour of the solution having a brighter hue, and the shade still darker after the addition of a soluble sulphide. When carbonate of cobalt, however, is dissolved in excess of tartaric acid, and sulphuretted hydrogen passed through the solution, no precipitate takes place, even if the current of gas be continued for many hours. The same phenomenon occurs with citric acid, the liquid remaining perfectly clear and bright. If a small quantity of a solution of zinc be added, an immediate precipitate takes place, of a pure white colour, showing that the sulphide of this metal is insoluble in tartaric and citric acids. The separation of zinc from cobalt and nickel cannot be accurately effected by this means, however, as far as I have been enabled to judge from my own experiments. The carbonates of zinc and cobalt were dissolved in excess of tartaric acid in the cold, and sulphuretted hydrogen passed through the solution. The sulphide of zinc, which appeared perfectly white, was filtered off, but the filtrate, after standing a few minutes, became cloudy and deposited more sulphide. In order to prevent this, gentle heat was employed, and an hour or two allowed to elapse before filtration. On filtering, the sulphide of zinc was no longer white, but of a pale blue, evidently containing cobalt. In another experiment, the flask with the metals was placed upon the sand-bath for many hours after the passage of the gas, and the sulphide of zinc changed from white to blue, and from blue to green. The precipitate was separated by filtration, and had then very much the appearance of carbonate of copper, having as nearly as possible the same tint. That the cobalt was not in the state of sulphide, unless modified to an extraordinary extent, for which it is difficult to account, was evident for two reasons. In the first place, minute traces of sulphide of cobalt impart a blackish or dull-greyish tinge to white precipitates, such as sulphide of zinc; and in the second place, the compound was perfectly soluble in very dilute hydrochloric acid. After the disengagement of the sulphuretted hydrogen, sulphide of ammonium was added, which threw down a brownish compound, consisting of the sulphides of zinc and cobalt. The green colour which sulphide of zinc assumes when precipitated by sulphuretted hydrogen from its solution in tartaric acid, is certainly somewhat curious, especially when it is remembered that a salt of cobalt treated with

oxide of zinc gives a green colour exactly similar to that just described. In pursuing these experiments further, it was observed that when oxide of zinc is digested in a neutral solution of nitrate of cobalt, little or no reaction is exercised, but on adding the same oxide to cobalt dissolved in a considerable excess of tartaric acid, nearly the whole of that metal is precipitated as a beautiful peach-coloured powder, which contains a large amount of its precipitant.

The separation of nickel and zinc is equally unsatisfactory, the same phenomena occurring as in the case of cobalt. Iron, also, is similarly affected. No precipitate whatever is produced in a solution of protoxide of iron in tartaric acid, by the passage of sulphuretted hydrogen but when zinc is present the sulphide of that metal, at first white, becomes slightly coloured after digestion in the supernatant liquid, and when placed upon a filter and washed, assumes a pinkish hue, the iron having passed to the state of peroxide by exposure to the atmosphere, and imparted the colour to the zinc. The quantity of iron is certainly very small, but that it exists is evident by the blue tint imparted on addition of ferrocyanide of potassium to the solution of the compound in weak hydrochloric acid. From observing that these metals cannot be entirely separated from each other by this method, I did not examine the filtrates containing the greater part of the cobalt, nickel, or iron, for zinc. As a very small quantity of this metal in tartaric acid affords an immediate precipitate upon the addition of sulphuretted hydrogen, it is probably entirely thrown down by that re-agent carrying with it small traces of the other metals of the group, with the exception of manganese, when they happen to be present, although in what state these latter are precipitated experiment alone can decide.

Estimation of Carbon by Combustion, when Sulphur is Present, by M. L. CARIUS.

In the elementary analysis of sulphuretted organic combinations we know that the estimation of carbon is often erroneous on account of the disengagement of sulphurous acid. The best means of avoiding this source of error is to burn the sulphuretted substances with chromate of lead, and to place a long column of chromate of lead in the anterior part of the tube, which is heated to a dull red; the sulphurous acid which disengages itself in the posterior portion of the tube, where the chromate is in the fused state, is entirely absorbed in the anterior part, where it is fixed in the state of sulphate.—*Annales de Chimie et de Physique*, 3rd Series, vol. lx. p. 504.

On the Reduction of Potassium Compounds by Sodium, by J. A. WANKLYN, Demonstrator of Chemistry in the University of Edinburgh.

THE reduction of caustic potash by means of sodium, in presence of heated caoutchouc, mentioned by Greville Williams in the *CHEMICAL NEWS*, No. 58, is very remarkable phenomenon, which may justly lead us to ask why we rank potassium as more electro-positive than sodium.

Some time ago a similar fact came under my own observation. I fused some dry acetate of potash in a glass flask, and dropped into the fused salt pieces of sodium. Violent effervescence occurred, the evolved gas containing free hydrogen. The reaction continued on

removing the lamp from beneath the flask. Only just towards the last was there any carbonisation. Long after the materials had grown cold, an examination of the product showed that it contained a quantity of the alloy of potassium and sodium, which is liquid at common temperatures.

Like Greville Williams, I was led to view processes of this kind as an economical source of potassium, and went even so far as to propose the method to a manufacturer. If the alloy of potassium and sodium were wanted in large quantities, the reduction of acetate of potash by sodium would be far preferable to the ordinary plan.

*On the Elementary Analysis of Organic Compounds
by M. L. CARIUS.*

WHEN readily-oxidisable organic substances, such as tartaric acid, alcohol, and amylic alcohol, are heated in closed tubes with nitric acid density 1.4, the oxidation of these substances takes place at a temperature a little above 100° C., and they are rapidly transformed into water and carbonic acid. The complete oxidation of these matters by the aid of nitric acid, of a density of 1.12 to 1.2, requires a temperature of from 120° to 150° C. Phenyl alcohol and naphthalene are oxidised with difficulty under these conditions. Nevertheless, they may be completely oxidised if heated for six or eight hours at 150° C., and finally at 180° C.

A very exact method for estimating the sulphur, chlorine, and phosphorus contained in organic matters may be founded on these reactions. The method may be thus practically applied:—Weigh the substances to be analysed in small and slight glass bulbs, hermetically sealed. Introduce the liquids, with as little air as possible, into a glass bulb which has been blown in the centre of a glass tube, drawn to a fine point at each end, and turned at right angles. In order to introduce a solid body into a bulb, commence by forming it at the extremity of a tube; then fill it with the pulverised substance; lastly, draw out the tube and close the capillary end. The quantity submitted to analysis may vary from 0.15 to 0.40 grammes. The bulb and its contents are introduced into a glass tube closed at one end, of 10 to 12 millimètres interior diameter, and sufficiently long so that the empty part is of the same capacity as that filled with acid.

The acid employed should have a density of 1.20, its quantity varying from 20 to 60 times the weight of the substance. This should be regulated according to the richness of the substance to be analysed in combustible principles. When the substance and acid are in the tube, thicken the extremity before the blow-pipe, then draw it out, so as to form a capillary tube with thick sides. Then boil the acid, and when the vapours have completely dispelled the air, close the capillary tube. When cold, shake the liquid briskly, so as to break the ends of the bulb. Heat the tube, thus arranged, in an air-bath. For this purpose employ the following apparatus:—A rectangular sheet-iron case, length 0.33, height 0.1 millimètres, one of the long inner sides of which is furnished with a sliding-door. Pierce the front vertical side with four round openings, which can be closed with buttons turning on an axis. Insert in each opening a cast-iron tube, from 20 to 22 millimètres in diameter, closed at one end, and placed obliquely in the case. Small openings made in the sides of the case, between the tubes, admit of the introduction of ther-

mometers. Place this case on the large ring of a retort-stand, and fix it with another ring which grasps the top of it. The apparatus thus disposed, slide the glass tube into one of the cast-iron tubes dipping into the air-bath, and heated with a gas-lamp from 120° to 140° C., the openings of the tubes being turned towards a corner of the laboratory. Under this arrangement, any explosions, should they occur, are perfectly harmless, the pulverised glass being projected against the wall. The operation finished, it is necessary to open the tube, which requires some precaution, on account of the accumulation of gases resulting from the combustion of the organic matters. After having carefully expelled the small quantity of liquid contained in the tapering point of the tube, make it red-hot, when it puffs up, and the gases quietly blow out.

Estimation of Sulphur.—This element is contained under the form of sulphuric acid in the acid liquid which the preceding experiment yielded. The sulphuric acid can be directly precipitated by chloride of barium. The precipitate will contain traces of nitrate of baryta; it must, therefore, be treated with hydrochloric acid and washed in water after calcination.

Sulphuretted organic compounds cannot well be completely oxidised by this process at the above-mentioned temperature. Some there are which, when oxidised by nitric acid, are converted into acid ethers of sulphurous acid, and which are not completely transformed into sulphuric acid at 120° or 140° C.

Ethyl- and naphthyl-sulphurous acids are among the most refractory bodies in this respect. If nitric acid of a density of 1.2 is employed, these bodies cannot be oxidised sufficiently to transform sulphur into sulphuric acid, except by maintaining the temperature, during two or three hours, at 180° to 200° C. But it is not advisable to employ this process when it requires such very high temperatures. To analyse the substances in question, dissolve them in nitric acid of a density of 1.2; neutralise the liquid by carbonate of soda; then add excess of carbonate of soda; evaporate, and melt in a silver crucible. Finish the analysis by the usual processes.

Estimation of Phosphorus and Arsenic.—Phosphorus is found, in the state of phosphoric acid, in the nitric liquid which has been exposed to a temperature of from 120° to 130°. This acid is estimated in the state of ammonio-magnesian phosphate by the ordinary process. The arsenic of organic combinations, under the same conditions, passes to the state of arsenic acid, which is estimated as ammonio-magnesian arseniate, and is weighed after being dried at 100°.

Estimation of Chlorine.—When chlorinetted compounds are oxidised by nitric acid by the above method, one part of the chlorine is converted into hydrochloric acid, but the greatest part remains free at the end of the operation. To estimate the chlorine, turn the cooled tube, so as to have the point downwards, plunge it into a vessel containing a solution of sulphite of soda,¹ and break it carefully. The liquid in the tube is thus forced into the solution in a very fine thread, and the chlorine in contact with the sulphite is immediately converted into hydrochloric acid. When the equilibrium of pressure is established, drive a certain quantity of gas out of the tube by heat; then allow the liquid to re-enter the tube; wash it; then heat it for an hour or two to drive off all the sulphurous acid. As it is extremely acid,

¹ From 8 to 10 parts of sulphite of soda to 1 part of the analysed substance are employed, and the salt is dissolved in 40 parts of water to 1 part of the nitric acid used for oxidation.

it will be convenient to almost entirely neutralise it with carbonate of soda before precipitating it by nitrate of silver.

Estimation of Bromine and Iodine.—Bromine combinations, when heated in a closed vessel with nitric acid, behave like the chlorine compounds. Only the bromine appears to be entirely liberated. This operation ended, open the end of the tube under a solution of sulphite of soda, and terminate the analysis in the manner just indicated.

The iodine of iodised combinations is deposited in an entirely solid state, and crystallises in the tube where the oxidation is effected. Open this tube while the point is red-hot, as has before been pointed out; but before doing so place it in a refrigerating mixture, in order that the disengaged gas may not carry off any considerable quantity of the iodine vapour. Then put the liquid and iodine in a vessel containing a solution of sulphite of soda, the quantity and concentration of which is to be calculated as has been already stated. The iodine dissolves rapidly, and is transformed into hydriodic acid, which can be precipitated as iodide of silver, after being evaporated in a water-bath and neutralised by carbonate of soda.

Estimation of Metals.—The process indicated is easily applicable to the determination of metals contained in organic combinations. When the metals are dissolved and the liquid contains no phosphoric acid, they can be precipitated by appropriate re-agents,—carbonate of soda, for instance. The estimation is rather more difficult when the metals separate under the form of insoluble compounds. In the latter case, the greatest care must be taken to eliminate the pieces of glass caused by the breaking of the capillary tube.—*Annales de Chimie et de Pharmacie*, 3rd Series, vol. ix. p. 497.

TECHNICAL CHEMISTRY.

On Coal Gas, by the Rev. W. R. BOWDITCH.

(Continued from page 54.)

Sulphocyanide of ammonium may be obtained in considerable quantity from an alcoholic solution of foul clay. Upon one occasion I obtained nearly an ounce, in a fair state of purity, from less than a quart bottle of foul clay; and so tenaciously does clay retain this compound, that from some clay which had been exposed to the full action of the weather in a field for two years, I obtained a considerable coloration with perchloride of iron. Sulphocyanide of ammonium may be obtained from gas which has been purified by oxide of iron, by passing the gas through spirit of wine and evaporating.

When common yellow brick-clay is used in the purification of coal-gas, the solutions from it always contain salts of iron; but they never become of a blood-red colour until a mineral acid is added. When, however, the solutions are evaporated, and the deliquescent residue is exposed to the air, most, and sometimes all of the iron is peroxidised, and yields the well-known reaction.

The nitrogen in tar may be shown from the spirit-solution off foul clay. The spirit, evaporated to dryness and allowed to stand, deposits tar and a mixture of deliquescent crystalline salts. They were allowed to deliquesce, the fluid was removed, and the residual tar well washed with water. Subsequently it was dissolved in hot spirit, precipitated by water, and well washed.

When nothing more was removed by washing, the tar was heated, and evolved sulphide of hydrogen and ammonia. Contrary to every other compound in gas with which I have experimented, this tar gave off sulphide of hydrogen before ammonia. In other instances I have found the nitrogen compound evolved at a lower temperature than the sulphur one.

Mineral matter derived from the clay is found in all solutions; but as my object in this paper is to speak only of substances in gas, I purposely omit those united with them derived from the clay. For the same reason I make no mention of the value of the foul clay as a manure.

Sulphur Compounds in Gas purified so as not to affect Basic Acetate of Lead, and their removal.

—A recent Royal Commission on lighting picture-galleries has stated the large quantity of sulphur found in some London gas, and has intimated a doubt about the possibility of its removal. Dr. Letheby concludes, from seven years' examination of gas in London, that though quite free from sulphide of hydrogen, it contains, on an average, 200 grains of sulphur in 1000 cubic feet; and Dr. Frankland, in the new edition of "Ure's Dictionary of Arts," Part iv. pp. 730, 731, writes, "It is probable that volatile organic compounds of sulphur are produced by the action of this element with carbon and hydrogen simultaneously, although we have as yet no positive evidence of their presence in illuminating gas. . . . When once generated with coal-gas, all attempts to remove these constituents have hitherto proved ineffectual, and there seems little ground for hope that any practicable process will be devised for their abstraction." I have now the honour to submit evidence of the existence of these sulphurised compounds, and also a practical process for their removal. My attention was specially drawn to this subject by a conversation with the manager of a London gas-works. He informed me that he not unfrequently filled his gas-holders with gas which would not affect acetate of lead, and that after the gas had been stored a few hours it became so foul as to blacken lead-paper the instant it was applied. He sought an explanation of this phenomenon; and as the water of his gas-holder tanks was clean, and there were no accidental sources of sulphide of hydrogen, I concluded that an organic compound containing sulphur and hydrogen had been broken up, and that the sulphide of hydrogen was thus produced. I learnt, also, by other observations, that gas which went to the gas-holders free from ammonia, sometimes became ammoniacal if kept, and joining this fact with the former one, inferred that the compound which thus broke up contained nitrogen as well as sulphur and hydrogen. Subsequently I observed that the saturated clay taken from the purifiers of gas-works contained a quantity of foul naphthalin. This led me to procure a quantity of (so-called) naphthalin which had been taken from the mains of a London gas-works, and which, therefore, must have been deposited by purified gas. Some portions of this naphthalin were white, but others were slightly darkened by the presence of carbonaceous matter, and the whole was in fine powder, aggregated together by the process of deposition. The tendency to form exceedingly small crystals seems a constant characteristic of naphthalin which has been deposited in gas pipes, for by no amount of care and trouble have I been able to obtain it in large crystals, though the solutions from which it has crystallised have been months in evaporating. With naphthalin from tar, on the contrary, I have obtained, from an ethereal solution, crystals an

eighth of an inch thick, nearly half-an-inch broad, and more than half-an-inch in length. The supposed naphthalin from gas-pipes dissolves wholly in ether and hot alcohol, and crystallises from the spirit on cooling as pure naphthalin does. The solutions are neutral to test-papers. Boiled with an alcoholic solution of potash it evolves no ammonia, and with hydrochloric acid no sulphide of hydrogen. Heated alone, it evolves first ammonia, and then sulphide of ammonium, mixed, I think, with a trace of bisulphide of carbon, and then distils. Several samples began to give off their ammonia at 388° F., and sulphide of hydrogen at 422° F., leading to the hope that here was a compound of definite composition which would admit of correct analysis and perhaps of formulation; but some more of the naphthalin, produced, like the other, from Newcastle coal, but at another gas-works, possessed such different physical properties as to convince me that very much more must be known of this substance before any reliable analyses can be published. The latter sample gave a neutral solution in spirit, like the other, but of a considerably browner colour. When heated alone it gave off ammonia with ebullition at 218° F., and then became tranquil. When the temperature reached 375° F., it began to evolve sulphide of hydrogen, which continued to increase in quantity up to 390° F., when it nearly ceased, and quite ceased at 410° F. The proportion of tar in this sample was much greater than I have seen it in any other. Subsequently I obtained some more naphthalin, which had been deposited in the pipes of another London gas-works, and this, like the former, contained both nitrogen and sulphur, which were evolved upon distillation as sulphide of hydrogen and ammonia.

Having thus obtained one sulphurised hydrocarbon, and determined the temperature at which its sulphur and nitrogen could be obtained as easily-removable compounds, I was prepared to advance towards a better purification of gas with great probability of success. Another well-confirmed observation helped to guide me. Gas freed from every trace of sulphide of hydrogen always blackens lead-paper strongly when passed through clay; and if it be subsequently passed through lime, it affects turmeric though quite free from ammonia when taken for experiment. This process may be repeated through a series of ten or twelve purifiers containing clay and lime placed alternately, the test-papers being less affected at each purifier, until at length they are not discoloured at all. This experiment has been made upon gas produced in various parts of England and Scotland from many kinds of coal, and I think the number of instances sufficient to justify the conclusion that all gas, as sold, contains the compound from which clay liberates sulphide of hydrogen. I have not yet been able to separate the compound upon which clay thus acts. I have, however, ascertained that the clay which has liberated sulphide of hydrogen from gas which did not affect test-papers when taken for experiment contains tar, which may be dissolved out by alcohol, and may be obtained alone by evaporating the solvent.

(To be continued.)

On the Preparation of Starch from Cassava,
by JOSIAS CORE.

THE growth of cassava in quantity has had my best, my most earnest consideration. If it cannot, in every sense of the word, compete with other staples, now, or which may be grown, my praise of the root must receive qualification.

First, then, cassava is alike at home in the mountains and in the lowlands; it grows readily everywhere. The least moisture in the soil is sufficient to induce vegetation at the time of planting, and, once growing, no drought destroys, it simply for the time retards the crop.

But, if general experience does not fully corroborate this positive statement,—and there are lands of so dry a nature that a long drought does kill the cassava plant, and I will not say there may not be such lands anywhere, as I can only decisively speak of what I have noted in my own experience,—still this evil, if it anywhere exists, is less in the cassava than in any other crop I know.

For, secondly, the propagation, and, therefore, also, the supply of a field partially injured by a drought, is about the easiest thing a planter has to do. To grow corn, pulse, yams, and many other things, a portion of valuable produce must be used for seed. Not so the cassava. All that is needed is, to take a cassava stick, and mark off a length containing three or four "eyes;" pass a knife round and cut just below the skin, and then use a slight breaking power with both hands. Each bit is now a future plant; the eye or two below the soil serve for future roots; the eye or eyes above put forth leaves and stalks. It must be evident, then, that a bundle of cassava sticks, which anyone can carry under his arm, will suffice for a considerable breadth of prepared ground, and the time and labour to cut up and plant is almost the least possible to be conceived for such operations, and the cost is positively nothing. Anybody, anywhere, who has cassava growing, can give readily a few cassava sticks to anybody, and everybody, and yet have enough left for himself, and to spare.

Thirdly.—Another fine feature is this:—It is not an exhausting crop. Like other productions grown for a substance which appears to derive its constituents from the atmosphere, or composed, like sugar, of carbon, hydrogen, and oxygen, you may grow and re-grow, with change, of course, or some rest, or a slight manuring, on the same lands *ad infinitum*. In fact, everybody feels that land is always fit for cassava, however unfit it may have become for other productions.

Fourthly.—This is a very accommodating crop. The roots ripen and are fit to be manufactured, or remain and grow, and again ripen, and again remain and grow. Although there is not a positive certainty as to the exact time, for the changes from rain to dry weather are uncertain, still there is a choice of a whole year or more for the grower. If the roots be not dug when they become dry and mealy, they soon acquire what is termed a "fresh spring,"—that is, the roots become somewhat watery, and enlarge, and then ripen. This goes on from the age of about ten months to more than two years. I do not know from experience beyond twenty-four months, but it is evident to me that it is not the limit, or anything like it. All the while the roots have been enlarging, so that there has been, as far as I can judge, no loss from delay. I have known a field of cassava cut down to near the roots in a most ruthless way by a lazy fellow who desired a field of corn and would not prepare fresh land. I have seen the crop of corn, and the crop of cassava again, covering by vigorous stems and leaves the ground, and, after a while, the ripe crop of cassava. Of course, this system is the *acme* of laziness, and to be wholly condemned, but, nevertheless, the fact stands forth, as a proof of the extreme hardihood and wonderfully-accommodating nature of the cassava crop.

Fifthly.—Quantity, as I have proved, or sought to prove, is, as other root crops, very large. I need not,

therefore, enlarge on this head, but pass on to manufacture.

There again the cassava says "Try me—try me: you will find me, in parts, and in whole, a crop worth your best care and attention." On this head I observe,—

First.—Each root has to be washed, and the cuticle must be taken off. The plain surface of the root makes the washing easy; the soft nature of the substance renders the skinning process as easy as easy can be. The comparison in favour of cassava, as against arrowroot, is in both cases fully as three to one in favour of cassava.

Secondly.—The grating is also in favour of cassava, as compared to arrowroot, in almost an equal rate. But,

Thirdly.—I would not grate at all; but quote from pages of a known scientific work, and I suppose the site of operations to be the old works of a thrown up sugar estate, and unfortunately many such a spot can too readily be found. The manufacture is arrowroot, but of course the same works would equally do for cassava, and I may just observe that the manufacture is in the Island of St. Vincent, and I have no doubt its completeness is the cause of the superior price obtained for the St. Vincent arrowroot, which is just threefold the price of Jamaica arrowroot in the English market:—

"The skinned roots are thrown into a large cistern, with a perforated bottom, and then exposed to the action of a copious cascade of water. The cleansed roots are next put into the hopper of the mill, and are subjected to the powerful pressure of two pairs of polished rollers of hard brass, the lower pair of rollers being set much closer than the upper. The starchy matter is thus ground into a pulp which falls into the receiver placed beneath, and is thence transferred to large and fixed copper cylinders, tinned inside, and perforated at the bottom with numerous minute orifices like a kitchen drainer. Within these cylinders, wooden paddles are made to revolve with great velocity, by the power of a waterwheel, at the same time that a stream of pure water is admitted from above. The paddle arms beat out the fecula from the fibres and parenchyma of the pulp and discharge it in the form of a milk through the perforated bottom of the cylinder. This starchy matter runs along pipes, and then through strainers into large reservoirs, where, after the fecula has subsided, the supernatant water is drawn off. The drying-house on the Hopewell estate is constructed like the hot-house of an English garden, but instead of plants it contains four dozen of drying-pans made of copper, seven and a-half feet by four and a-half, and tinned inside. Each pan is supported on a carriage, and they run on rails; immediately after sunrise these carriages, with their pans covered with white gauze to exclude dust and insects, are run out into the open air, but if rain be apprehended they are run back under the glazed roof. In about four days the fecula is thoroughly dry and ready to be packed."

What is done in St. Vincent may be done in Jamaica. The trash-houses of the past could easily be converted into drying-houses for the future on any old sugar estate having a supply of water. The water which might easily be made to turn a cassava mill with two pairs of rollers, the boiling, curing, and still houses might all be transformed and brought into use for cassava; and with the ingenious contrivance of our Island architect, Mr. Leahy, of having blades of glass, the drying-house might be thoroughly ventilated. I would not only have a glass roof, but for the most part glass walls, with thorough ventilation, and thus lessen the time to the minimum for thorough drying.

The cost, then, of transformation would be very moderate indeed. The metal of the sugar boilers and still would pay, or nearly pay, for the vessels required for the cassava.

There is no duty on glass manufacture, and large slabs of thick glass cost little in England. The cost of manufacture of starch and blocking the fibre, would certainly be less than the cost of sugar manufacture.

To anyone, then, who will turn his attention to cassava he would certainly have a prospect before him of making an old sugar estate a handsomely paying concern as a cassava plantation.—*Jamaica Guardian*.

On the Alloys of Copper and Zinc,

by FRANK H. STORER.

(Continued from page 53.)

Pieces of the alloy, weighing from 4 to 12 grammes, accordingly as they contained a greater or less amount of copper, which had been carefully cleaned by filing and brushing, were placed in covered porcelain dishes, and dissolved in strong nitric acid on the sand-bath, the usual precautions to avoid loss from spattering being observed. To the solution thus obtained, sulphuric acid—which had been freed from its impurity of lead by dilution with water and subsequent partial concentration—was added, and the mixture evaporated to dryness over the water-bath; more sulphuric acid being added, and the solution again evaporated until all the nitric acid was expelled, or till no trace of blue crystals remained. I have rarely found it necessary to evaporate the solution more than twice in order to effect this.

It is of importance that the nitric acid be thoroughly removed, for in the presence even of a mere trace of this acid it is exceedingly difficult to precipitate, by means of iron, the last portions of copper from its solution.

After all the nitric acid had been expelled, the mixed sulphates were dissolved in hot water, and the sulphate of lead—from impurity in the zinc of the alloy—allowed to settle. This was then separated and washed with acidulated water, by decantation.

The solution, which had been collected in a capacious beaker, was now largely diluted with water and placed upon a sand-bath, the heat of which was regulated so that the solution could attain a temperature nearly equal to boiling, without, however, coming into actual ebullition. A sheet of the purest iron obtainable was next placed in the solution, leaning against the sides of the beaker, so that the largest possible amount of its surface should be exposed, in order that the iron might be attacked equally at all points by the acid, and that the copper should be precipitated in even sheets upon it.

The solution should be acid enough to ensure a slight evolution of hydrogen during the precipitation. Care should, however, be taken to guard, in the first instance, against the addition of too great an excess of acid, lest the iron be corroded to an unnecessary, or even ruinous extent. It is of course desirable to avoid dissolving any more iron than is necessary to insure regularity and completeness in the precipitation; for in this case a smaller portion of its impurities will be exposed, and its surface will remain smoother.

After the introduction of the iron, the beaker is to be covered with a glass plate, and left to itself until the copper is entirely precipitated, which occurs at the end of an hour or two at furthest. Having proved that no more copper is contained in the solution, by testing a

small portion of it with sulphuretted-hydrogen water, the beaker is removed from the sand-bath, and the clear acid solution decanted off as completely as possible from the copper and iron.

If this solution be diluted with a large quantity of hot water, it immediately becomes yellow, then turbid, and in a few minutes deposits a bulky, flocculent precipitate of a basic salt of peroxide of iron, although the supernatant liquid is strongly acid. Since a quantity of this solution remains adhering to the metals, it is well, in order to avoid the precipitate which would form if they were treated at once with hot water, first to pour a small quantity of cold water upon them. The piece of iron is then to be taken from the beaker, and any particles of copper which may be loosely attached to it removed by means of the wash-bottle; the iron being carefully preserved for subsequent examination. The copper having been once more rinsed with cold water, is now to be treated with boiling water, which should be decanted off into a large, clean beaker, without waiting for all the copper to be deposited, as soon as the liquor assumes a yellow colour. After two or three additions of hot water, the subsalt of iron is no longer precipitated, and the copper may be allowed to subside at its leisure. Any particles of copper which are found adhering to the sheet of iron which was used for the precipitation must now be removed by gently rubbing them with the finger, care being taken not to disturb the black coat¹ which is found adhering to the iron any more than can be avoided; they are then to be washed into a small porcelain capsule, into which is also brought the mixed deposit of subsalt of iron and particles of copper collected during the first rapid washing of the copper. The contents of the capsule are now treated with dilute chlorhydric acid, in which the subsalt is readily soluble, washed with water, and added to the original copper, the washing of which is to be continued till the last traces of acid are removed. The copper, having been transferred to a porcelain boat, was dried at 100° C., ignited in a current of hydrogen gas, and subsequently weighed.

The iron used in these assays was that known as the "best Russia sheet." In order to obtain the best possible pieces, I have followed the custom of copper assayers in allowing a sheet-iron worker to cut up a number of pounds of Russia sheet into bits of about two and a-half by three and a-half inches, this size having been found well adapted to the work at hand, smaller pieces being unequally corroded; from this supply only those pieces which presented a perfectly smooth and even surface were selected for use. Those pieces which are at all corrugated or uneven ought to be rejected, for such are acted on unequally by the acid, and copper would be lost in the cavities thus formed.

Since the sharp angles of these rectangular bits of iron are liable to be corroded very much by the acid, in which

case particles of iron might drop off and contaminate the copper, it is best to remove them by means of a rasp, and to make the corners round and smooth.

The glazed coating which occurs upon the surface of Russian sheet-iron, and which, according to Wells,² consists of silicate of iron, is readily removed by digestion in moderately dilute chlorhydric acid, in which it soon falls off in flakes. After washing with water, the bits of iron are dried and preserved for use.

In conducting the assay it is of importance that the solution should be dilute, since the precipitation of the copper in this case goes on more regularly, and is sooner completed; it should be warm, not only that the copper may be precipitated more rapidly, but in order to avoid the formation of a basic iron salt, which is very liable to contaminate the copper when it is precipitated in the cold. This basic salt frequently forms during the precipitation, even in warm solutions, if they are not sufficiently acidulated. If a solution be at the same time somewhat concentrated, and not sufficiently acid, it often happens that a portion of the copper becomes so firmly attached to the iron that it cannot be rubbed off. This accident will rarely occur, however, to any one accustomed to the assay.

It is remarkable that no one who has written upon this process has, so far as I can ascertain, appreciated the necessity of igniting the dry copper in a current of hydrogen before weighing it. That this operation is of the first importance is, however, well known to the analysts of this vicinity, who have long been accustomed to apply in practice.³ It is necessitated less, as it appears to me, by the oxide of copper formed during drying,—for the bright surfaces of the copper precipitated by this method remain untarnished when rapidly dried at temperatures even as high as 110° to 115°,—than by an impurity consisting of organic matter derived from the iron; either carbon, or perhaps some one of the offensive hydro-carbons which are generated during the action of acids upon iron, which may adhere to the spongy copper. At all events, if a portion of the precipitated copper which has been dried at 100°—or at any temperature lower than that at which oxidation occurs—be placed, as was suggested to me by Mr. Dickinson in a bulb tube with narrow outlet, such as is used in testing for arsenic, and heated in the flame of a spirit-lamp, it will be found that a quantity of water collects in the narrow part of the tube, while the copper becomes lustrous and assumes the bright red colour proper to it. At first sight it would appear as if this behaviour was produced by the reaction of a portion of the spongy copper upon the oxide of copper with which it might be soiled, in which event a small quantity of suboxide of copper would be formed, the colour of which could hardly mask, to any extent, that of the pure copper. But on examining the reaction more attentively I have found that carbonic acid as well as water can readily be detected in the atmosphere expelled from the tube in which the copper is heated; a distinct empyreumatic odour is at the same time perceptible. I have also several times observed traces of ammonia. This gas is always evolved in considerable quantity when the dirty copper is heated in an atmosphere of hydrogen; the aqueous vapour driven off in this case being strongly alkaline from its presence. Organic matter must therefore have been attached to the spongy copper, and the lustre which the latter acquired

¹ The black crust which forms on the iron beneath the layer of copper contains a small amount of carbon and some silica, but consists for the most part of what appears to be a compound of silicic acid and oxide of iron. This substance, called "slag" by Morfit and Booth in their very able "Report upon the Chemical Analysis of Cast-iron Gun-metal" (vide "Report of Experiments, on Strength, &c., of Metals for Cannon, by Officers of the Ordnance Department, U.S. Army" [Philadelphia, Baird, 1856, 4to] p. 408), is but sparingly soluble in dilute chlorhydric acid; slowly soluble in cold, more rapidly in hot concentrated chlorhydric acid; easily soluble in concentrated nitric acid, especially when hot; with separation of silica in both cases. It is slowly decomposed by strong soda lye. This "slag" is formed in a considerably larger quantity when sulphuric acid is used than when chlorhydric acid is substituted for it in the assay. As it usually adheres, however, to the iron with considerable firmness, the copper can generally be rubbed off without disturbing it materially.

² "Proceedings of Boston Society of Natural History," iv. p. 296.

³ In lieu of this, some assayers convert the precipitated copper into oxide, and weigh the latter.

when heated may have been occasioned by the simple expulsion of the impurity, or in part by the reduction of oxide of copper by it.

That the phenomenon does not depend upon the presence of free carbon seems to be proved by the fact that portions from the exterior of a mass of spongy copper, which had been protected from contact with the iron by a layer of copper nearly an inch in thickness, were found to exhibit reactions similar to those just described, water and empyreuma being evolved when the dry copper was heated in a closed tube. Indeed, the formation of water in any case in Mr. Dickinson's experiment with the arsenic-tube is an insuperable objection to this explanation, as well as to the idea that mixed copper and oxide of copper have reacted upon each other.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures¹ (adapted to a Juvenile Auditory), on the Chemical History of a Candle; by M. FARADAY, D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.

LECTURE V. (Jan. 5, 1861.)

Oxygen present in the Air—Nature of the Atmosphere—Its Properties—Other Products from the Candle—Carbonic Acid—Its Properties.

WE have now seen that we can produce hydrogen and oxygen from the water that we obtained from the candle. Hydrogen, you know, comes from the candle, and oxygen, you believe, comes from the air. But then you have a right to ask me, "How is it that the air and the oxygen do not equally well burn the candle?" If you remember what happened when I put a jar of oxygen over a piece of candle, you recollect there was a very different kind of combustion to that which took place in the air. Now, why is this?—it is a very important question, and one I shall endeavour to make you understand; it relates most intimately to the nature of the atmosphere, and is most important to us.

We have several tests of oxygen besides the mere burning of bodies; you have seen a candle burnt in oxygen, or in the air; you have seen phosphorus burnt in the air, or in oxygen, and you have seen iron filings burnt in oxygen. But we have other tests besides these, and I am about to refer to one or two of them for the purpose of carrying your conviction and your experience further. Here you have a vessel of oxygen. I will show its presence to you: if I take a little spark and put it into that oxygen you know by the experience you gained the last time we met, what will happen,—if I put that spark into the jar it will tell you whether we have oxygen here or not. Yes! We have proved it by combustion; and now here is another test for oxygen, which is a very curious and useful one. I have here two jars full of gas, with a plate between them to prevent their mixing; I take the plate away, and the gases are creeping one into the other. "What happens," say you, "they together produce no such combustion as was seen in the case of the candle." But see how the presence of oxygen is told by its association with this other substance. What a beautiful, curious gas I have obtained in this way, showing me the presence of the oxygen. In the same way we can try this experiment by mixing common air with this test-gas. Here is a jar containing air—such air as the candle would burn in, and here is a jar or bottle containing the test-gas. I let them come together over water, and you see the result: the contents of the test-bottle are flowing into the jar of air, and you see I obtain exactly the same kind of action as before, and that shows me that there is oxygen in the air,—the very same substance that has been already obtained by us from the water produced by the candle. But then, beyond that, how

is it that air does not burn the candle as well as oxygen will? We will come to that now. I have here two jars: they are filled to the same height with gas, and the appearance to the eye is alike in both, and I really do not know at present which of these jars contains oxygen and which contains air, although I know they have previously been filled with these gases. But here is our test-gas, and I am going to work with the two jars, in order to examine whether there is any difference between them in the quality of reddening this gas. I am now going to turn this test-gas out into one of the jars, and observe what happens:—There is reddening you see; there is then oxygen present. We will now test the other jar, but you see this is not so bright, not so red, not so distinct, as the first; and, further, this curious thing happens, if I take these two gases and shake them together well with water, we shall absorb the red gas; and then if I put in more of this test-gas and shake again we shall absorb more, and I can go on as long as there be any oxygen present to produce that effect. If I let in air it will not matter, but the moment I introduce water, the red gas disappears, and I may go on in this way, putting in more and more of the test-gas, until I come to something left behind which will not redden any longer by the use of that particular body that rendered the air and the oxygen red. Why is that? You see in a moment it is because there is, besides oxygen, something else present which is left behind. I will let a little more air into the jar, and if it turns red you will know that some of that reddening gas is still present, and that, consequently, it was not for the want of this producing body that that air was left behind.

Now, you will begin to understand what I have got to say. You saw that when I burnt phosphorus in a jar, as the smoke produced by the phosphorus and the oxygen of the air condensed, it left a good deal of gas unburnt, just as this red gas left something untouched,—there was, in fact, this gas left behind which the phosphorus cannot touch, which the reddening gas cannot touch, and this is something which is not oxygen, and yet is part of the atmosphere.

So that is one way of opening out air into the two things of which it is composed,—oxygen, which burns our candles, our phosphorus, or anything else, and this other substance which will not burn them. This other part of the air is by far the largest part. Now, this substance is a very curious thing when we come to examine it; it is remarkably curious, and yet you say, perhaps, that it is very uninteresting. It is uninteresting in some respects because of this,—that it shows no bright appearance of combustion. If I test it with a taper as I do oxygen and hydrogen, it does not burn like hydrogen, nor does it make the taper burn like oxygen. Try it in any way I will, it does neither the one thing or the other; it will not take fire: it will not let the taper burn; it puts out the combustion of anything. There is nothing that will burn in it in common circumstances. It does not smell: it is not sour; it does not dissolve in water; it is neither an acid or alkali; it is as indifferent to all our organs as it is possible for a thing to be. And you might say, "It is nothing; it is not worth chemical attention; what does it do in the air?" Ah! then come our beautiful and fine results shown us by an observant philosophy. Suppose, in place of having nitrogen, or nitrogen and oxygen, we had pure oxygen as our atmosphere. What would become of us? You know very well that a piece of iron lit in a jar of oxygen goes on burning to the end. When you see a fire in an iron grate, imagine where the grate would go to if the whole of the atmosphere were oxygen. The grate would burn up more powerfully than the coals; for the iron of the grate itself is even more combustible than the coals which we burn in it. A fire put into the middle of a locomotive would be a fire in a magazine of fuel, if the atmosphere were oxygen. The nitrogen lowers it down and makes it moderate and useful for us, and then with all that it takes away with it the fumes that you have seen produced from the candle, disperses them throughout the whole of the atmosphere, and carries them away to places where they are wanted to perform a great and glorious purpose of good to man, for the sustenance of vegetation; and thus does a most wonderful work, although you say, on examining it, "why it is a perfectly indifferent thing." This nitrogen in its ordinary state, is an inactive element; no action short of the most intense electric force, and then in the most infinitely small degree, can cause the nitrogen to combine directly with the other element of the atmosphere, or with other things round about it; it is a perfectly indifferent, and therefore to say, a safe substance.

¹ Reported verbatim by special permission.

But before I take you to that result, I must tell you about the atmosphere itself; I have written on this diagram, the composition of one hundred parts of atmospheric air:—

	Bulk.	Weight.
Oxygen.....	20	22.3
Nitrogen.....	80	77.7
	100	100.0

it is a true analysis of the atmosphere, so far as regards the quantity of oxygen and the quantity of nitrogen present. By our analysis, we find that 5 pints of the atmosphere contains only 1 pint of oxygen, and 4 pints or 4 parts of nitrogen by bulk. That is our analysis of the atmosphere. It requires all that quantity of nitrogen to reduce the oxygen down, so as to be able to supply the candle properly with fuel, so as to supply us with an atmosphere which our lungs can healthily and safely breathe; for it is just as important to make the oxygen right for us to breathe, as it is to make the atmosphere right for the burning of the fire and the candle.

But now for this atmosphere. First of all let me tell you the weight of these gases. A pint of nitrogen weighs 10 grains and $\frac{1}{10}$ ths, or a cubic foot weighs $1\frac{1}{2}$ oz. That is the weight of the nitrogen. The oxygen is heavier; a pint of it weighs $11\frac{1}{2}$ grs., and a cubic foot weighs $1\frac{1}{2}$ oz. A pint of air weighs about $10\frac{1}{2}$ grs., and a cubic foot $1\frac{1}{2}$ oz.

You have asked me several times, and I am very glad you have, "How do you weigh gases?" I will show you; it is very simple, and easily done. Here is a balance, and here is a copper bottle made as light as we can consistent with due strength, turned very nicely in the lathe, and made perfectly air-tight, with a stop-cock, which we can open and shut, which at present is open, and, therefore, allows the bottle to be full of air. I have here a nicely-adjusted balance in which I think the bottle, in its present condition, will be balanced by the weight on the other side. And here is a



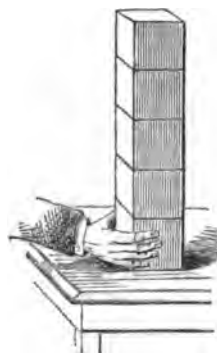
pump by which we can force the air into this bottle, and with it we will force in a certain number of volumes of air as measured by the pump [Twenty measures were pumped in]. We will shut that in and put it in the balance. See how it sinks; it is much heavier than it was. By what? By the air that we have forced into it by the pump. There is not a greater bulk of air, but there is the same bulk of heavier air, because we have forced in air upon it. And that you may have a fair notion in your mind as to how much this air is, here is a jar full of water. We will open that copper vessel into this jar, and let the air return to its former state. All I have to do now is to screw them tightly together, and to turn the taps, when there, you see, is the bulk of the twenty pumps of air which I forced into the bottle; and to make sure that we have been quite correct in what we have been doing, we will take the bottle again to the balance, and if it is now counterpoised by the original weight, we shall be quite sure we have made our experiment correctly. It is balanced; so, you see, we can find out the weight of

the extra volumes of air forced in in that way, and by that means we are able to ascertain that a cubic foot of air weighs $1\frac{1}{2}$ oz. But that small experiment will by no means convey to your mind the whole literal truth of this matter. It is wonderful how it accumulates when you come to larger volumes. This bulk of air [a cubic foot] weighs $1\frac{1}{2}$ oz. What do you think of the contents of that box above there which I have had made for the purpose? The air which is within that box weighs one pound—a full pound, and I have



calculated the weight of the air in this room,—you would hardly imagine it, but it is above a ton. So rapidly do the weights rise up, and so important is the presence of the atmosphere, and of the oxygen, and the nitrogen in it, and the use it performs in conveying things to and fro from place to place, and carrying bad vapours to places where they will do good instead of harm.

Having given you that little illustration with respect to the weight of the air, let me show you certain consequences of it. You have a right to them because you would not understand so much without it. Do you remember this kind of experiment: have you ever seen it? Suppose I take a pump somewhat similar to the one I had a little while ago to force air into the bottle, and suppose I place it in such a manner that by certain arrangements I can apply my hand to it. My hand moves about in the air so easily that it seems to feel nothing, and I can hardly get velocity enough by any motion of my own in the atmosphere to make sure that there is much resistance to it. But, when I put my hand here [on the air-pump receiver, which was afterwards exhausted] you see what happens. Why is my hand fastened to this place, and why am I able to pull this pump about? And see! how is it that I can hardly get my hand away? Why is this? It is the weight of the air—the weight of the air that is above. I have another experiment here which I think will explain to you more



about it. When the air is pumped from underneath the bladder which is stretched over this glass, you will see

the effect in another shape ; the top is quite flat at present, but I will make a very little motion with the pump, and now look at it,—see how it has gone down, see how it is bent in ; you will see the bladder go in more and more, until, at last, I expect it will be driven in and broken by the force of the atmosphere pressing upon it [the bladder, at last, broke with a loud report]. Now, that was done entirely by the weight of the air pressing on it, and you can easily understand how that is. The particles that are piled up in the atmosphere stand upon each other, as these five cubes do ; you can easily conceive that these five cubes are resting upon the bottom one, and if I take that away the others will all sink down. So it is with the atmosphere ; the air that is above is sustained by the air that is beneath, and when the air is pumped away from beneath them, the change occurs which you saw when I placed my hand on the air-pump, and which you saw in the case of the bladder, and which you shall see better here. I have tied over this jar a piece of sheet india-rubber, and I am now about to take away the air from the inside of the jar, and if you will watch the india-rubber—which acts as a partition between the air below and the air above, you will see when I pump how the pressure shows itself. See where it is going to, I can actually put my hand into the jar ; and yet this result is only caused by the great and powerful action of the air above. How beautifully it shows this curious circumstance.

Here is something that you can have a pull at when I have finished to-day. It is a little apparatus of two hollow brass hemispheres, closely fitted together, and having connected with it a pipe and a cock, through which we can exhaust the air from the inside ; and although the two halves are so easily taken apart while the air is left within, yet, you will see when we exhaust it by-and-by, no power of any two of you will be able to pull them apart. Every square inch of surface that is contained in the area of that vessel sustains fifteen pounds by weight, or nearly so, when the air is taken out ; and you may try your strength presently in seeing whether you can overcome that pressure of the atmosphere.

Here is another very pretty thing,—the boys' sucker, only refined by the philosopher. We young ones have a perfect right to take toys, and make them into philosophy, inasmuch as now-a-days we are turning philosophy into toys. Here is a sucker, only it is made of india-rubber ; if I clap it upon the table, you see at once it holds. Why does it hold ? I can slip it about, and yet if I try to pull it up, it seems as if it would pull the table with it. I can easily make it slip about from place to place ; but only when I bring it to the edge of the table can I get it off. It is only kept down by the pressure of the atmosphere above. Here is a couple of them ; if you take these two and press them together, you will see how strong they stick. And, indeed, we may use them as they are proposed to be used, to stick against windows or against walls, where they will adhere for an evening, and serve to hang anything on that you want. I think, however, that you boys ought to have experiments that you can make at home ; and so here is a very pretty experiment in illustration of the pressure of the atmosphere. Here is a tumbler of water ; suppose I were to propose to you to turn that tumbler upside down so that the water should not fall out, and yet not keep it in by my hand, but merely by using the pressure of the atmosphere ; could you do that ? Take a wine-glass, either quite full or half full of water, and put a flat card on the top ; turn it upside-down, and then see what becomes of the card and of the water. The air cannot get in because the water by its capillary attraction round the edge keeps it out.

I think this will give you a strong notion of what you may call the materiality of the air, when I tell you that that box holds a pound of it, and this room more than a ton, and you will begin to think that air is something very serious. I will make another experiment to convince you of this positive resistance. There is that beautiful experiment of the pop-gun, made so well and so easily you know out of a quill, or a tube, or anything of that kind ; where we take a slice of potato for instance, or an apple, and take the tube and cut out a pellet, as I have now done, and push it to one end. I have made that end tight ; and now I take another piece and put it in : it will confine the air that is within the tube perfectly and completely for our purpose ; and now I shall find it absolutely impossible by any force of mine to drive that little pellet close up to the other. It cannot be done ; I may press the air to a certain amount, but if I go on pressing, long before it comes to the second the confined air will drive the front one out with a force something like that of

gunpowder ; for gunpowder is in part dependent upon the same action that you see in this case.

Here is an experiment which I saw the other day and was very pleased with, as I thought it would serve our purpose here. (I ought to have held my tongue for four or five minutes before I began this experiment, because I depend upon my lungs for the success of it.) By the proper application of air I expect I can drive this egg out of one cup into the other by the force of my breath, but if I fail it is in a good cause, and I do not promise success, because I have been talking more than I ought to do to make the experiment succeed.

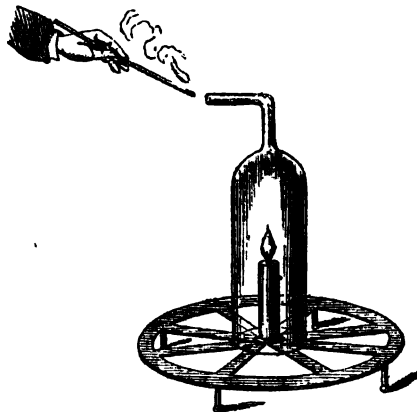
[The Lecturer here tried the experiment, and succeeded in blowing the egg from one egg-cup to the other.]

You see that the air which I blow goes downwards between the egg and the cup, and makes a blast under the egg, and is thus able to lift a heavy thing—for a full egg is a very heavy thing for air to lift. If you want to make the experiment you had better boil the egg quite hard first, and then you may very safely try to blow it from one cup to the others with a little care.

I think I have now kept you long enough upon this property of the weight of the air, but there is another thing I should like to mention. You saw the way in which, in this pop-gun, I was able to drive the second piece of potato half or two-thirds of an inch before the first piece started, by virtue of the elasticity of the air ; just as I pressed into the copper bottle the particles of air by means of the pump. Now this depends upon a wonderful property in the air, namely, its elasticity, and I should like to give you a good illustration of this. It is this : if I take anything that confines the air properly, as this membrane, it is able to contract and expand so as to give us a measure of the elasticity of the air, and to confine in it a certain given portion of air ; and then, if we take the atmosphere off from the outside of it, just as in these cases we put the pressure on—if we take the pressure off you will see how it will then go on expanding and expanding, larger and larger, until it will fill the whole of this bell-jar, showing you that wonderful property of the air, its elasticity, its compressibility, and expansibility, to an exceedingly large extent ; and this is very essential for the purposes and services it performs in the economy of creation.

We will now turn to another most important part of our subject, remembering that we have examined the candle in its burning, and have found that it gives rise to various products. We have the products, you know, of soot, of water, and of something else, which you have not yet examined. We have collected the water, but have allowed the other things to go into the air. Let us now examine some of these other products.

Here is an experiment which I think will help you in part in this way. We will put our candle there, and place over it



a chimney, thus. I think my candle will go on burning, because the air-passage is open at the bottom and the top. In the first place, you see the moisture coming—that you know about. It is water produced from the candle by the action of the air upon its hydrogen. But besides that, something is going out at the top : it is not moisture,—it is not water,—it is not condensable ; and yet, after all, it has very singular properties. You will find that the air coming out of the top of our chimney is nearly sufficient to blow the

light out I am holding to it, and if I put the light fairly opposed to the current, it will blow it quite out. You will say, that is as it should be, and I am supposing that you think it ought to do so, because the nitrogen does not support combustion, and ought to put the candle out, since the candle will not burn in nitrogen. But is there nothing else there than nitrogen? I must now anticipate,—that is to say, I must use my own knowledge, to supply you with the means that we adopt for the purpose of ascertaining these things, and examining such gases as these. I will take an empty bottle,—here is one,—and if I hold it over this chimney, I shall get the combustion of the candle below sending its results into the bottle above; and we shall soon find that this bottle contains, not merely an air that is bad as regards the combustion of a taper put into it, but having other properties.

Let me take a little quick lime and pour some common water on to it,—the commonest water will do. I will stir it a moment, then pour it upon a piece of filtering paper in a funnel, and we shall very quickly have a clear water proceeding to the bottle below, as I have here. I have plenty of this water in another bottle, but nevertheless I should like to use the lime water that was prepared before you so that you may see what its uses are. If I take some of this beautiful clear lime-water and put that into this jar which has collected the air from the candle, you will see a change coming about. Do you see that that water has got quite milky? Observe, that will not happen with air merely. Here is a bottle filled with air, and if I put a little lime-water into it neither the oxygen nor the nitrogen, nor anything else that is in that quantity of air will make any change in the lime-water, it remains clear and perfect, and no shaking of that quantity of lime-water with that quantity of air in its common state will cause any change; but if I take this bottle with the lime-water and hold it so as to get the general products of the candle in contact with it, in a very short time you see we shall have it milky—there is the chalk, consisting of the lime which we used in making the lime-water, combined with something that came up from the candle,—that other product which we are in search of and which I want to tell you about to-day: This is a substance made visible to us by its action, which is not the action of the lime-water either upon the oxygen or upon the nitrogen, nor upon the water itself, but it is something new to us from the candle. And then we find this white powder produced by the lime-water and the vapour from the candle appears to us very much like whitening or chalk, and when examined it does prove to be exactly the same substance as whitening or chalk. So we are led, or have been led, to observe upon the various circumstances of this experiment, and to trace this production of chalk to its various causes to give us the true knowledge of the nature of this combustion of the candle,—to find that this substance issuing from the candle is exactly the same as that substance which would issue from a retort if I were to put some chalk into it and make it red hot with a little moisture: you would then find that exactly the same substance would issue from it as from the candle.

But we have a better means of getting this substance, and in greater quantity, so as to ascertain what its general characters are. We find this substance in very great abundance in a multitude of cases where you would least expect it. All lime-stones contain a great deal of this gas which issues from the candle, and which we call *carbonic acid*. All chalks, all shells, all corals, contain a great quantity of this curious air. We find it fixed in these stones, for which reason Dr. Black called it "fixed air,"—finding in these fixed things like marble and chalk,—he called it fixed air because it lost its quality of air, and assumed the condition of a solid body. We can easily get this air from marble. Here is a jar containing a little muriatic acid, and here is a taper which, if I put it to that jar, will show only the presence of common air. There is, you see, pure air down to the bottom; the jar is full of it. Here is a substance—marble, a very beautiful and superior marble, and if I put these pieces of marble into the jar, a great boiling apparently goes on. That, however, is not steam; it is a gas that is rising up, and if I now search the jar by a candle I shall have exactly the same effect produced upon the taper as I had from the air which issued from the end of the chimney over the burning candle. It is exactly the same action, and caused by the very same substance that issued from the candle; and in this way we can get carbonic acid in great abundance,—we have already nearly filled the jar. We also find that this gas is not merely contained in marble. Here is a vessel in which I

have put some common whitening-chalk which has been washed in water and deprived of its coarser particles, and so supplied to the plasterer as whitening. Here is a large jar containing this whitening and water, and I have here some strong sulphuric acid, which is the acid you might have to use if you were to make these experiments (only in using this acid with limestone, the body that is produced is an insoluble substance, whereas, the muriatic acid produces a soluble substance that does not so much thicken the water). And you will seek out a reason why I take this kind of apparatus for the purpose of showing this experiment. I do it because you may repeat in a small way what I am about to do in a large one. You will have here just the same kind of action, and I am evolving in this large jar carbonic acid exactly the same in its nature and properties as the gas which we obtained from the combustion of the candle in the atmosphere. And no matter how different the two methods by which we prepare this carbonic acid, you will see, when we get to the end of our subject, that it is all exactly the same, whether prepared in the one way or in the other.

We will now proceed to the next of our experiments with respect to this gas. What is its nature? Here is one of the vessels full, and we will try it as we have done so many other gases—by combustion. You see it is not combustible, nor does it support combustion. Neither, as we know, does it dissolve much in water, because we collect it over water very easily. Then you know that it has an effect and becomes white in contact with lime-water, and when it does become white in that way, it becomes one of the constituents to make carbonate of lime or lime-stone.

Now, the next thing is to show you that it does dissolve a little in water, and therefore that it is unlike oxygen and hydrogen in that respect. I have here an apparatus by which we can produce this solution. In the lower part of this apparatus is marble and acid, and in the upper part cold water. The valves are so arranged that the gas can get from one to the other; I will set it in action now, and you can see the gas bubbling up through the water, as it has been doing all night long, and by this time we shall find that we have this substance dissolved in the water. If I take a glass and draw off some of the water, I find that it tastes a little acid to the mouth; it is impregnated with carbonic acid; and if I now apply a little lime-water to it, that will give us a test of its presence. This water will make the lime-water turbid and white, which is the carbonic acid test.

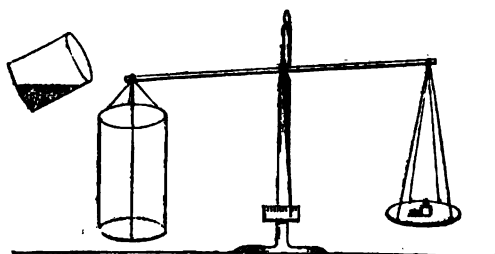
Then it is a very weighty gas; it is heavier than the atmosphere. I have put their respective weights at the lower part of this table, along with, for comparison, the weights of the other gases we have been examining:—

	Pint.	Cubic Foot.
Hydrogen	$\frac{1}{2}$ grs.	$\frac{1}{16}$ oz.
Oxygen	11 $\frac{1}{2}$	1 $\frac{1}{2}$
Nitrogen	10 $\frac{1}{2}$	1 $\frac{1}{2}$
Air	10 $\frac{1}{2}$	1 $\frac{1}{2}$
Carbonic acid	16 $\frac{1}{2}$	1 $\frac{1}{2}$

A pint of it weighs 16 $\frac{1}{2}$ grs., and a cubic foot weighs 1 $\frac{1}{2}$ oz., almost two ounces. You can see by many experiments that this is a heavy gas. Suppose I take a glass containing nothing else but air, and this vessel containing the carbonic acid; and suppose I pour a little of this gas into that glass, I wonder whether any has gone in or not; I cannot tell by the appearance, but I can in this was [introduces the taper]. Yes, there it is, you see; and if I were to examine it by lime-water, I should find it in the same way. I will take this little bucket, and put it down into the well of carbonic acid,—indeed we too often have real wells of carbonic acid,—and now, if there is any carbonic acid, I must have got to it by this time, and it will be in this bucket, which we will examine with a taper. There it is, you see; it is full of carbonic acid.

I have another experiment here by which I will show you its weight. I have here a jar suspended at one end of a balance—it is now equipoised, but when I pour this carbonic acid into the jar on the one side which now contains air, you will see it sink down at once because of the carbonic acid that I pour into it. And now I examine this jar with the lighted taper. I shall find that the carbonic acid has fallen into it, and it no longer has any power of supporting the combustion. If I blow a soap bubble, which of course will be filled with air, and let it fall into this jar of carbonic acid, it will float. But I shall first of all take one of these little balloons filled with air. I am not exactly sure where the carbonic acid is, we will just

try the depth, and see whereabouts is its level. There you see we have this bladder floating on the carbonic acid, and if



I evolve some more of the carbonic acid, you will see the bladder lifted up higher. There it goes, the jar is nearly full, and now I will see whether I can blow a soap bubble on that and float it in the same way. [The Lecturer here blew a soap bubble and allowed it to fall into the jar of carbonic acid, when it floated in it midway.] It is floating as the balloon floated by virtue of the greater weight of the carbonic acid than of the air. And now, having so far given you the history of the carbonic acid, as to its sources in the candle, as to its physical properties and weight, when we next meet I shall show you of what it is composed, and where it gets its elements from.

[The Sixth and Last Lecture will appear in our next Number.]

THE ROYAL INSTITUTE OF BRITISH ARCHITECTS.

A General Meeting of this Society was held at the rooms of the Institute, on Monday evening last, Jan. 23, 1861.

Mr. DIGBY WYATT occupied the chair.

Some preliminary business having been disposed of,

Mr. TITE, M.P., opened a discussion on the various methods which had been proposed for the preservation of the stone used in the construction of the New Houses of Parliament. The notice which had been put upon the paper was, that he would give some account of the processes used by the Ancients for protecting stone or other materials. If he had been asked this time last year whether the Ancients possessed any such processes, or adopted any such practice, he should have contradicted it in the strongest possible terms; and it was only recently that a single incident induced him to modify that opinion. All his experience of their buildings, and certainly all his reading on the subject, would induce him to believe that they made no such attempt to preserve their building-stones, because in their climate no such attempt was necessary. They painted their buildings and statues with colour, it was true; but he believed that no colouring which they used was in the slightest degree intended for the purpose of preventing decay. Recently, however, notice had been given of two processes: one of which had been called *Zopissa*, and the other had a distinct reference to the ancient encaustic. He had found two references to the ancient *zopissa*; the one in Dioscorides, and the other in Pliny. They both gave a similar description of it as being composed of pitch scraped off old vessels, and which had been exposed to the action of water, united with wax. He had failed to discover any other reference in any of the ancient authors. He considered there was no analogy between it and the material for which the name was now claimed, and the present use of that word was only the employment of a very hard term with a very unintelligible meaning. With reference to encaustic, Vitruvius, a great authority in the matter, gave a very detailed account of its use. From that author it appeared, that encaustic was made from marble burnt into quick-lime, and applied very much the same as plaster in the present day. At the time it was proposed to build

the Houses of Parliament, the commissioners wished to obtain a stone which would not be subject to the decay to which Portland stone was liable; they at last found between Derbyshire and Yorkshire, a stone called Dolomite, which was, in point of fact, magnesian limestone, or a compound of carbonic acid, lime, and magnesia. The name Dolomite, was given by the French, in consequence of Dolomieu, an eminent mineralogist, having distinguished it from ordinary limestone. The magnesia of commerce was mainly manufactured from this Dolomite. He believed that the main cause of the failure of this stone was, that the specimens employed were not crystalline. When crystalline, the stone was almost imperishable, but when crystallisation had not taken place, decay was very rapid. He then decomposed a portion of the stone by means of sulphuric acid, thereby evolving carbonic acid. What was there done rapidly the sulphuric and sulphurous acids in the air were doing slowly. It was that action which destroyed the stone in London. As an example of the absence of these acids in the air of some countries, he referred to the glittering appearance of the roofs and spires of Swiss edifices. The angles of the spires and the guttering of the roofs of the houses were made of tin; but in London it was known that tin would not preserve its brightness for more than three or four months. It was this effect of the London atmosphere with which they had to deal, and, in point of fact, to guard against. With regard to the remedy, the most obvious means for the preservation of a surface, whether of cement or stone, was to paint it. Another remedy which had been proposed was, to bitumenize it. Specimens of bitumenized stone were to be seen at Tunbridge Wells. But the black appearance of stones so treated rendered the process unfit for the present case. The process was exceedingly valuable for some purposes. The remedies proposed might be classified into bituminous, oleaginous, and silicious, the first of which he had already shown to be inapplicable. With regard to the oleaginous, he cited an instance from his own experience. About 25 years ago, when the great streets of the city of London were being constructed under Sir Robert Smirk, a preparation was patented under the name of Howlett's Cement. The result was beautiful in colour, and the surface which it made admirably compact. This substance was ordered for use, and was composed of sand or ground stone, grey oxide of lead, and red oxide of lead; the red lead being used merely to give the composition a warm colour. These were united with linseed oil, and laid in the form of paste on the front of the building. He submitted a portion to Mr. Faraday for examination, who showed that it was nothing more than a coarse paint, and that, like any other paint, it would decompose; and demonstrated his opinion by the simple experiment of burning a piece of the cement in a candle. "That which the candle will do readily," said Mr. Faraday, "the air will do sooner or later." Another of the oleaginous class was the patent of Mr. Szerelmey. It was dated in 1857. He describes his invention as a preparation for rendering walls and other structures waterproof. There was no direction as to how the composition was to be used, but all the patentees claimed the application of their patents to stone. With regard to silicious methods, the first process was patented by Mr. W. Edward Newton; but it appeared that that specification was merely the patenting of all Kohlmann's processes. The only other process to which he would refer was Mr. Ransome's process. It referred mainly to the preparation of artificial stone, and he (Mr. Tite) believed that in that particular it had succeeded; he believed the artificial stone was a very useful and economical material. There was also a claim for applying the solution to stone. It involved the question of silication. The meeting would see that his leaning for the cure of the evil they so much deplored, lay in that direction. If they used anything that decayed rapidly, they would only be palliating and concealing the

evil. He could not understand why, in the present state of science, a thorough remedy could not be devised. The obvious means was to communicate to the face of the stone a surface of glass—a material almost indestructible. We had now found a means of liquefying glass, and all that was wanted was to find a means of applying it. The whole of the question was carefully stated in certain papers which had been printed at the instance of H.R.H. the Prince Consort. Kohlmann's essay on this subject had also been translated from the German at the request of the Prince Consort. The method recommended in that essay had been adopted very extensively in France, at Lisle, and many other places, and was deemed a perfect success. It was also used by Kaulbach for the coating of fresco pictures. With regard to its application, the inventor had stated that the concentrated solution was not perceptibly affected by the atmosphere, although it was decomposed and converted into a stiff jelly on passing carbonic acid through it. He (Mr. Tite) begged to correct that statement. When concentrated water-glass was brushed upon marble or thick paper, although at first forming a shining surface, it afterwards cracked. Water-glass, when applied to stone in England, must be especially liable to this accident, as the stone was very rarely dry. Our moist climate and constant rain saturated the stone, and left little room for the absorption of the silica. The silica must enter into combination with the lime. The great durability of the ancient mortars was owing, not to the silica, but to the silicate of lime which was formed. He had been asked whether he could not coat the silica of soda with bitumen so as to give time for the silicate of lime to unite with the lime underneath it. He believed that could not be done, because the silica of lime threw out the soda, which, by its efflorescence, would cause the surface to fall away; or, if that were not the case, the carbonic acid would not unite with the silicate of soda and the lime, and silication would not take place. He wished to show what he apprehended was the difficulty in Mr. Ransome's patent. The theory was that it was possible by introducing over the surface of the silicate a composition to produce an artificial stone. If that could be effected, if the carbonate of lime and silica could be made to unite on the surface of the stone, the secret would be solved, but that could not be because the lime was not soluble. Mr. Ransome therefore used the muriate of lime, or chloride of calcium, as it was called in the softer language of the present day. The substance was hard, and could certainly be applied to the outside of the Palace at Westminster; but the difficulty which occurred was, that when it got thoroughly dry it became powdery and liable to be washed away.

Mr. BURNELL said the stone which had been used was an amorphous Dolomite, and that was the great error that had been made. It should have been a crystalline Dolomite, and would not then have been liable to the disintegration of the surface. But the real question which concerned them, as practical and professional men, was, having a material which had already absorbed causes of decay, how were they to be counteracted? Bituminous matter and paints were liable to rapid decomposition; and he believed the only material applicable was some precipitate of the silicate of lime; but if it were attempted to apply that upon a stone saturated with moisture, it would not penetrate, and must be thrown off by the first frost.

Mr. COWPER, M.P., Chief Commissioner of Works, said Szerelemey's process had been applied to a portion of the building, at the instance of the late Sir Charles Barry, but the result had not been satisfactory. Her Majesty's Government were naturally looking to scientific men for guidance, and would be willing to adopt the best means that could be suggested.

The discussion was adjourned till Monday next, February 4th.

CHEMICAL SOCIETY.

Thursday, January 17.

ALFRED SWEE, Esq., F.R.S., V.P., in the Chair.

Dr. APFERN was elected a Fellow, and Mr. A. REYNOLDS an Associate, of the Society.

The following papers were read:—

"Analysis of the Saline Water of Purton, near Swindon, North Wilts," by Dr. NOAD, F.R.S. The spring, which has a great medicinal reputation, yields about 120 gallons of water daily. It is situated at the distance of about a mile and a-half from the Thames at Crickdale. The soil of the neighbourhood rests upon limestone. The fixed contents in an imperial gallon of the water amounted to 409.6 grains. These consisted principally of the carbonates, sulphates, and chlorides of sodium, calcium, and magnesium, with a smaller quantity of some potassium, salt, and traces of iodine, bromine, and phosphoric acid.

Dr. VOELCKER also read a paper "On the Composition of Purton Water." His specimen was obtained in October, 1859. He observed that the water varied considerably in the total and relative amounts of its constituents. His arrangement of results was different from Dr. Noad's. On account of the strongly-marked alkalinity of the water, he had expressed the fixed carbonic acid in the form, not of carbonate of lime, but of carbonate of potash.

The direct results obtained by the two analysts are as follow:—

	Grains per Imperial Gallon.	
	Noad.	Voelcker.
Carbonate of lime ..	40.608	—
Carbonate of magnesia ..	2.104	—
Lime ..	25.760	34.536
Magnesia ..	38.060	25.736
Potash ..	5.544	20.707
Soda ..	76.360	49.006
Chloride of sodium ..	—	34.297
Chlorine ..	22.480	—
Sulphuric acid ..	191.200	165.074
Carbonic acid ..	—	33.090
Silica ..	2.240	1.280
Phosphoric acid ..	0.248	0.280 } phosph. of iron and alumina.
Iodine ..	0.075	0.056
Bromine ..	traces	0.080
Apocrenic acid ..	0.896	—
Crenic acid ..	traces	—
Organic matter, &c. ..	—	8.750
	405.575	372.892

Professor BLOXAM read a paper "On the Electrolytic Test for Arsenic." In a former communication on this subject he had shown that when arsenical compounds mixed with diluted sulphuric acid were acted upon by the current produced from four or five cells, arseniuretted hydrogen gas was liberated from the negative pole; but that arsenic acid did not respond to this test, and that the presence of mercury interfered with it. The author now obviated these difficulties by adding a small quantity of sulphuretted hydrogen water to the liquid during its electrolysis. By this means a ring of tersulphide of arsenic, followed by one of metallic arsenic, is readily obtained, both from arsenic acid and from organic substances containing arsenic, which have been destroyed by chlorate of potash and hydrochloric acid. The sulphuretted hydrogen, moreover, precipitates the mercury, and thereby prevents its interference. It also precipitates any antimony that may be present, whereby the contamination of the arseniuretted hydrogen with antimoniuuretted hydrogen is avoided. The author found that, when antimony and arsenic were present in an organic substance, clear evidence of both metals could be successively obtained. The product of the action of hydrochloric acid and chlorate of potash upon the substance was intro-

duced into the decomposing cell, whereby antimoniuiretted hydrogen was evolved free from arseniuiretted hydrogen. Afterwards, on the addition of sulphuretted hydrogen, arseniuiretted hydrogen free from antimoniuiretted hydrogen was evolved. The author observed that, according to his experience, sulphuric acid, as a rule, could be obtained more pure than hydrochloric acid. Moreover, he had found various samples of caustic potash to be contaminated with arsenic. Sulphuretted hydrogen prepared from diluted pure sulphuric acid and sulphide of iron was found to contain traces of arseniuiretted hydrogen.

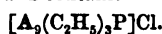
Dr. Hofmann made a short verbal communication on "*The Silver Bases of the Phosphorus, Arsenic, and Antimony Series.*" Triethylphosphine, triethylarsine, and triethylstibine, as is well known, have no action on test-papers. Treated with freshly precipitated oxide of silver, these substances are converted into powerfully alkaline liquids, containing respectively,—

Hydrate of argento-triethylphosphonium,
 $\left\{ \text{Ag}(\text{C}_2\text{H}_5)_3\text{P} \right\} \text{O},$

Hydrate of argento-triethylarsonium,
 $\left\{ \text{Ag}(\text{C}_2\text{H}_5)_3\text{As} \right\} \text{O},$ and

Hydrate of argento-triethylstibonium,
 $\left\{ \text{Ag}(\text{C}_2\text{H}_5)_3\text{Sb} \right\} \text{O}.$

The solutions of these three bases exhibit with metallic salts the deportment of potassa. Treated with alkaline chlorides these bases yield white precipitates of difficultly soluble double compounds, which, by ebullition with water, are gradually converted into the chlorides of the three compound metals. The salt of the phosphorus series is crystalline, and contains



The hydrate of argento-triethylphosphonium is decomposed by hydrochloric acid, with formation of chloride of silver and chloride of triethylphosphonium. The existence of the above compounds appears to throw some light upon the composition of the explosive substances formed by the action of ammonia on oxide of silver, the nature of which is still doubtful.

Dr. Odling suggested that the formation of compounds of this description might possibly serve to settle the question of the bi-equivalency or bi-atomicity of the metals—lead, mercury, zinc, &c. If lead, for instance, were diatomic, it should form a diamine base. How could Dr. Hofmann distinguish by its chemical properties the chloride or oxide of a diamine base from the chloride or oxide of a monamine base?

Dr. Hofmann replied, that the platino-chlorides of the diamine bases crystallised in needles, and those of the monamine bases in octahedra. If the double chloride of platinum and triethyl-zinc-ammonium crystallised in octahedra, he should consider the compound to be a monammonium, and to contain monatomic zinc = 31, thus $\text{Zn Et}_3\text{N Cl}$; but, if it crystallised in needles, he should be inclined to regard it as a diammonium, containing diatomic zinc = 63, thus $\text{Zn}'' \text{Et}_6\text{N}_2\text{Cl}_2$.

NOTICES OF BOOKS.

The Successful Application of Charcoal Air-Filters to the Ventilation and Disinfection of Sewers. A Letter to the Right Honourable the Lord Mayor, William Cubitt, M.P. By JOHN STENHOUSE, LL.D., F.R.S. Churchill, London.

In this pamphlet Dr. Stenhouse once more draws attention to the extraordinary power of charcoal in disinfecting and deodorising the gaseous products of putrefactive fermentation. The first practical application of this import-

ant property was made in 1854, when, at the author's suggestion, a charcoal air-filter was fitted up in the Justice-room of the Mansion House. The window of this room opens just above a large urinal, the smell from which was the subject of constant complaint in the room. The filter at once destroyed the nuisance, and "although six years have elapsed, the charcoal has never required to be renewed."

The deleterious nature of the gases evolved from decomposing sewage are too well proved by the numerous outbreaks of disease which have been distinctly traced to their influence. Yet, strange to say, the simple remedy, or rather preventive measure, which Dr. Stenhouse suggested six years ago, has yet received but a very limited application. It seems that 103 filters have been put down in one district in the City, and that so far the results are perfectly satisfactory, no bad smell being observed where they are placed, and no obstruction being caused to the ventilation of the sewers. Mr. Rawlinson has also applied them with a like success in two or three country towns, and expresses an opinion that the entire metropolis may be freed from sewer-gas smell by a very simple plan and at very small cost. Under these circumstances, we must thank Dr. Stenhouse for again thrusting the subject on public attention; and it will be for the Board of Works, which so readily spends hundreds of thousands of pounds on new streets and doubtful improvements, to consider whether a few thousands might not be very profitably invested in the abatement of a dangerous nuisance, and the probable salvation of many valuable lives.

NOTICES OF PATENTS.

Improvements in the Manufacture of Certain Colouring Matters by the Oxidation of Salts of Aniline and other Salts of a Similar Nature. By E. J. HUGHES, of Manchester. Communication from MM. Depouilly and Lauth, of Clichy, France.

In the Provisional Specification of this patent, hypochlorous acid and the hypo-chlorites are claimed for producing violet colours by acting with them upon salts of aniline, and nitric acid is claimed for producing red. The final specification, however, is confined to the use of nitric acid, the Patentees having, probably, become aware of the previous patents alluded to in our seventh number.

To produce a colour from aniline by the agency of nitric acid, the patentees mixes in a still two hundred parts of aniline with sixty-five parts of nitric acid specific gravity, 1.360. The mixture is heated by means of a sand-bath, or some convenient substitute, to 300° or 400° F. The distillate consists partly of aniline and partly of water, previously contained in the nitric acid. When no more water comes over, the residue is boiled for another hour; this converts the mixture into a dark red, syrupy fluid. This latter is then mixed with a little water, neutralised by carbonate of soda, and washed with water until the latter comes away colourless or slightly reddened. The product when dried consists of the red colouring matter and a brownish impurity. The two are to be separated by taking advantage of the comparative insolubility of the impurity. On boiling the impure colour, therefore, with water, the resinous matters float and may be removed by skimming and subsequent filtration through sand. The colour precipitates on cooling, and may be dissolved in alcohol or acetic acid for use.

Improvements in the Production of Colours for Dyeing or Printing. By E. CHAMBERS NICHOLSON. Provisional Protection only.

Mr. Nicholson takes nitric acid and adds to it aniline or its homologues, and heats the mixture in a still until

the colour is sufficiently developed. The coloured product is to be extracted by boiling the residue in the still with water, or extracting it with alcohol. "From these solutions the colouring matter may be prepared for use by any convenient and well-known method." In the same manner that Mr. Nicholson's provisional specification, dated January 25, 1860, for the use of arsenic acid, followed Mr. Medlock's of the week previous; so his provisional for nitric acid follows Mr. Hughes's, dated four days previously. Probably neither process will be very largely employed. One of our correspondents, who carefully tried the action of nitric acid upon aniline, with a view of obtaining colouring matter, reports very unfavourably, both as to the colour produced, and the economy of working. This remark only applies to the acid used without the intervention of an oxide.

A New Process for Manufacturing Red and Violet Colouring Matter. By Messrs. GEORGES DE LAIRE and CHARLES GIRARD, of the Imperial Mint, Paris.

As the patentees merely employ arsenic acid which was patented in this country by Mr. H. Medlock, in January 18, 1860, and as their patent is dated May 26, 1860, we do not think it necessary to describe it any further.

Improvements in Obtaining Colouring Matters for Dyeing and Printing. By JOHN DALE and HEINRICH CARO.

First Process.—Purples.—An aqueous solution of a salt of aniline is mixed with a solution of chloride of copper, or mixtures of soluble salts of copper, with alkaline chlorides. On boiling these solutions, a black or dark purple precipitate is formed, containing the colouring matters. The following is a more precise direction:—One equivalent of a natural [doubtless a printer's error for neutral] salt of aniline is mixed with six equivalents of perchloride of copper, dissolved in as much water as is equal to thirty times the weight of the aniline used. The mixture having been boiled to determine the formation of the dark precipitate previously alluded to, is to be filtered, and washed with a weak solution of an alkali or alkaline carbonate until all soluble chlorides are removed. The purple colour may be extracted by means of boiling water, and from the aqueous solution the colouring matter may be precipitated by an alkali or alkaline carbonate, the solution of such precipitate in methylated spirit may be used for dyeing or printing.

A portion of the dark precipitate refuses to dissolve in boiling water; it is, therefore, to be dried at 212° F., and exhausted with methylated spirit, specific gravity 0.95. The spirit having been removed by distillation, the colouring matter is to be thrown down in the same manner as before. The precipitate is to be dried and extracted as previously described; the solution in methylated spirit may be used for dyeing or printing in the same manner as the other aniline purples.

Second Process.—Reds.—The patentees employ mixtures containing aniline, an anhydrous nitrate, and an anhydrous acid. The following is a condensed account of their method of working:—Two parts of aniline and two parts of nitrate of lead, dry and in fine powder, are to be raised to the boiling point of aniline [360° F.]. One part of anhydrous phosphoric acid is then added in small portions, with constant agitation, the temperature not being allowed to rise above 380° F. In an hour and a-half or two hours the operation will be complete. The colour is to be extracted from the mass by hot water, and may be precipitated by common salt. The solution in methylated spirit is used for dyeing and printing.

The patentees also give the following process which, if equally productive, and if the colour be equally good, is

preferable on account of its dispensing with anhydrous phosphoric acid, a re-agent which is excessively unmanageable on a large scale. One part of aniline is saturated with dry, muriatic acid gas. The resulting compound is to be heated with constant agitation to 380° F. for the same time as before, two parts of dry and powdered nitrate of lead being added in small portions. The extraction, &c. of the colour from the mass is to be conducted as before.

This patent appears to us to be exceedingly interesting and instructive. It opens up a new class of aniline reactions. It is true that the final result is probably the same with all processes for violets; and also that the reds producible by such a vast number of decompositions are all essentially the same; nevertheless, some of the modes of operating which have been patented yield such inferior products that it is evident they were never intended to be followed. We do not think this reproach applies to the patent last described.

CORRESPONDENCE.

An Apparatus for Finding the Density of Vapours and Gases.

To the Editor of the CHEMICAL NEWS.

SIR,—The density of steam and other vapours and gases could be found by constructing a framework of sails, which could be worked in any convenient manner, and placing it, after being set in motion, into the chamber or vessel containing the gas or vapour, when the rapidity of its rotation would indicate the comparative density of the resisting medium; inasmuch as, whatever the density of this, the pressure would be exerted upon the contrivance equally from every direction, the results with gases and vapours of various densities would be proportionable, and therefore correct. It is of course supposed that the number of the rotations would be registered, and from this their velocity could clearly be found.

Mr. Babbage has ("Economy of Machinery and Manufactures") proposed a framework of sails for measuring the altitudes of mountains in this manner.—I am, &c.

J. ALEXANDER DAVIES.

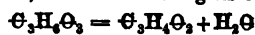
January 14, 1861.

Chemical Notices from Foreign Sources.

II. ORGANIC CHEMISTRY.

Constitution of Lactic Acid.—Kolbe and Wurtz discuss the constitution and basicity of lactic acid,—the former in the *Annalen der Chemie und Pharmacie*, bd. cxliii. s. 223; the latter in the *Annales de Chimie et de Physique*, t. lix. p. 161. Kolbe believes that lactic acid is monobasic. His reasons are the following:—1. Propionic, chloropropionic, and lactic are derived from one another by substitution. The two former are monobasic; therefore, lactic acid must be so. 2. No neutral lactate is known containing two equivalents of oxide. 3. We know no neutral lactic ether containing two atoms of basic ethyl. 4. No diamide of lactic acid is known. Wurtz admits that in a majority of cases lactic acid is only monobasic, but, at the same time, contends that it must be diatomic. He distinguishes between the word *diatomic*, which is applied to the molecular constitution of a body, and *bibasic*, which expresses the capacity of saturation. In answer to Kolbe, Wurtz shows that lactic acid is also derived from propyl-glycol, a diatomic compound, by simple substitution. Lactic acid is also allied to the bibasic acids by the circumstance that it may be changed

into the anhydrous acid by the simple elimination of one molecule of water, without doubling its own molecule.



Lactic acid. Lactide.

It is impossible, says Wurtz, to quote a single monatomic acid which possesses this character.

Solubility of Glucose in Alcohol.—Anthon has determined (*Chemisches Centralblatt*, bd. v. s. 292) that glucose is soluble in alcohols of different strengths, at 64° F., to the following extent:—

	Specific gravity.				
100 parts of alcohol	0.837	will dissolve	1.95	parts of glucose	
" "	0.880	"	9.30	" "	"
" "	0.910	"	17.74	" "	"
" "	0.950	"	36.45	" "	"

After some time, however, a little glucose will crystallise from these solutions, and the following numbers, the author says, will show the real solubility:—

	Specific gravity.		Per cent.		
Alcohol 0.837		will hold in solution	1.94	of glucose	
" 0.880	"	"	8.10	"	"
" 0.910	"	"	16.00	"	"
" 0.950	"	"	32.50	"	"

LABORATORY MEMORANDA.

Preparation of Phosphuretted Hydrogen Gas.—This body may be obtained with as much facility as hydrogen itself, by the following modification of an old process:—Fit a tube-funnel and a delivery-tube to a Florence, or other flask; introduce some pieces of phosphorus and about a couple of ounces of water; boil briskly until a spontaneously inflammable jet of phosphorised steam issues from the delivery-tube; lastly, gradually add strong solution of caustic soda, or potash, by the tube-funnel. By this method, the gas is given off very regularly, and the upper part of the flask being full of steam, no unpleasant explosions occur there.—*J. Attfield.*

MISCELLANEOUS.

Chemical Society.—At the next Meeting, on February 7, a paper will be read by Professor Field, "On the Carbonates of Copper, Nickel, and Cobalt."

Royal Institution.—The following Lectures will be delivered at this Institution during the ensuing week:—Monday, February 4, at 2 o'clock, General Monthly Meeting. Tuesday, February 5, at 3 o'clock, Professor Owen "On Fishes." Thursday, February 7, at 3 o'clock, Professor Tyndall "On Electricity." Friday, February 8, at 8 o'clock, Professor T. H. Huxley "On the Nature of the Earliest Stages of the Development of Animals." Saturday, February 9, at 3 o'clock, Dr. E. Frankland "On Inorganic Chemistry."

Sugar. Wine. Tanning.—We extract two or three statements from the *Moniteur Universel* which our readers may take for what they are worth. The first is, that a sugar-maker in Louisiana has discovered that the cane-juice when exposed to a hot sun will granulate or crystallise without giving any molasses. Another is, that wine and brandy acquire the properties of old age after being submitted for a time to diminished atmospheric pressure. The third is, that if Divi-divi be used in place of bark in tanning a skin, it is as effectually tanned in fifteen days as in several months by the old process.

ANSWERS TO CORRESPONDENTS.

THE CHEMICAL SOCIETY.

The Council of the above Society having expressed to us their objection to seeing papers which have been read at the Meetings of the Society published in our columns, we beg to inform gentlemen who desire to have their discoveries published in the *CHEMICAL NEWS*, that their papers must in future be forwarded to us before they are sent to the Chemical Society.

*. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

*. All Editorial Communications are to be addressed to Mr. CROOKES and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the *CHEMICAL NEWS*, containing a copious index, is now ready, price 12s. by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1867, and will be complete in 26 numbers.

Colloid Balloons may be made by pouring some colloid of the proper consistency into a perfectly dry glass globe or flask of the desired shape. When the ether has evaporated the film will separate from the glass. Surgical, not photographic, colloid should be used.

Cracking of Kitchen Boilers.—W. asks if there is any other cause for a kitchen boiler cracking besides want of water, or sudden influx of cold water into the hot boiler. When the inside of the boiler gets thickly coated with fur it is very liable to crack. We have known several boilers cracked by the servant sending the point of the kitchen poker through them.

W. H.—No particular time was mentioned, twenty minutes would be about right. The bottom layer should be the mixture of cryolite, &c. Any large Operative Chemist should now supply you with sodium at a cheap rate. We do not know the present price; but it is only a shilling or two per ounce.

C. E.—Cyanide of calcium is not at present an article of commerce: It would have to be made to order by a Manufacturing Chemist, and of course a considerable price would have to be paid. We are much obliged for the information.

J. P. P.—The quantity of iron in 1 grain of the compound $\text{Fe}_2\text{O}_3\text{PO}_4$ may be calculated by the following formula: as 151 (eq. of $\text{Fe}_2\text{O}_3\text{PO}_4$) : 1 grain :: 56 (2 eqs. of Fe) : the amount required.

Paste or Liquid Blacking.—A correspondent (C. Eades) has favoured us with the following receipt in answer to an inquiry a few weeks ago:—Fuller's earth, 8oz.; treacle, 3lbs.; ivory black, 1lbs.; butter scrapings, 4oz.; rape oil, 4oz.; strong gum-water, half-a-pint; powdered Prussian blue, 4oz.; oil of vitriol, 8oz. if wanted liquid, use half-a-gallon of vinegar.

J. H. M., M.A.—Apply to the Secretary at Burlington House, who will forward every particular: you would be quite eligible, in our opinion.

A Subscriber, F.C.S., F.G.S.—We do not think such an advertisement ever appeared in our columns. Chlorate of potash can be obtained at any Chemist's.

A Subscriber wishes to know the most recent work or publication giving information on the subject of distilling wood, making naphtha, acetate of lime.

R. C. C. writes as follows:—"I have lately heard there is a plant growing in the Caucasus, which yields nitrate of potash in sufficient quantity to make gunpowder. Can you tell me anything about it? Can you also let me know the best method of preparing arsenic acid in quantities?"

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26s. per dozen, usual Discount—Showcards.

We feel it incumbent upon us to warn the Trade against buying an article purporting to be the genuine, and which is sold at lower price. The original has "F. NEWBERRY, No. 45, St. Paul's Churchyard," on the Government Stamp.

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THE CHEMICAL NEWS

VOL. III. No. 62.—February 9, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Molecular Constitution of Glycollic and Lactic Acids, by W. H. PERKIN, F.C.S.

THE artificial formation of glycollic acid from the chlorine and bromine derivatives of acetic acid, and also the transformation of lactic acid into chloropropionic acid, have of late brought these interesting substances under the consideration of chemists, and many are the views regarding their constitution which have been brought forward during the last two or three years. It is, therefore, with some diffidence that I venture to add anything to what has been already written on this subject; but as what I have subjoined is supported by experimental evidence, I have had the boldness to produce it.

Before entering into any considerations regarding either glycollic or lactic acids, I wish to call attention to those substances from which they are derived,—namely, acetic and propionic acids.

Gerhardt considers acetic acid, propionic acid, and all other acids of the formula $C_nH_{2n}O_2$, to consist of formic acid, in which one equivalent of hydrogen is replaced by an alcohol radical, such as methyle or ethyle, thus:—

Formic acid may be written . . . C_2HHO_2
Acetic acid (or, methyle formic) . . . $C_2(C_2H_5)HO_2$
Propionic acid (or, ethyle formic) . . . $C_2(C_4H_9)HO_2$

Which similitude of constitution is justified, as he remarks, by the decomposition of these acids when subjected to the influence of the electric battery, yielding, as is well known, carbonic acid and anhydro-carbons which are the alcohol radical of which they are in part built up, thus:—

Formic acid . . . gives Hydrogen
Acetic acid . . . „ Methyle
Propionic acid . . . „ Ethyle.

Also, the decomposition of the cyanides of alcohol radicals under the influence of the hydrate of potassium give additional confirmation to this view.

If, then, we view acetic and propionic acids as methyle and ethyle formic acids, what is the constitution of formic acid?

I think the following table throws much light upon this subject:—

	C_2O_2''	Carbonyle.
	$\begin{matrix} Cl \\ C_2O_2'' \\ Cl \end{matrix}$	Chloride of Carbonyle.
	$\begin{matrix} H \\ C_2O_2'' \\ H \end{matrix}$	Formic aldehyde. (Hydride of Carbonyle.)
Chlorocarbonic acid	$\begin{matrix} H \\ C_2O_2'' \\ Cl \end{matrix}$	O_2 Chloroformic acid. (Chloride and Hydrate of Carbonyle.)

$\begin{matrix} H \\ C_2O_2'' \\ H \end{matrix}$ O_2 Formic acid.
(Hydride and Hydrate of Carbonyle.)

$\begin{matrix} H \\ C_2O_2'' \\ H \end{matrix}$ O_2 Carbonic acid.
 O_2 (Hydrate of Carbonyle.)

Here formic acid is represented as a compound of carbonyle. If we replace part or all of the hydrogen in these compounds by methyle and ethyle, we obtain acetic acid, propionic acid, and their derivatives, thus:—

$\begin{matrix} Cl \\ C_2O_2'' \\ Cl \end{matrix}$ Chloride of Carbonyle.

$\begin{matrix} Cl \\ C_2O_2'' \\ C_2H_5 \end{matrix}$ Chloride of Acetyle.
(Chloride and Methylide of Carbonyle.)

$\begin{matrix} Cl \\ C_2O_2'' \\ C_4H_9 \end{matrix}$ Chloride of Propionyle.
(Chloride and Ethylide of Carbonyle.)

Aldehyde $\begin{matrix} H \\ C_2O_2'' \\ C_2H_5 \end{matrix}$ Hydride of Acetyle.
(Hydride and Methylide of Carbonyle.)

Propionic aldehyde $\begin{matrix} H \\ C_2O_2'' \\ C_4H_9 \end{matrix}$ Hydride of Propionyle.
(Hydride and Ethylide of Carbonyle.)

$\begin{matrix} C_2H_5 \\ C_2O_2'' \\ C_2H_5 \end{matrix}$ Acetone.
(Methylide of Carbonyle)

$\begin{matrix} C_4H_9 \\ C_2O_2'' \\ C_4H_9 \end{matrix}$ Propione
(Ethylide of Carbonyle.)

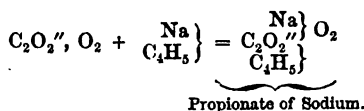
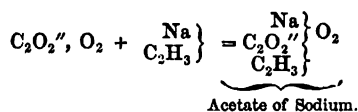
$\begin{matrix} H \\ C_2O_2'' \\ C_2H_5 \end{matrix}$ O_2 Acetic acid.
(Hydrate and Methylide of Carbonyle.)

$\begin{matrix} H \\ C_2O_2'' \\ C_4H_9 \end{matrix}$ O_2 Propionic acid.
(Hydrate and Ethylide of Carbonyle.)

$\begin{matrix} H \\ C_2O_2'' \\ C_2H_5 \end{matrix}$ O_2 Methyle Carbonic acid.
 O_2 (Hydrate of Oxide of Methyle and Carbonyle.)

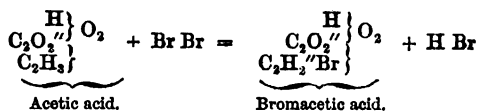
$\begin{matrix} H \\ C_2O_2'' \\ C_4H_9 \end{matrix}$ O_2 Ethyle Carbonic acid.
 O_2 (Hydrate of the Oxide of Methyle and Carbonyle.)

That the formulæ for acetic and propionic acids given in the above table are correct, I think the experiments of Wanklyn prove. As all are aware, he found that the remarkable substances sodium-methyle and likewise sodium ethyle, when brought in contact with carbonic anhydride (oxide of carbonyle), combined, yielding acetate of sodium in the one case and propionate of sodium in the other, thus:—

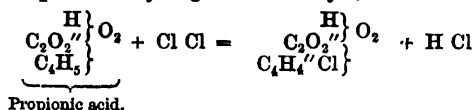


Acetic acid thus written may be viewed as the hydrate of the methyloide of carbonyle, or, perhaps more clearly, as a compound of monohydrate of carbonyle ($\text{C}_2\text{O}_2''$) $\left\{ \begin{array}{c} \text{H} \\ \text{O}_2 \end{array} \right\}$ with methyle, and in the case of propionic acid, with ethyle.

When bromine acts upon acetic acid it is evident that it must attack the hydrogen in the methyle thus:—



Or if chlorine is made to act upon propionic acid, it must replace the hydrogen in the ethyle,—



Bromoacetic acid represents a compound of monohydrate of carbonyle ($\text{C}_2\text{O}_2''$) $\left\{ \begin{array}{c} \text{H} \\ \text{O}_2 \end{array} \right\}$ with monobromide of methylene ($\text{C}_2\text{H}_2''\text{Br}$), while chloropropionic acid represents a similar compound, containing the monochloride of ethylene ($\text{C}_4\text{H}_4''\text{Cl}$) in place of the monobromide of methylene.

The manner in which the bromine, in bromoacetic acid, or the chlorine, in chloropropionic acid behaves towards re-agents is remarkable. Bromoacetic acid, for example, when treated with ammonia, yields glycolle, just in the same manner as bromide of methyle and ammonia yields methylamine.

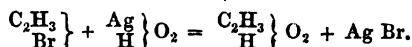
Chloropropionate of ethyle and butyrate of sodium re-act, forming an ethereal body, containing butyric acid, just as chloride of ethyle would act upon the same salt and produce butyric ether.

Bromoacetic ether likewise reacts on acetate of potassium, benzoate of potassium, and succinate of potassium, producing bodies having properties somewhat similar to acetate, benzoate, or succinate of methyle.

Bromoacetate of ethyle also acts upon picoline and other nitryle bases producing the bromides of ammoniums exactly as bromide of ethyle does.

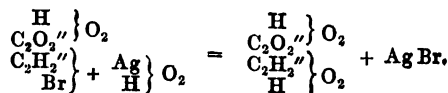
From these facts, it is evident that chloropropionic acid has the double property of an acid and of a chloride of an electro-negative radical, and that bromoacetic acid has the twofold property of an acid and of a bromide of an electro-negative radical, as the formula I have proposed for them shows.

When bromide of methyle is decomposed by hydrate of silver, hydrate of methyle (methylic alcohol) and bromide of silver are formed,—



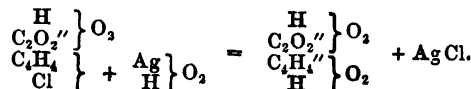
A similar reaction takes place when bromoacetic acid is treated in the same manner, the monobromide of methylene of the acid being converted into a hydrate of

methylene, and the hydrate of silver into the bromide of silver, thus:—



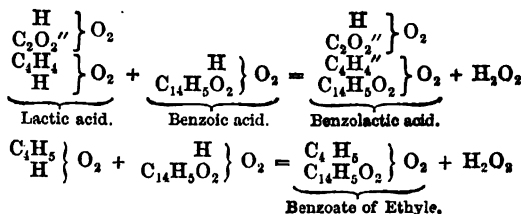
Consequently, glycollic acid may be said to consist of the monohydrate of carbonyle and monohydrate of methylene, the former acting as an acid, and the latter as an alcohol.

The formation of lactic acid from chloropropionic acid, under the influence of hydrate of silver, is precisely similar to the formation of glycollic acid, and may be represented thus:—

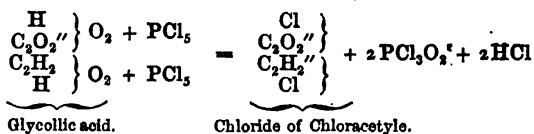
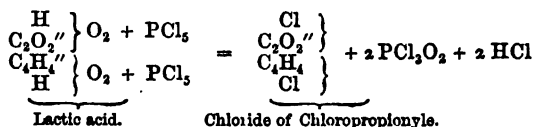


Therefore, lactic acid may be represented as consisting of the monohydrate of carbonyle and monohydrate of ethylene.

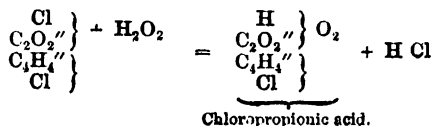
The formation of benzolactic acid, by the action of heat on a mixture of lactic and benzoic acids, goes to show the existence of an alcohol in lactic acid, and is also analogous to the formation of benzoic ether from alcohol and benzoic acid:—

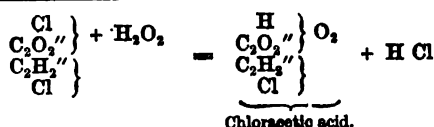


The fact of these substances having the twofold character of an acid and alcohol shows why they are monobasic, although representing two molecules of water. It also explains the reason why lactic acid when distilled with pentachloride of phosphorus yields chloride of chloropropionyle, and also why glycollic acid, under similar circumstances, yields the chloride of chloroacetylo, thus:—

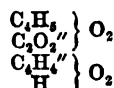


As chloride of chloropropionyle and chloride of chloroacetylo consist of the chloride of an electro-positive radical, and the chloride of an electro-negative radical, they are only half decomposed by water, as the chlorides of the electro-negative radicals are not easily decomposed by that agent, thus:—



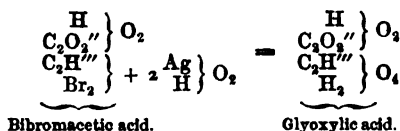


Being still desirous of further verifying the idea of glycollic and lactic acids being both acids and alcohols, I made further experiments. I wished first to get rid of the acid properties of one of these acids, so that I might study its alcoholic nature alone. For this purpose I selected lactic ether, which may be represented thus:—

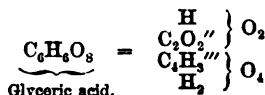


This ether dissolves sodium with evolution of hydrogen just as alcohol does. If brought in contact with the chloride of acetyl or chloride of succinyle it evolves large quantities of hydrochloric acid, with formation of oily ethereal bodies, just as ordinary alcohol does. Therefore, I think there can be no doubt that they possess the properties of alcohols.

The decomposition of acids containing a still greater amount of bromine or chlorine than bromacetic or chloropropionic acids yield also analogous results,—thus, bibromacetic acid and hydrate of silver yields an acid which represents a diatomic alcohol and a monobasic acid:—



This acid is analogous to glyceric acid, which may be expressed in a similar manner:—



On the Analysis of Milk, by JOHN HORSLEY.

1000 grains, specific gravity 10°30, on evaporation to dryness yielded 100 grains, or $\frac{1}{10}$ th, of solid matter, which, by a set of careful experiments with further quantities of the same milk, was found to consist of

Butter	20 parts
Casein	35 "
Lactin and salts	45 "
	100 "

M. Poggiale's experiments with milk make its composition as follows, but unfortunately the time of year is not stated, which is important, since milk is richer in the summer than winter; and from the very great difference in the amount of solids, yield of butter, &c., it is most probable his experiments were conducted in the summer time:—

Butter	43·8 parts
Casein	38 "
Lactin and salts	55·4 "
	137·2 "

TECHNICAL CHEMISTRY.

On Coal Gas, by the Rev. W. R. BOWDITCH.

(Continued from page 69.)

Although collateral matter has been carefully excluded from this paper, I cannot refrain from remarking that the property of clay here mentioned is in fact the announcement of a new property of soils, and one which will help to account for the formation of many natural metallic sulphides. I hope soon to have some investigations on this subject ready for publication.

To ascertain whether this property of breaking up a sulphurised compound in purified gas and removing tar was possessed by clay alone, or shared by other substances used in purification, some purified gas was passed through a considerable chemical excess of all the substances employed in purification, viz. lime, precipitated peroxide of iron, sulphate of iron, chloride of calcium, and dilute sulphuric acid, all but the lime being mixed with moist sawdust. Upon passing the gas next through a purifier filled with clay, it darkened lead-paper, and affected turmeric when it had passed a subsequent purifier filled with lime. This proves the power of clay to break up one or more sulphurised compounds which no other substance used in purification effects; and if this sulphur were not liberated from the impure naphthalin compound already mentioned, it seemed certain that gas which had been previously purified by clay might be much improved, if not rendered pure, by a removal of the sulphur of the naphthalin. There is strong experimental evidence that the compound from which clay liberates sulphide of hydrogen is not the sulphurised naphthalin one; for if hydrogen be passed through a vessel containing this substance, then through clay, and subsequently over lead-paper, no trace of sulphide of hydrogen is found, though the gas passing smells strongly of impure naphthalin. This gas and vapour burn with a lightless flame. Subsequently some naphthalin was heated to ebullition, and a current of hydrogen sent through it and then burnt. The flame was lightless as before. I mention this fact to remove the popular error that naphthalin, as it exists in coal-gas, is a good illuminant. Even Dr. Frankland thus regards it; and both in Clegg's book on coal-gas, and in the new edition of Ure's Dictionary, states that the hydrocarbons in gas are valuable in proportion to the carbon they contain, and that naphthalin is the most valuable as containing the largest proportion of carbon. The above-mentioned experiments evince the contrary. On another occasion I determined the illuminating power of some gas, and then, without alteration of the quantity passing, sent the gas through a U-tube containing naphthalin from the London gas-mains. The character of the flame was changed from white to red, but the photometer indicated no difference in the light given. Two other persons conversant with photometry were present at this experiment and agreed in the result, though up to that time they had held the prevailing opinion as to the value of naphthalin in gas without testing the statements made upon the subject.

Another sulphur compound is said always to be present in coal-gas and to be irremovable, and which, like those I have hitherto spoken of, does not affect lead-paper, viz. bisulphide of carbon. To ascertain the presence of bisulphide of carbon, I pass gas through strong spirit of wine (methylated spirit answers perfectly) kept at about 160° F. The gas and vapours pass out of

the flask which contains the spirit up a long tube into an inverted flask, so that all which is condensed may run back into the spirit. It then passes into another flask for additional condensation, and thence forward to a gas-holder or burner. Bisulphide of carbon dissolved in spirit becomes precipitated as a white cloud which settles to the bottom of the vessel, when the spirit is copiously diluted with water. The white cloudy precipitate escapes slowly by single bubbles through the diluted spirit, and at length leaves a solution perfectly clear. The spirit through which gas has passed, and from which it has abstracted bisulphide of carbon, acts in precisely the same manner upon dilution, and no one who has seen the reaction once or twice can possibly mistake it. The study of other compounds led me to conclude that if this substance exist *as such*, and not merely *by its elements*, in gas, it could be removed almost as easily as the naphthalin compound could be purified, and that the same process could be made available to remove the sulphur of both. I thought that under certain conditions the affinity of hydrogen for sulphur would exceed that of carbon for sulphur, and therefore that I might obtain the sulphur of bisulphide of carbon as a sulphide of hydrogen, about the removal of which there is no difficulty. Experiment confirms the reasoning. When hydrogen mixed with vapour of bisulphide of carbon is passed through a tube filled with slaked lime or clay which has been dried at 400° or 500° F., and is kept between 400° and 600° F. during the passage of the gas and vapour, not a trace of bisulphide of carbon passes from the tube, but sulphide of hydrogen does pass. The lime is darkened by a deposit of carbon, and a sublimate of sulphur is found in the exit-tube. A considerable excess of hydrogen should be used, or else a portion of the bisulphide of carbon vapour is carried over by the current and escapes decomposition. That this reaction is not the result of heat merely, but is a truly chemical one which the base under the influence of heat effects, and the remarkable fact that slaked lime when heated forms, but does not unite with, sulphide of hydrogen, receive illustration from the following experiments.

Hydrogen and vapour of sulphide of carbon were passed through—1st, cold slaked lime; 2nd, cold clay; 3rd, hot oxide of iron used at a gas-works in purification; 4th, hot broken bricks; 5th, hot broken glass, without in any instance producing sulphide of hydrogen. On the contrary, when passed through (1) hot lime and (2) hot clay, sulphide of hydrogen was formed and passed over immediately, and continued to pass as long as the current was kept up. The lime, when cooled out of contact with the air, gave no sulphide of hydrogen upon being supersaturated with dilute sulphuric acid, but clay when thus treated gave off much.

The hydrogen used was in all cases passed through lime and over lead-paper, to secure its being free from sulphide before use. On one occasion, when the clay had been imperfectly dried before heating, I observed much sulphurous acid instead of sulphide of hydrogen. I therefore passed hydrogen, bisulphide of carbon vapour, and steam over hot clay which had been properly dried. At first sulphide of hydrogen passed over alone, then mixed with sulphurous acid, which at length passed alone. Subsequently sulphide of hydrogen passed, and at length sulphurous acid ceased. As the one gas increased the other diminished, and throughout the experiment sulphide of carbon vapour passed undecomposed. It is shown by this experiment that hot clay in the presence of more water than forms a hydrate acts very

differently from the same clay when dry, and the whole subject deserves a full investigation.

Action of Sulphide of Hydrogen upon Clay and Lime, Cold and Hot.—Well washed sulphide of hydrogen passed into cold slaked lime (obtained from Buxton) in a tube, colours the lime green as soon as it comes into contact with it, and the progress of the gas along the tube corresponds with the coloration. Lead-paper is not affected until the lime becomes coloured close up to the exit.

The same gas, passed into a tube containing slaked lime, kept about 600° F. at the middle, but cool at both ends, acts differently. The cool lime at the inlet end becomes coloured; the hot lime in the middle remains white, and the cool lime at the exit end becomes coloured, and lead-paper is stained as soon as these two cool portions are saturated, while the middle portion remains unchanged in colour.

The same gas, passed into a tube containing hot lime only, causes no discoloration, but instantly blackens lead-paper placed at the exit end; and upon being conducted into a tube of cold lime, colours it as if it had just passed from the vessel in which it is produced.

The same gas was passed into a tube containing lime which had been thoroughly dried at 600° F., and cooled out of contact with the air. No discoloration of the lime took place, but the gas passed unaffected by the lime, and blackened lead-paper. Water added to the lime gives it the power of decomposing the gas as if it had not been heated. The presence of more water than is necessary to form hydrate of lime (Ca O, HO) is thus shown to be required for the decomposition of sulphide of hydrogen by slaked lime.

Sulphide of hydrogen passed into a tube of cold clay is taken up in considerable quantity, and the clay becomes black from the formation of sulphide of iron. The blackening begins at the inlet end, and progresses with the passage of the gas towards the exit end of the tube.

The same gas, passed into clay, heated to 500° or 600° F., gives the same reactions; but when the clay has been heated and well-dried, and cooled in the closed tube, it takes up a very small quantity of the gas.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures¹ (adapted to a Juvenile Auditory), on the Chemical History of a Candle; by M. FARADAY, D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.

LECTURE VI. (Jan. 8, 1861.)

Carbon or Charcoal—Coal Gas—Respiration and its Analogy to the Burning of a Candle—Conclusion.

A lady who honours me by her presence at these Lectures has conferred a still further obligation by sending me these two candles which are from Japan, and, I presume, are made of that substance to which I referred in a former Lecture. You see that they are even far more highly ornamented than the French candles, and, I suppose, are candles of luxury, judging from their appearance. They have a remarkable peculiarity about them, namely, a hollow wick,—that beautiful peculiarity which Argand introduced into the lamp and made so valuable. To those who receive such presents

¹ Reported verbatim by special permission.

from the East, I may just say that this and such like materials, gradually assume a change which gives them on the surface a dull and dead appearance; but they may easily be restored to their original beauty if the surface is rubbed with a clean cloth or silk handkerchief, so as to polish the little rugosity or roughness; this will restore the beauty of the colours. I have so rubbed one of these candles, and you see the difference between it and the other which has not been polished, but which may be restored by the same process. Observe, also, that these moulded candles from Japan are made more conical than the moulded candles in this part of the world.

I told you, when we last met, a good deal about carbonic acid. We found by the lime-water test that when the vapour from the top of the candle or lamp was received into bottles and tested by this solution of lime water (the composition of which I explained to you, and which you can make for yourselves), we had that white opacity which was in fact calcareous matter, like shells and corals, and many of the rocks and minerals in the earth. But I have not yet told you clearly and chemically the history of this substance, carbonic acid, as we have it from the candle, and I must now take you to that point. We have seen the products, and the nature of them, as they issue from the candle. We have traced the water to its elements, and now we have to see where are the elements of the carbonic acid supplied by the candle: a few experiments will show this. You remember that when a candle burns badly it burns with smoke; but if it is burning well there is no smoke. And you know that the brightness of the candle is due to this smoke which becomes ignited. Here is an experiment to prove this: so long as the smoke remains in the flame of the candle and becomes ignited it gives a beautiful light, and never appears to us in the forms of black particles. I will light some fuel which is extravagant in its burning; this will serve our purpose—a little turpentine on a sponge. You see the smoke rising from it, and floating into the air in large quantities, and remember now, the carbonic acid that we have from the candle is from such smoke as that. To make that evident to you I will introduce this turpentine burning on the sponge into a flask where I have plenty of oxygen, the rich part of the atmosphere, and you see that the smoke is all consumed. This is the first part of our experiment, and now what follows? The carbon which you saw flying off from the turpentine flame in the air we have now entirely burned in this oxygen, and we shall find that it will by this rough and temporary experiment, give us exactly the same conclusion and result as we had from the combustion of the candle. The reason why I make the experiment in this manner is solely that I may cause the steps of our demonstration to be so simple that you can never for a moment lose the train of reasoning if you only pay attention. All the carbon which is burned in oxygen, or air, comes out as carbonic acid, whilst those particles which are not so burned show you the second substance in the carbonic acid, namely the carbon, that body which made the flame so bright whilst there was plenty of air, but which was thrown off in excess when there was not oxygen enough to burn it.

I have also to show you a little more distinctly the history of carbon and oxygen in their union to make carbonic acid. You have now a right to know this to a far greater extent than before, so I have three or four experiments for that purpose. I have here a jar filled with oxygen, and here is some carbon which has been placed in a crucible for the purpose of being made red hot. I keep my jar dry and venture to give you a result imperfect in some degree, in order that I may make the experiment brighter. I am about to put the oxygen and the carbon together. That this is carbon (common charcoal pulverised) you will see by the way in which it burns in air [letting some of the red-hot charcoal fall out of the crucible]. I am now about to burn it in oxygen gas, and look at the difference. It may appear to you at a distance as if it were burning with a flame; but it is not so. Every little piece of charcoal is burning as a spark, and whilst it so burns it is producing carbonic acid. I specially want these two or three experiments to point out what I shall dwell upon more distinctly by-and-by—that carbon burns in this way, and not as a flame.

Instead of taking many particles of carbon to burn I will take a rather large piece, which will enable you to see the form and size, and to trace the effects very decidedly. Here is the jar of oxygen, and here is the piece of charcoal, to which I have fastened a little piece of wood, which I can

set fire to and so carry in the combustion, which I could not conveniently do without. You now see the charcoal burning, but not as a flame (or if there be a flame it is the smallest possible one, which I know the cause of, namely, the formation of a little carbonic oxide close upon the surface of the carbon). It goes on burning, you see, slowly producing carbonic acid by the union of this carbon or charcoal (they are equivalent terms) with the oxygen. I have here another piece of charcoal, a piece of bark, which has the quality of being blown to pieces—exploding—as it burns. By the effect of the heat we shall reduce the lump of carbon into particles that will fly off; still every particle, equally with the whole mass, burns in this peculiar way—it burns as a coal and not like a flame. You observe a multitude of little combustions going on, but no flame. I do not know a finer experiment than this to show that carbon burns with a spark.

Here, then, is carbonic acid formed from its elements. It is produced at once, and if we examined it by lime-water, you will see that we have the same substance which I have previously described to you. By putting together 6 parts of carbon by weight (whether it comes from the flame of a candle or from powdered charcoal) and 16 parts of oxygen by weight, we have 22 parts of carbonic acid; and, as we saw last time, the 22 parts of carbonic acid combined with 28 parts of lime, produce common carbonate of lime. If you were to examine an oyster-shell and weigh the component parts, you would find that every 50 parts would give 6 of carbon and 16 of oxygen combined with 28 of lime. However, I do not want to trouble you with these minutiae; it is only the general philosophy of the matter that we can now go into. See how finely the carbon is dissolving away [pointing to the lump of charcoal burning quietly in the jar of oxygen]. You may say that the charcoal is actually dissolving in the air round about, and if that were perfectly pure charcoal, which we can easily prepare, there would be no residue whatever. When we have a perfectly cleansed and purified piece of carbon there is no ash left. The carbon burns as a solid dense body, that heat alone cannot change as to its solidity, and yet it passes away into vapour that never condenses into solid or liquid under ordinary circumstances; and what is more curious still is the fact that the oxygen does not change in its bulk by the solution of the carbon in it. Just as the bulk is at first, so it is at last, only it has become carbonic acid.

There is another experiment which I must give you before you are fully acquainted with the general nature of carbonic acid. Being a compound body, consisting of carbon and oxygen, carbonic acid is a body that we ought to be able to take asunder. And so we can. As we did with water so we can with carbonic acid,—take the two parts asunder. The simplest and quickest way is to act upon the carbonic acid by a substance that can attract the oxygen from it, and leave the carbon behind. You recollect that I took potassium and put it upon water or ice, and you saw that it could take the oxygen from the hydrogen. Now, suppose we do something of the same kind here with this carbonic acid. You know carbonic acid to be a heavy gas: I will not test it with lime-water, as that will interfere with our subsequent experiments, but I think the heaviness of the gas and the power of extinguishing flame will be sufficient for our purpose. I introduce a flame into the gas, and you will see whether it will put it out. You see the light is extinguished. Indeed, the gas may, perhaps, put out phosphorus, which you know has a pretty strong combustion. Here is a piece of phosphorus heated to a high degree. I introduce it into the gas, and you observe the light is put out, but it will take fire again in the air, because there it re-enters into combustion. Now let me take a piece of potassium, a substance which even at common temperatures can act upon carbonic acid, though not sufficiently for our present purpose, because it soon gets covered with a protecting coat; but if we warm it up to the burning point in air, as we have a fair right to do, and as we have done with phosphorus, you will see that it can burn in carbonic acid; and if it burns it will burn by taking oxygen, so that you will see what is left behind. I am going, then, to burn this potassium in the carbonic acid as a proof of the existence of oxygen in the carbonic acid. [In the preliminary process of heating the potassium exploded.] Sometimes we get an awkward piece of potassium that explodes, or something like it, when it burns. I will take another piece, and now that it is heated I introduce it into the jar, and you perceive that it burns in the carbonic acid—not so

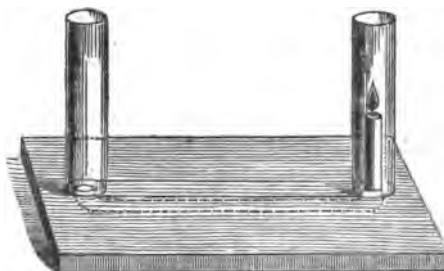
well as in the air, because the carbonic acid contains the oxygen combined, but it does burn, and takes away the oxygen. If I now put this potassium into water I find that besides the potash formed (which you need not trouble about) there is a quantity of carbon produced. I have here made the experiment in a very rough way, but I assure you that if I were to make it carefully, devoting a day to it, instead of five minutes, we should get all the proper amount of charcoal left in the spoon, or in the place where the potassium was burned, so that there could be no doubt as to the result. Here, then, is the carbon obtained from the carbonic acid, as a common black substance; so that you have the entire proof of the nature of carbonic acid as consisting of carbon and oxygen. So now, I may tell you, that whenever carbon burns under common circumstances, it produces carbonic acid.

Suppose I take this piece of wood, and put it into a bottle with lime-water. I might shake that lime-water up with wood and the atmosphere as long as I pleased, it would still remain clear as you see it; but suppose I burn the piece of wood in the air of that bottle. You, of course, know I get water. Do I get carbonic acid? [The experiment was performed.] There it is you see, that is to say, the carbonate of lime, which results from carbonic acid, and that carbonic acid must be formed from the carbon which comes from the wood, from the candle, or any other thing. Indeed, you have yourselves frequently tried a very pretty experiment, by which you may see the carbon in wood. If you take a piece of wood, and partly burn it, and then blow it out, you have carbon left. There are things that do not show carbon in this way. A candle does not so show it, but it contains carbon. Here also is a jar of coal-gas, which produces carbonic acid abundantly,—you do not see the carbon, but we can soon show it to you. I will light it, and as long as there is any gas in this cylinder it will go on burning. You see no carbon, but you see a flame, and because that is bright it will lead you to guess that there is carbon in the flame. But I will show it to you by another process. I have some of the same gas in another vessel, mixed with a body that will burn the hydrogen of the gas, but will not burn the carbon. I will light them with a burning taper, and you perceive the hydrogen is consumed, but not the carbon, which is left behind as a dense black smoke. I hope that by these three or four experiments you will learn to see when carbon is present, and understand what are the products of combustion, when gas or other bodies are thoroughly burned in the air.

Before we leave the subject of carbon, let us make a few experiments and remarks upon its wonderful condition, as respects ordinary combustion. I have shown you that the carbon in burning burns only as a solid body, and yet you perceive that after it is burned, it ceases to be a solid. There are very few fuels that act like this. It is in fact only that great source of fuel, the carbonaceous series, the coals, charcoals, and woods, that can do it. I do not know that there is any other elementary substance besides carbon that burns with these conditions, and if it had not been so, what would happen to us? Suppose all fuel had been like iron which, when it burns, burns into a solid substance. We could not then have such a combustion as you have in this fire-place. Here also is another kind of fuel which burns very well—as well as, if not better, than carbon—so well, indeed, as to take fire of itself when it is in the air, as you see. [Breaking a tube full of lead pyrophorus.] This substance is lead, and you see how wonderfully combustible it is. It is very much divided, and is like a heap of coals in the fire-place: the air can get to its surface and inside, and so it burns. But why does it not burn in that way now when it is lying in a mass? [Emptying the contents of the tube in a heap on to a plate of iron.] Simply because the air cannot get to it. Though it can produce a great heat, the great heat which we want in our furnaces and under our boilers, still that which is produced cannot get away from the portion which remains unburned underneath, and that portion, therefore, is prevented from coming in contact with the atmosphere, and cannot be consumed. How different is that from carbon! Carbon burns just in the same way as this lead does, and so gives an intense fire in the furnace, or wherever you choose to burn it; but then the body produced by its combustion passes away, and the remaining carbon is left clear. I showed you how carbon went on dissolving in the oxygen, leaving no ash; whereas, here [pointing to the heap of pyrophorus] we have

actually more ash than fuel, for it is heavier by the amount of the oxygen which has united with it. Thus, you see, the difference between carbon and lead or iron, if we chose iron, which gives so wonderful a result in our applications of this fuel, either as light or heat. If when the carbon burnt here the product went off as solid body you would have had the room filled with an opaque substance, as in the case of the phosphorus; but when carbon burns everything passes up into the atmosphere. It is in a fixed, almost unchangeable condition before the combustion; but afterwards it is in the form of gas, which it is very difficult (though we have succeeded) to produce in a solid or liquid state.

Now I must take you to a most interesting part of our subject—to the relation between the combustion of a candle and that living kind of combustion which goes on within us. In every one of us there is a living kind of combustion going on exactly like that of a candle, and I must try to make that plain to you. For that is not merely true in a poetical sense—the relation of the life of man to a taper, and if you will follow, I think I can make this clear. In order to make the relation very plain, I have devised a little apparatus which we can soon build up before you. Here is a board and a groove cut in it, and I can close the groove at the top part by a little cover; I can then continue the groove as a channel



by a glass tube at each end, there being a free passage through the whole. Suppose I take a taper or candle (we can now be liberal in our use of the word "candle," since we understand what it means), and place it in one of the tubes; it will go on, you see, burning very well. You observe that the air which feeds the flame passes down the tube at one end, then goes along the horizontal tube, and ascends the tube at the other end in which the taper is placed. If I stop the aperture through which the air enters, I stop combustion, as you perceive. I stop the supply of air, and consequently the candle goes out. But now what will you think of this fact? In a former experiment I showed you the air going from one burning candle to a second candle. If I took the air proceeding from another candle, and sent it down by a complicated arrangement into this tube, I should put this burning candle out. But what will you say when I tell you that my breath will put out that candle? I do not mean by blowing at all, but simply that the nature of my breath is such that a candle cannot burn in it. I will now hold my mouth over the aperture, and without blowing the flame in any way, let no air enter the tube but what comes from my mouth. You see the result. I did not blow the candle out. I merely let the air which I expired pass into the aperture, and the result was that the light went out for want of oxygen, and for no other reason. Something or other—namely, my lungs—had taken away the oxygen from the air, and there was no more to supply the combustion of the candle. It is, I think, very pretty to see the time it takes before the bad air which I throw into this part of the apparatus has reached the candle. The candle at first goes on burning, but so soon as the air has had time to reach it it goes out. And now I will show you another experiment, because this is an important part of our philosophy. Here is a jar which contains fresh air, as you can see by the circumstance of a candle or gas-light burning in it. I make it close for a little time, and by means of a pipe I get my mouth over it so that I can inhale the air. By putting it over water, in the way that you see, I am able to draw up this air (supposing the cork to be quite tight), take it into my lungs, and throw it back into the jar: we can then examine it, and see the result. You observe, I first take up the air, and then throw it back, as is evident from the ascent and descent of the water, and now, by putting a taper into the air, you will see the state in which

it is by the light being extinguished. Even one inspiration, you see, has completely spoiled this air, so that it is no use



my trying to breathe it a second time. Now you understand the ground of the impropriety of many of the arrangements among the houses of the poorer classes, by which the air is breathed over and over again, for the want of a supply, by means of proper ventilation, sufficient to produce a good result. You see how bad the air becomes by a single breathing, so that you can easily understand how essential fresh air is to us.

To pursue this a little further, let us see what will happen with lime-water. Here is a globe which contains a

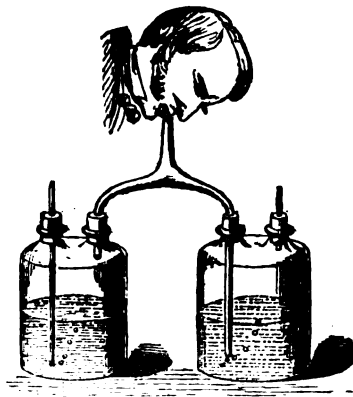


little lime-water, and it is so arranged as regards the pipes, as to give access to the air within, so that we can ascertain the effect of respired, or unrespired air upon it. Of course I can either draw in air (through A) and so make the air that feeds my lungs go through the lime-water, or I can force the air out of my lungs through the tube (B) which goes to the bottom, and so show its effect upon the lime-water. You will observe that however long I draw the external air into the lime-water, and then through it to my lungs, I shall produce no effect upon the water—it will not make the lime-water turbid; but if I throw the air *from* my lungs through the lime-water, several times in succession, you see how white and milky the water is getting, showing the effect which expired air has had upon it; and now you begin to know that the atmosphere which we have spoiled by respiration is spoiled by carbonic acid, for you see it here in contact with the lime-water.

I have here two bottles, one containing lime-water and the other common water and tubes which pass into the bottles and connect them. The apparatus is very rough, but it is useful notwithstanding. If I take these two bottles, inhaling here and exhaling there, the arrangement of the tubes will prevent the air going backwards. The air coming in, will go to my mouth and lungs, and in going out, will pass through the lime-water, so that I can go on breathing and making an experiment, very refined in its nature, and very good in its results. You will observe that the good air has done nothing to the lime-water; in the other case nothing has come to the lime-water, but my respiration, and you see the difference in the two cases.

Let us now go a little further. What is all this process

going on within us which we cannot do without, either day or night, which is so provided for by the Author of all things,



that He has arranged that it shall be independent of all will? If we restrain our respiration, as we can to a certain extent, we should destroy ourselves. When we are asleep, the organs of respiration and the parts that are associated with them, still go on with their action, so necessary is this process of respiration to us, this contact of the air with the lungs. I must tell you in the briefest possible manner, what this process is. We consume food: the food goes through that strange set of vessels and organs within us and is brought into various parts of the system, into the digestive parts especially; and alternately the portion which is so changed, is carried through our lungs by one set of vessels, while the air that we inhale and exhale, is drawn into and thrown out of the lungs by another set of vessels, so that the air and the food come close together, separated only by an exceedingly thin surface: the air can thus act upon the blood by this process, producing precisely the same results in kind as we have seen in the case of the candle. The candle combines with parts of the air, forming carbonic acid, and evolves heat; so in the lungs there is this curious, wonderful change taking place. The air entering, combines with the carbon (not carbon in a free state, but, as in this case, placed ready for action at the moment), and makes carbonic acid, and is so thrown out into the atmosphere, and thus this singular result takes place; we may thus look upon the food as fuel. Let me take that piece of sugar, which will serve my purpose. It is a compound of carbon, hydrogen, and oxygen, similar to a candle, as containing the same elements, though not in the same proportion; the proportions being as shown in this table:—

Sugar.			
Carbon	72		
Hydrogen	11		
Oxygen	88	99	

This is, indeed, a most curious thing, which you can well remember, for the oxygen and hydrogen are in exactly the proportions which form water, so that sugar is compounded of 72 parts of carbon and 99 parts of water; and it is the carbon in the sugar that combines with the oxygen carried in by the air in the process of respiration, so making us like candles; producing these actions, warmth, and far more wonderful results besides, for the sustenance of the system, by a most beautiful and simple process. To make this still more striking, I will take a little sugar, or to make the experiment shorter I will use some syrup, which contains about three-fourths of sugar and a little water. If I put a little oil of vitriol on it it takes away the water, and leaves the carbon in a black mass. [The lecturer mixed the two together.] You see how the carbon is coming out, and before long we shall have a solid mass of charcoal, all of which has come out of sugar. Sugar, as you know, is food, and here we have absolutely a solid lump of carbon where you would not have expected it. And if I make arrangements so as to oxidise the carbon of sugar, we shall have a much more striking result. Here is sugar, and I have here an oxidiser—a quicker one than the atmosphere; and so we shall oxidise this fuel by a process different from respiration in its form, though not different in its kind. It is the combustion of the carbon by the contact of oxygen which the body has supplied to it.

If I set this into action at once you will see combustion produced. Just what takes place in my lungs—taking in oxygen from another source, namely, the atmosphere, takes place here by a more rapid process.

You will be astonished when I tell you what this curious play of carbon amounts to. A candle will burn some four or five, or six, or seven hours. What then must be the daily amount of carbon going up into the air in the way of carbonic acid! What a quantity of carbon must go from each of us in respiration! What a wonderful change of carbon must take place under these circumstances of combustion or respiration! A man in twenty-four hours converts as much as seven ounces of carbon into carbonic acid; a milch cow will convert seventy ounces, and a horse seventy-nine ounces, solely by the act of respiration. That is, the horse in twenty-four hours burns seventy-nine ounces of charcoal, or carbon, in his organs of respiration to supply his natural warmth in that time. All the warm-blooded animals get their warmth in this way, by the conversion of carbon, not in a free state, but in a state of combination. And what an extraordinary notion this gives us of the alterations going on in our atmosphere. As much as 5,000,000 pounds, or 548 tons, of carbonic acid is formed by respiration in London alone in twenty-four hours. And where does all this go? Up into the air. If the carbon had been like the lead which I showed you, or the iron which, in burning, produces a solid substance, what would happen? Combustion could not go on. As charcoal burns it becomes a vapour and passes off into the atmosphere, which is the great vehicle, the great carrier for conveying it away to other places. Then what becomes of it? Wonderful is it to find that the change produced by respiration, which seems so injurious to us (for we cannot breathe the air twice over) is the very life and support of plants and vegetables that grow upon the surface of the earth. It is the same also under the surface, in the great bodies of water, for fishes and other animals respire upon the same principle, though not exactly by contact with the open air. Such fish as I have here [pointing to a globe of gold-fish] respire by the oxygen in the air, which is dissolved by the water, and form carbonic acid, and they all move about to produce the one great work of making the animal and vegetable kingdoms subservient to each other. And all the plants growing upon the surface of the earth, like that which I have brought here to serve as an illustration, absorb carbon: these leaves are taking up their carbon from the atmosphere to which we have given it in the form of carbonic acid, and they are growing and prospering. Give them a pure air like ours, and they could not live in it; give them carbon with other matters, and they live and rejoice. This piece of wood gets all its carbon, as the trees and plants get theirs, from the atmosphere, which, as we have seen, carries away what is bad for us and at the same time good for them,—what is disease to the one being health to the other. So are we made dependent not merely upon our fellow-creatures but upon our fellow-existents, all Nature being tied together by the laws that make one part conduce to the good of another.

There is another little point which I must mention before we draw to a close—a point which concerns the whole of these operations, and most curious and beautiful it is to see it clustering upon and associated with the bodies that concern us—oxygen, hydrogen, and carbon, in different states of their existence. I showed you just now some powdered lead, which I set burning; and you saw that the moment the fuel was brought to the air it acted, even before it got out of the bottle,—the moment the air crept in it acted. Now, there is a case of chemical affinity by which all our operations proceed. When we breathe the same operation is going on within us. When we burn a candle the attraction of the different parts one to the other is going on. Here it is going on in this case of the lead, and it is a beautiful instance of chemical affinity. If the products of combustion rose off from the surface, the lead would take fire, and go on burning to the end; but, you remember, that we have this difference between charcoal and lead—that, while the lead can start into action at once if there be access of air to it, the carbon will remain days, weeks, months, or years. The manuscripts of Herculaneum were written with carbonaceous ink, and there they have been for 1800 years or more, not having been at all changed by the atmosphere, though coming under various circumstances in contact with it. Now, what is the circumstance which makes the lead and carbon differ in this respect? It is a striking thing to see that the matter which is appointed to serve the purpose of fuel *waits* in its action; it does not start off

burning, like the lead and many other things that I could show you, but which I have not encumbered the table with; but it waits for action. This waiting is a curious and wonderful thing. Candles—those Japanese candles, for instance—do not start into action at once like the lead or iron (for iron finely divided does the same thing as lead), but there they wait for years, perhaps for ages, without undergoing any alteration. I have here a supply of coal-gas. The jet is giving forth the gas, but you see it does not take fire—it comes out into the air, but it waits till it is hot enough before it burns. If I make it hot enough it takes fire. If I blow it out the gas that is issuing forth waits till the light is applied to it again: It is curious to see how different substances wait—how some will wait till the temperature is raised a little, and others till it is raised a good deal. I have here a little gunpowder and some gun-cotton; even these things differ in the conditions under which they will burn. The gunpowder is composed of carbon and other substances, making it highly combustible; and the gun-cotton is another combustible preparation. They are both waiting, but they will start into activity at different degrees of heat, or under different conditions. By applying a heated wire to them we shall see which will start first [touching the gun-cotton with the hot iron]. You see the gun-cotton has gone off, but not even the hottest part of the wire is now hot enough to fire the gunpowder. How beautifully that shows you the difference in the degree in which bodies act in this way. In the one case the substance will wait any time until the associated bodies are made active by heat; but, in the other, as in the process of respiration, it waits no time. In the lungs, as soon as the air enters, it unites with the carbon, even in the lowest temperature which the body can bear short of being frozen, the action begins at once, producing the carbonic acid of respiration; and so all things go on fitly and properly. Thus you see the analogy between respiration and combustion is rendered still more beautiful and striking. Indeed, all I can say to you at the end of these Lectures (for we must come to an end at one time or other) is to express a wish that you may, in your generation, be fit to compare to a candle; that you may, like it, shine as lights to those about you; that, in all your actions, you may justify the beauty of the taper by making your deeds honourable and effectual in the discharge of your duty to your fellow-men.

SOCIETY OF ARTS.¹

Wednesday, January 30, 1861.

W. H. BODKIN, Esq., Assistant Judge for Middlesex,
Vice-President, in the Chair.

MR. WENTWORTH LASCELLES SCOTT read the following paper "*On Food; its Adulterations, and the Methods of Detecting them.*"

It is with no little diffidence that I have undertaken to address you this evening upon the subject of our food and drink—its various impurities and sophistications; a subject of such ample scope and grave importance, that I feel quite incapable of doing justice to it in the brief hour allotted for the reading of this paper. Nevertheless, important nationally and individually as the subject is, it has not hitherto received a commensurate amount of attention from scientific men, the commercial world, or the general public; indeed, at the present moment, the latter are accused of apathy and indifference in the matter—but the charge is a false one, as I shall presently endeavour to show.

The practice of adulterating articles of food and drink as well as those myriad non-alimentary substances so necessary to our comfort in various ways, is by no means of recent origin, as, according to Pliny, the wines of his period were largely adulterated; lead, either in the metallic form, or in that of acetate, being added, to work off the sourness of new or inferior varieties; while, if I am not mistaken, our Saxon Harold greatly distrusted the confectionary department (as well as the holy relics) of Duke William's palace, when on his memorable and

¹ From the *Journal of the Society of Arts*.

decidedly uncomfortable visit to that crafty potentate. A history of the "rise and progress" of adulteration, however, although it might be interesting to some, is quite removed from my present purpose, which has to deal exclusively with the evil propensities of our own time and our native country.

Adulteration, in the more comprehensive sense of the word, may be stated to signify, 1st, The dilution of any articles for sale as genuine with other substances of inferior commercial value or less marked properties, as instanced in the addition of water to wine or spirits, or of chicory, &c., to coffee. 2ndly, The heightening or simulation of their active principles, by the admixture of various matters foreign to their legitimate composition, of which examples may be found in the falsification of bread with alum, gin with capsicums, &c. 3rdly, The employment of various colours, essences, &c., for the purpose of rendering the articles more attractive to the eye, or to the senses of taste and smell. In this division may be placed bottled fruits and pickles, rendered green by cupreous salts; various syrups and light beverages, coloured and flavoured to a dangerous degree, and in fact, confectionary *in toto*. Lastly, any combination may be made of these several kinds of adulteration in the fabrication or getting up for the market, of any of those cheap food-preparations, so terribly abundant in poorer neighbourhoods, and but too frequently met with in fashionable localities.

Adulteration, however, is not quite confined to the types I have just indicated, for if a manufacturer of any one of the almost countless "foods, drinks, condiments, or narcotics," with which we are acquainted has an accident in the process of that manufacture, or if his consignment of a certain ingredient is of a very inferior quality, does he put aside for other purposes the deteriorated article, or explain its deficiencies, and sell it at a lower price? No; in the vast majority of instances he proceeds in the manufacture of that article as usual, adding thereto various colouring or flavouring adulterations (which may be poisonous, injurious, or harmless, as chance disposes), to simulate the physical qualities it ought to possess, or conceal their absence by heightening the tint, pungency, &c.

Again, I ask, in how many cases will the retail tradesman confess that an article which has been exposed in his shop window for a month or two, to the influences of sun, dust, and atmosphere, is injured in appearance and properties? I have here an illustrative specimen—some "crystallised" sugar, containing about 3 or 4 per cent. of dust, sold to me as the "same quality" as that which attracted admiring observers to the window, by its superior lustre and whiteness. Clearly this is adulteration, should be recognised as such, and should be punished as such. But perhaps the most heartless and pernicious species of adulteration, is that shown when persons make it their rule to manufacture nothing else but inferior, adulterated, and deleterious articles, buying up all sorts of injured produce, and refuse matter, for the compounding of their "pure and nutritious" or "health-giving" foods, as their advertisements would say;—relying upon an ambiguous but high-flown testimonial from "the late Dr. Blank," or some other celebrity of the same moral calibre, and also upon the tendency of a too-confiding public to purchase "cheap" things, to get rid of their vile, perhaps poisonous, mixture at an immense profit.

It must be readily seen that an intelligible and practical classification of this complex subject is somewhat difficult. I have attempted, however, to embody in the form of tables a kind of general outline of adulteration and its effects, which, if in the hands of the people at large, might possibly guard them from some few impositions.

Having now made up our minds that the cause of adulteration is simply that fraudulent tradesmen wish to acquire, in an indolent manner, more money than the honest man can obtain by years of toil and application, we

have to consider, in the first place, by what methods we can discover and tangibly demonstrate the presence of adulterants of any kind in articles sold as genuine, and find out in what proportion they have been added. Secondly, what measures, social, scientific, or legislative, require to be taken for the suppression or diminution of this great and growing evil; and, lastly, what part in such measures this Society, known throughout the world by its beneficial influences upon all things tending to advance the knowledge or promote the welfare of our countrymen, seems to be called upon to perform.

The limits of my time to-night are, of course, far too narrow for me to enter upon a detailed account of the various analytical processes for the detection of food-adulterants known to modern chemists, or employed by myself, but, for the benefit of those persons (forming the greater portion of the public) who are unacquainted with any means of testing the purity of their food, and who are unprovided with the complicated and costly apparatus of the analyst's laboratory, I will briefly narrate what everyone might do, and, as I hold ought to do, with the various articles of his daily food and drink.

I will commence with what has been aptly and poetically denominated the "staff of life"—bread. Recent analytical experiences, however, show very forcibly that in most cases the "staff," as supplied in the form of "half-quarterns," is but a very frail reed indeed for a working-man to lean upon. Bread is adulterated with a variety of substances not legitimately entering into its composition—some harmless, some injurious to health, but none, to my knowledge, of an actively poisonous nature.² The general characters of pure bread are tolerably well-known to most people, and after Dr. Daughlish's able paper at this Society last Session, I should only be recapitulating were I to enumerate them. Of pure bread there are several specimens on the table, illustrating Dr. Daughlish's process, as carried out by Messrs. Peek and Frean, and the ordinary method, of which latter Mr. W. Salmon, of the King's Road, Chelsea, has furnished me with samples.

When bread is adulterated with other flours or starches the admixture is often somewhat difficult to detect without the aid of a good microscope, but some of the simpler and less expensive varieties of this valuable instrument will frequently answer the purpose. The presence of potatoes in bread imparts a peculiar crumbly texture, which a very casual inspection will enable us to recognise; while rice-flour causes a brittleness of structure not easy to mistake. Nitric acid affords a means of showing the presence of potato-starch when in large quantity, as it will give a coloration to wheat flour, while the other farina remains white. Maize, or Indian corn-flour, may be detected (without a microscope), if a little care is taken, by roughly estimating the amount of fatty matter, as maize contains fully four times the quantity of fat that is usually found in wheat. The bread should be carefully and completely dried, powdered, weighed, and washed with (what is sold for) pure benzole or benzine, upon a filter, then dried perfectly and weighed again. The loss upon the former weighing will give the amount of fat or oil extracted by the benzole.

Oatmeal also contains a comparatively large quantity of fat, but is seldom, if ever, used as an adulterant for bread or flour in this country. No instance has come under my own observation.

Chalk, or carbonate of lime, is shown to be present if the suspected bread effervesces strongly on the addition of an acid. If gypsum, or plaster of Paris be looked for, boil the bread in water³ for a long time, till quite broken up and partly dissolved; then, after standing, pour off;

¹ Sulphate of copper (bluestone) is used by Belgian and Austrian bakers, but not in this country.

² The water should be distilled.

wash the sediment (if any), and boil the latter in a solution of pure carbonate of soda; filter, or pour off the clear liquid, to which add some chloride of barium; if this gives a decided milkiness, and the washed sediment an effervescence with acids, the bread has been adulterated with plaster of Paris—a substance of great importance to fraudulent tradesmen, who use it extensively in the manufacture of confectionary (when it goes under the name of “daft”), mustard, and many other articles.

I now come to speak of a substance concerning which there has been a great deal of discussion in this room and elsewhere, both as to the best methods of detecting it, and also on the question of its presence being hurtful or the reverse—I refer to alum. This is about the most frequent adulterant of bread, very few samples being uncontaminated by this salt. I may here mention, as an act of simple justice, that the sample of fermented bread before you on the table, is from the shop of Mr. Salmon, who is almost the only baker in the district of Chelsea, so far as my knowledge extends, who sells perfectly pure and unadulterated bread. This may be considered a bold assertion, but it is my well-founded opinion, nevertheless.

Alum, when present in pretty considerable quantity, may be detected by soaking the bread in water for some time and adding a salt of barium to the clear extract, when a cloudiness, more or less marked, will occur, an experiment I think I may be able to show you. Another way is to char the bread, and burn it nearly to an ash, and boil the latter with diluted hydrochloric acid, to which a little nitric acid has been added; the addition of ammonia to the filtered liquid will precipitate various substances, including the alumina of the alum. This precipitate must then be boiled in potassa, when, after filtration, the liquid must be neutralised with hydrochloric acid, and the alumina may then be thrown down by ammonia.

Bone-dust is now but seldom found in bread; it may be detected very readily with the microscope. The preceding observations apply of course equally well to flour which is very largely adulterated with inferior varieties of wheat and other meals in addition to alum and the adulterations before mentioned. My experiences show that on an average the bread in London is more or less adulterated to the amount of 87 per cent.—by which I mean to say that if I were to buy 100 loaves, at as many different bakers in various parts of the metropolis, I should get about 13 of good and genuine bread. At this present time I should probably not get quite so many good loaves, as the late bad harvest has thrown large quantities of damaged corn on the market, which of course is adulterated proportionately.

The adulteration of cakes, biscuits, and similar articles are chiefly those of the several ingredients composing the same with but few special additions. Saffron, gamboge, chrome yellow, &c., are, however, frequently used in the manufacture of sponge-cakes and Bath buns, for the purpose of deluding the unfortunate school-boy whose weakness lies in that direction into the belief that no expense has been spared in the matter of eggs; gastric, and probably another kind of irritation often follows the experiment. Messrs. Huntley and Palmer have favoured me with some specimens of the various biscuits made by them, as regards which they say, “it is impossible to select the articles of our manufactures of better quality or of greater purity”—an assertion my own extended observations have fully borne out. I may also direct your attention to the pure biscuits of Messrs. Peek and Frean, to those of Messrs. Hill and Jones, and to some intended for the nursery made by Mr. Salmon. Samples of all these are on the table.

Corn is subject to a variety of diseases, which often greatly impair its quality, and even render it dangerous as food; of these perhaps the *ergot fungus* and the pepper-brand (*uredo fatida*) are the most disgusting in their effects, the former acting sometimes like an irritant poison,

and the latter is said to have occasioned skin-diseases. Corn or meal affected with the smut or pepper-brand is at once distinguished by the disagreeable odour it emits and thus may be bought up cheaply, which to my personal knowledge is often done for the purpose of making cheap highly-flavoured cakes and puddings with the flour, which is also used for adulterating mustard, pepper, ginger, and other articles having a powerful odour of their own, which conceals that emanating from the *uredo fungus*. Another variety of *uredo* (the *uredo segetum*) is known to growers by the name of dust-brand, black-burn, or smut, but though injurious and destructive to growing crops, it does not appear to deteriorate the flour to any very great extent. I have, however, noticed a deficiency of nitrogen in wheat affected by smut. Various insects, too, such as the *musca pumilionis*, *cecidomyia tritici*, &c., attack and injure corn. I may not, however, dilate upon them now, but will refer those interested in the subject to the able papers of Mr. E. Quekett and Professor Henslow. An inquiry into the causes of the cereal fungi and the means of preventing their occurrence, would be one of great scientific interest and practical value, and in my opinion would be accomplished without great difficulty, if this Society were to offer a prize for the research. While on the subject of corn, I may mention a notable instance of its adulteration, which came under my notice at Wakefield some few years back. A gentleman purchased a quantity of wheat grain at the market there, at a rather lower rate than ordinary, for the apparent quality. After-examination showed that the bulk of the sacks were filled in with shrivelled or diseased wheat and barley, the uppermost twelve or fourteen inches being grain of fair quality. Certain circumstances which I need not relate rendered redress impossible.

Barley and oatmeal are frequently adulterated with inferior samples of either, and our poor horses frequently get some five-and-twenty per cent. of brewers' or distillers' “grains” served out to them in their reputed measure of oats.⁴

Peas, beans, lentils, and other *leguminosæ*, all come in for a share of adulteration; albeit, they are used largely as adulterants themselves. I may here remark that, in the examination of all cereal and pulse flours, starches, prepared farinas, &c., I have found that the previous abstraction of the oil or fat, by means of benzole, greatly assists the investigation, especially for the microscope, where very slight differences of the form and structure of starch granules have to be observed. Generally speaking, the too liberal use of alcohol or ether, as recommended in many works, should be avoided, for those liquids will occasionally produce a slight alteration in some of the granules, by cracking or wrinkling the outer membrane, and, by making them appear more like other starches, render their true origin doubtful. I am, perhaps, a little prolix on this point, as I believe that the errors now and then said to be made by microscopists may be due to this cause. The starches afforded by various plants all differ in size, form, or character of surface when viewed under a good microscope, and may be thus distinguished from one another. Into these literally microscopic details we may not enter now, but I have arranged a series of the more important starches, which Mr. Baker's very excellent microscopes will enable you to compare. Unforeseen circumstances have prevented my appending to this paper some notes on the microscopy of starches, and a table of their micrometric measurements.

Arrowroot is a farina or starch of remarkable purity—that is, the genuine “Maranta” variety; but out of 100 samples bought promiscuously in London, about 48 only would be genuine, and all those even would not prove of first-rate quality, as many of the samples imported are

⁴ The rape cake given to horses and cows often contains a quantity of refuse mustard-seed, which always proves dangerous, and sometimes fatal, to the former animal.

contaminated with albuminous and ligneous matter, from having been carelessly prepared. The adulterants are, as might naturally be supposed, various other starches and farinas, those most in vogue being potato and wheat starches, and sago meal. It has been said, and generally with truth, that the adulteration of arrowroot is of comparatively little consequence since none of the adulterants are detrimental to health, and they all answer the intended purpose nearly as well as the genuine article; but I have had a sample of "genuine Maranta arrowroot" containing a large quantity of barley starch of a bad colour, and a notable amount of flake-white—a carbonate of lead, and a substance not calculated to preserve one's health or invigorate the system.

Maranta arrowroot gives an opaque paste when mixed with about twice its volume of strong hydrochloric acid, while potato-starch, under similar circumstances, yields a translucent jelly.

There are various other arrowroots known to trade, such as "East India arrowroot," derived from the tubers of *Curcuma angustifolia*, that from Tahiti (from the *Tacca oceanica*); "Brazilian arrowroot" (the farina of the *Manihot utilisima*), and many others. They are not often to be bought pure at retail shops.

I must necessarily pass over a number of mixed or "prepared" farinas with high-sounding titles, and, if we are to believe the packet-labels, superlative powers of restoring health, happiness, and peace of mind, in cases "where many eminent physicians had failed to effect a cure;" they are, without exception, frauds of the worst description, and the selling of such articles should be distinctly known to the public, as, in fact, equivalent to the grave offence of "obtaining money under false pretences." On this section of the subject, Dr. Hassall and the Editor of the *Lancet* have done good service by exposing many of these wretched impostors.

More recently, however, various manufacturers, importers, and others, have evinced a laudable desire to supply the public with pure food, and these I hold ought to receive every encouragement, as they frequently labour under very great difficulties from the opposition against which they have to contend. To cite an instance, I will point to Brown and Polson's Patent Corn-flour—an article of which I have a very high opinion, as it professes to be nothing but what it is—the fecula of maize, very carefully prepared. Specimens of this starch, or flour, are on the table, and also of the gluten and husk, which, when mixed, form an excellent occasional food for horses, cattle, &c.

Even this prepared flour, however, apparently protected by the signature and trade-mark of the manufacturers, is sometimes adulterated. I met with an instance at Islington, where, with the "trade-mark" slightly altered, some potato starch and barley flour, with but little of the maize, was sold to me for "Brown and Polson's patent farina." I am inclined to think that, in this instance, the "corn flour" was used to adulterate arrowroot, while the packets were refilled with the above mixture.

Passing from bread and farinas, we naturally come to meat, as the next staple component of our daily food. Here, again, we have ample scope for detective examination and remedial legislation, for not only is a great deal of that commonly sold not of the quality it should be, but, of the meat disposed of at the London markets, nearly ten per cent. is more or less unfit for human consumption. It is well known that butchers will adopt any means of making their meat appear better and fresher than it really is, and accordingly we not unfrequently find that washes of vinegar and water are used to heighten the colour of some meats; weak alkaline leys to improve the appearance of others; and, when decomposition or disease renders more potent agents necessary, even arsenical solutions are employed without hesitation. With game and poultry this latter practice is, indeed, quite common, and I call upon you to-night, as a small but influential portion of the

British public, to express unmistakeably your opinion of such practices, and to do all in your power to suppress them.

Vegetables and fruits here follow most appropriately—their adulterations and deteriorations must be known to you all, and also the methods of detecting the same, but I may perhaps name a few points without being accused of tediousness.

The question has often been put to me—Can fruit ever be adulterated? As often, my answer has been in the affirmative. Without going into what might be called the natural and accidental adulterations—such as blight, mildew, insects, &c., I may mention that I have seen English apples, of rather inferior quality than otherwise, coloured superficially in imitation of the American Newtown Pippins, and sold as such at the rate of two and three shillings per dozen! Although we may admire the artistic genius thus displayed, we must still condemn the fraud. Old and inferior oranges, well boiled, with a little saffron added to assist Nature when her shortcomings in the matter of external colouring are a little too obvious, may be purchased at many shops, as we all know. Melons and cucumbers, too, when looking pale and dejected from waiting so long to be eaten, have their rusty coats furnished up with a little acetate of copper, so that a "green old age" at least is accorded to them.

The mention of verdigris reminds us to turn to pickles, sauces, condiments, and other similar contrivances for manufacturing appetites for debilitated humanity. Here the field is so wide, as regards adulterants of all kinds and species, that a hasty glance at them is all we can afford to give.

Before considering the special adulterations of pickles, it will be well to note those practised with the several articles used in their manufacture. We will commence with vinegar, which is largely sophisticated with water and crude acetic acid,⁵ sulphuric acid, or oil of vitriol, together with burnt sugar, and other materials for colouring. The strength of vinegar (i.e., the per-centage of acetic acid therein) can only be accurately estimated by employing an alkalimeter, and discovering by such means how many grains of dry carbonate of soda are required to neutralise a given measure of the vinegar. A more simple, but less exact, method is to weigh carefully a small, dry, white lump of Carrara marble, and to place the same in a known weight or measure of the vinegar⁶ until effervescence has entirely ceased. Then the marble may be rinsed, dried, and re-weighed, when a loss of five grains upon its original weight would show the presence of six grains of monohydrated acetic acid in the measure of vinegar taken for the experiment. The vinegar of commerce may be divided into two classes—fermented and distilled vinegar; the former derived from either sugar, malt, or wine, the latter obtained by the distillation of wood. According to the researches of Dr. Hassall, the per-centage of acetic acid in ordinary vinegar varies from 2.38 to 5.66 per cent., a disparity sufficiently large to show how greatly standard strengths are required resembling those employed in determining the value of spirits. Very weak vinegars should always be discarded, as they are unable to prevent the decomposition of meat and vegetables, while the extremely pungent varieties are immediately open to suspicions of cayenne pepper and sulphuric acid. The latter is very readily detected by the addition of a barium salt, which throws down a white, insoluble precipitate if the acid is present. Now, vinegar may often contain a small quantity of combined sulphuric acid, derived from the water with which it is made and diluted, and 1/1000th part of pure acid is permitted to be added by law, although there is never any real occasion for this, as, if the vinegar itself is good, it will keep for any time. On this account vinegar

⁵ Derived from the destructive distillation of wood.

⁶ After the sulphuric acid present (if any) has been separated by the addition of a little chloride of barium.

will generally give a milkiness with chloride (or nitrate) of barium, but of course not to such an extent as when oil of vitriol is distinctly added as an adulteration. I have myself found as much as 7·4 grains of sulphuric acid in 1000 of vinegar, I have also once detected oxalic, and in another sample formic acid (evidently having been purposely added),—facts which I believe have not been noticed heretofore. The onions and cauliflowers in pickles are generally slightly tinted with turmeric, but as this colouring matter is itself frequently adulterated with the poisonous chromate of lead, its employment is often fraught with danger to the consumer. On the other hand, with the cucumbers, girkins, and French beans, the custom is to render them considerably greener than they ever were by nature, by the addition of salts of copper. This may be accomplished in several ways—either by directly adding verdigris, oxide of copper, or sulphate of copper, ("bluestone") to the pickle, or by leaving the vinegar in copper pans for some time so as to dissolve a portion of the metal. In relation to pickles, I must say that consumers are open to very great blame, for the too general rule with them is to buy those pickles which are of the deepest green, for no better reason, it would appear, than that they are considered prettier. It is quite time all such weaknesses were dissipated, and I hold that he who chooses to buy girkins where he can get them greenest is of the same intellectual tint personally, and, whenever he purchases a pickle, deserves also to get into one. For various specimens, and much valuable information on this subject, I am indebted to Messrs. Crosse and Blackwell, who certainly evince a great desire to deal fairly with the public—if the public will only allow them to do so, and not persist in asking for green pickles, bright yellow mustard, and intensely red cayenne pepper; I can conscientiously affirm that these dangerous articles cannot now be purchased in Soho Square.

I have here a bottle of pure pickles, and here an adulterated sample; the difference in tint, you see, is very apparent, and if I add to both a little solution of ammonia, the presence of a large quantity of copper in the one case, and its entire absence in the other, will be speedily shown. The properties of dissolved copper, as an active poison and powerful irritant, are very generally known; it is a great pity that people will not think of them a little oftener than they do. According to my calculation, about 91 per cent. of the pickles sold in London are more or less adulterated, and in Liverpool the proportion is often greater still.

I have now to exhibit what might justly be considered a very great curiosity—one, too, which it is probable that not many of those present have ever seen before—I refer to this specimen of genuine mustard, kindly procured by a friend, specially for me. Even this, however, is not absolutely pure, for the microscope detects traces of wheat-flour in the sample—looking rather as if the mill which ground it had indulged in bad habits so long that they could never be entirely eradicated. The chief adulterants of mustard are plaster of Paris, chalk, clay, quick-lime, various flours and starches, cayenne and other peppers, annatto, turmeric, gamboge, and chromate of lead, all of which come in also for making "ground ginger." I once bought a sample of mustard so largely adulterated with *fresh* plaster of Paris, that when mixed with water it "set" into a solid cake in the course of a few minutes.

I will mix a little of my genuine sample here and also some bought in a packet, which I know to be adulterated with flour and other matters. You will observe that the addition of ammonia causes no change in the good sample, while the adulterated is altered in colour to a reddish brown, thus proving the presence of turmeric. Ginger is falsified so much in the same manner that I need not again refer to it. Ginger is seldom to be met with pure, mustard *never*. The microscope furnishes the

only reliable method of testing for flours and starches, in ground spices and condiments.

Turmeric is a usual and legitimate component of currie powder, which I am told cannot be made without it—a broad assertion I am greatly inclined to doubt. It is greatly falsified with various injurious and poisonous yellow colours, and as currie may contain almost every conceivable abomination, under cover of "peculiarity of the original recipe," I do not see how its adulteration can be prevented unless by the adoption of standard recipes. The specimen of currie powder on the table is quite free from all injurious admixture.

The adulterations of pepper present a few points of interest, so they must not pass quite unnoticed, for in addition to the usual flours, meals, &c., we find lamp-black, black-lead, coal ashes, and even road dust in some samples. In New Orleans, Philadelphia, Liverpool, and occasionally in this metropolis, a "pepper" is sold consisting of old ships or dogs' biscuits (such as those upon the table, but in a considerably worse condition); first soaked in an infusion of capsicums, then dried and ground finely with a little lime, and any other little additions in the way of colouring matter that might be required. This would be "nice and strong"—anyhow, "almost too genuine," as I have heard some people say in their innocence, when commenting upon a sample more than usually adulterated.

Mace and nutmegs are chiefly adulterated with damaged and inferior specimens, and occasionally a portion of the aromatic oil is extracted before they are sold, thus diminishing their intrinsic value. According to Chevallier,⁷ nutmegs are frequently adulterated with worm-eaten varieties, the apertures being filled up with a paste made of nutmeg powder, flour, and oil; the same composition is used to form imitation nutmegs. In the United States wooden nutmegs are now and then sold along with the purchaser.

Somewhat similar remarks will apply to cinnamon, cloves, cassia, and pimento; the first, however, is, I believe, imitated, but inferior and spent varieties of all are used as adulterants. Microscopical examination shows when the samples have been previously boiled, as the granules are then much altered in appearance and augmented in size. With ground spices, of course, adulteration flourishes vigorously to an average of about 60 per cent. in the larger towns of Britain, and a still higher figure in country villages.

For an illustrative collection of genuine spices, I am beholden to Messrs. Travers and Sons, of St. Swithin's Lane; the specimens are now before you: I have here a pure sample of cayenne pepper, and also one adulterated with, among other things, red-lead; by sprinkling some of each upon the surface of some water they are readily distinguished, as the mineral poison will sink to the bottom whenever it is present.

The preservation of food-substances is a subject of no little importance, upon which a dozen papers might be written without fear of exhausting it; I can therefore only select a point or two here and there, for discussion to-night. Preserved fruits and vegetables are greatly sophisticated, a fact the more to be regretted, because it is evident the guilty parties must be the original manufacturers—not the retail tradesmen. The observations concerning green pickles may be wholly applied to green preserves. Various colouring matters are employed in preparing the other varieties, but these are seldom of a deleterious nature, as body colours are quite inadmissible. Gamboge, however, is a favourite addition for inferior marmalades; it requires to be carefully distinguished from turmeric, which it somewhat resembles; in testing for either colouring matter in preserves, strong alcohol should be used as the solvent.

⁷ "Dictionnaire des Altérations et Falsifications des Substances Alimentaires."

Specimens of various bottled fruits, of Messrs. Crosse and Blackwell's manufacture are on the table; they are prepared by a process as simple as it is efficient; the carefully-picked fruit, with the addition of a small quantity of water, is introduced into wide-mouthed bottles, which are placed up to their necks in large cisterns, or water-baths, heated to a temperature of 200°, where they remain for about 24 hours; they are then taken out and stowed away in cellars until required. Some kinds of vegetables, such as green peas, are treated in a similar manner, cylindrical tin cases being substituted for bottles. I learn from an inspection of the works of the firm in question that no less than 9370 tins of green peas were sealed at their works in 1860, while of the fruits 51,000 dozen bottles, and of preserves, or, as they are more popularly called, jams, nearly 250 tons were prepared within the same year. I merely mention these statistics to show that if I describe any process or practice of Messrs. Crosse and Blackwell, I quote from no mean authority.

A point here occurs to me that is, I consider, too important to omit. There are a great number of varieties of tropical and other fruits which are seldom seen in this country, owing to the very great difficulty of transporting them safely; if, however, they were carefully suspended in metallic cases of peculiar construction, provided either with small tubes containing phosphorus or an alkaline solution of pyrogallie acid, or else with fragments of charcoal soaked in chloroform, I am fully convinced that the form, colour, and flavour of the banana, mango, mangosteen, alligator pear, plantain, and many other luscious and delicious fruits, might be known in this country, almost as perfectly as in their own.

Fruit and wine essences and syrups next claim our attention: in this department, a variety of sophistications are apparent, as the result of careless manipulation, the use of impure sugars and of a host of injurious colouring matters, or flavouring principles. Thus the liquid generally known as fusel-oil has a colour and flavour of a very disagreeable kind, and in many cases acts injuriously on the animal economy⁸ but treated with various chemical reagents, it at once becomes the source of several fruit ethers or flowering essences. Treated with bichromate of potassa and sulphuric acid, it yields by distilling the mixture, a volatile fluid, the *valerianate of amyle*, which is employed by confectioners as an essence of apples. The *acetate of amyle*, better known as essence of jargonelle pears, is the result of the distillation of fusel-oil with acetate of potassa and sulphuric acid. Melon-essence (*coccinate of ethyle*) is also artificially prepared from ordinary, or wine ethers; cocoa-nut-oil, the essence of quinces (*pelargonic ether*) by distilling together oil of rue and nitric acid, and that of pine-apples (*butyrate of ethyle*) by saponifying butter and distilling the resulting soap with sulphuric acid and alcohol. Benzole, too, under the influence of concentrated nitric acid, yields a very good imitation, in point of flavour, of essence of almonds, and, accordingly, is largely employed for the purpose. All these artificial essences, and a great many others, would probably effect their intended object much better than the natural ones, if due care were exercised in their preparation, which, unfortunately, is not very often the case; we are, as yet, greatly in the dark upon the subject, which presents a wide field for future investigation. Many of the compound ethers, I believe to be injurious to health, even in small quantities; fusel-oil, for instance, a very common impurity in essences, wines, and spirits, seems to have a powerfully narcotic effect upon some people, producing headache, nausea, and a tendency to vomit; while with others it is apparently harmless.

Sugar shall be our next article of food,—a rather prominent one, if we consider the quantity annually consumed

in the United Kingdom, which, in 1859, amounted to no less than 8,641,927 cwt. of the raw variety, or about 32½ lbs. per head of the population; and of refined sugar and sugar candy, 242,379 cwt., or, between 13 and 14 ounces per head.

Raw, moist, or brown sugar, as imported, is much contaminated with dust, fragments of cane, molasses, and some minute insects of the *acarus* genus,—the *acarus sacchari* of Hüssall, to whom the honour of their first discovery is due. Retail grocers, however, add to their sugars, for purposes of adulteration, inferior kinds, fine sand, sawdust, salt, water, flour, potato, and other starches. The best way of examining a sample of brown sugar is, in my opinion, to determine, in the first place, the per-centage of moisture, by carefully drying, at a temperature not exceeding 120°, a known weight of the sugar; the loss sustained will give the water contained in the same. The dried powder should then be placed upon a filter, and washed with cold distilled water until the washings are no longer perceptibly sweet, when the albuminous matters, if present, may be precipitated by boiling the solution.⁹

The insoluble matter, together with the filter, may now be dried and weighed, after which the starch granules, if any be present, can be recognised under the microscope with great facility; also *acari* and sporules of *fungi*, when they occur. Crystallised sugar is much more wholesome, and also economical variety, especially that made by the centrifugal process, as it is nearly always tolerably free from imported impurities. It is, however, constantly adulterated with the inferior kinds, so that purchasers should be particular in their selection. Loaf sugar may contain a few of the ordinary impurities, if not carefully and thoroughly refined; also traces of pipe-clay, albumen, &c. Broken lump sugar has been adulterated with fragments of white marble, but this is, of course, quite exceptional.

It is greatly to be regretted that the sale of the impure brown sugars of commerce should be permitted at all, as the amount of injury their use inflicts upon the public health can be by no means slight. I hold, therefore, that remedial measures are urgently needed here. The common practice of selecting the very coarsest and darkest coloured sugars (popularly known as *foots*) for the preparation of cakes, puddings, &c., is one which I cannot so strongly reprehend; if it be required to "make a cake look rich"—the usual pretext—why not employ a pure white sugar for the purpose of sweetening, and supply the place of the deficient dirt and colouring matter by a little wholesome burnt sugar, which will answer a great deal better. As to the finance part of the question—a delicate point with housekeepers—it will always be found cheaper to employ a fine white, or at least a light-coloured sugar, than to use a dark brown variety of a lower price.

The uses of sugar as a preservative agent are well known, and in the department of confectionary proper it holds the first place. Unluckily we insist upon having our bon-bons and conserves made, not only to please the palate, but also to attract the eye; thus has been called into existence the most universal and pernicious system of adulteration with which we are acquainted. It is easy to understand that in this department very large profits are to be made, if, as a rule, as much China clay, plaster of Paris, and flour, as possible are added to the various saccharine abominations manufactured, which are then ornamented with a variety of poisonous colours, and flavoured with some crude essence, to conceal more effectually their intrinsic inferiority. We will take the simple diluent adulterants first; the readiest methods of testing for these have already been given when speaking of the adulterations of bread, and therefore need not be repeated here. In all cases the suspected bon-bon, sugar-

⁸ The real properties of fusel-oil, or "potato-spirit" in this respect are as yet but very imperfectly known.

⁹ These may, of course, be dried, and the weight ascertained if desirable.

plum, or other article should be immersed for some time in distilled water, when the insoluble matter is then presented in a convenient form for examination. In some instances it may be found desirable to burn away all the organic matters, thus leaving the ash alone for analysis. Next, proceeding to the colouring ingredients, it should be carefully noted whether these are soluble or not in the water, alcohol, or ether; if only one tint is apparent the entire substance of the article may be taken for experiment, if several they must be carefully separated by means of a small sharp knife. I have condensed in a Table some general information relating to the chief colouring matters used for purposes of (what might be called) ornamental adulteration, amounting to upwards of sixty in number, of which twelve or thirteen are active and powerful poisons—a like number are harmless, except in special cases of disease or morbid debility—while the remainder are either more or less injurious to health, or we know too little about their effects upon the animal economy to venture a decided opinion either way. I beg here to record that my friend Dr. Benjamin W. Richardson has kindly brought his large experience to my assistance in this section of the subject, and that to him my best thanks are due, and are most gratefully proffered. Some pure confectionary of Messrs. Hill and Jones's manufacture is on the table before you.

It would absorb too much of our time to-night to narrate at length the method of testing for each colour, so—with some reluctance—I dismiss those points in a very few words. If a colour be soluble in water, it is far less likely to be an injurious mineral pigment than if it remain undissolved; it should be remembered, too, that alkalis will deepen into purples most of the animal and vegetable red colours, while acids will heighten them, and increase their brilliancy. Indigo may be recognised by mixing it with plaster of Paris, drying the cake thus formed, and cautiously heating the same in a glass tube until the indigo (if present) sublimes in minute crystals. Blue litmus is changed to red by addition of an acid, ammonia or any alkali restoring the original tint; by this latter addition the colour of Prussian blue is destroyed. The greens derived from copper or arsenic are readily detected by the various well-known tests for these metals, while the red and yellow lead colours are quickly identified by their deportment with sulphide of ammonium, which turns them black. Specimens of coloured confectionary are so familiar to the eye of almost every person, that I have considered it unnecessary to exhibit many to-night—shop-windows furnish examples without number daily.

Reverting for a time to the Animal Kingdom, we have there still four articles which claim our attention, viz., milk, butter, cheese, and lard. The two first are rarely to be met with pure in this metropolis, for the simple reason that they are very easily adulterated.

Water is the usual adulterant of milk, as the dairyman who has only 40 gallons of pure milk at his disposal, while he requires 50 or 60, finds the "cow with the iron tail" ever ready to aid him in making up the deficiency. I have myself found the quantity of extraneous water in London milk to vary from about 8 to 61 per cent., while out of 100 promiscuous samples, from all parts of this city, I believe the number, more or less adulterated, in one way or another, would be about 74. Large additions of water not only impart a bluish tint to milk, but also decrease its specific gravity; some yellow colouring matter is therefore added, generally annatto, turmeric, or gamboge; and the liquid may be thickened with various starches, mucilage, or, rarely, with the brains of some animal. This latter is best detected by the microscope. Starch is shown to be present if the milk, after separating the curd, give a blue colour with tincture of iodine. Improper feeding and housing of the cows, too, are fruitful sources of bad milk,

which, when derived from diseased animals, certainly is injurious. Caution must always be exercised in determining the density of milk, as cream will diminish this very powerfully. The lactoscope of M. Donné is useful in determining the richness of milk; but a simpler, and perhaps a better instrument, was described some time since in the *Dublin Medical Press*, consisting of a thin, hollow wedge of glass, graduated on one side, into which milk can be introduced, and its opacity discovered by ascertaining at what thickness of the wedge the graduations can no longer be seen through. The instrument has also been described in the *CHEMICAL NEWS*.

(To be continued.)

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Quarterly Meeting, January 22, 1861.

Dr. J. P. JOULE, President, in the Chair.

THE following gentlemen were duly elected as honorary members:—Wilhelm Haidinger, Director-General of the I. R. Geological Institute, Vienna, and Professor James Joseph Sylvester, M.A., F.R.S.

As corresponding members:—Professor George Buckland, of University College, Toronto, and Professor Joseph Henry, Secretary of the Smithsonian Institute.

As ordinary members:—William Henry Fisher, Rev. Thomas Buckley, M.A., Simon Pincoffs, Rev. G. H. Greville Anson, M.A., Professor R. B. Clifton, B.A., John Shae Perring, William Radford, Thomas Alcock, M.D., Charles O'Neill, George Parr, jun., John Curtis, James Bottomley, and Francis Preston.

The new rules were submitted by the Council.

It was moved by Mr. BINNEY, seconded by Mr. MACLURE, and resolved:—

"That the rules proposed by the Council be adopted in place of those hitherto in force."

MICROSCOPICAL SECTION.

January 21, 1861.

Letters were read by the SECRETARY from Professor Huxley and from Mr. W. K. Parker, respecting Soundings.

Mr. HEYS, of Hazel Grove, read a paper "*On the Kaloscope*," his newly-invented instrument for the use of coloured light in the examination of objects under the microscope. This the author effects by two sets, of four discs each, of differently-coloured glass, two and a-half inches in diameter, mounted on a stand twelve inches high, one set of which is placed between the light and the bull's-eye condenser, and the other between the light and the mirror, underneath the stage, each disc having an independent motion, so that the light can be transmitted through one or more of both sets at the same time, when the object appears of the colours refracted and reflected through the discs. One of the important uses of the instrument is the protection of the eye from injury occasioned by the use of common artificial light. Many objects which do not polarise, by the kaloscope are made to disclose the beauties of polarised light; for instance, the anthers of the mallow, with their pollen, when viewed by means of red light below the stage, and, at the same time, green light (the complementary colour) through the condenser, appear of a beautiful green colour on a red or crimson ground. The author observes that some objects, viewed by means of the kaloscope, appear in such relief that they might be supposed to be seen through a stereoscope; these are anthers, jointed hairs, oil-glands, and vegetable sections in general. The calyx of the moss-rose is alluded to, under ordinary illumination, as a mere entanglement of fibres with dark beads; but by this

method it is transformed into a stereoscopic branch, with glittering glands at its extremities. Sections of wood, spines of ecchini, &c., will be found as beautiful as with the polariscope; but, by another arrangement, details are brought out not observable with the latter instrument. A black surface being placed below the stage, coloured light is thrown very obliquely from the mirror, and the complementary colour through the condenser; hairs on the edges of leaves, petals, and filaments of stamens, &c., then appear, illuminated by the light of the condenser, of one colour, and fringed with the opposite colour on an intensely black ground. The author gives a list of the botanical names of objects advantageously illuminated by this method. A single coloured disc may be also used to advantage with white light from the bull's-eye lens. Details of structure are observable by means of this instrument, which, the author observed, are inconspicuous without its aid, and thinks that its efficacy in connection with such a variety of purposes cannot fail to render it of value to the scientific observer.

The reading of the paper gave much satisfaction to the members of the Section, and it was resolved to communicate the same to the Society, with a recommendation that it should be printed *in extenso* in its "Memoirs."

The SECRETARY read a paper "On Preparing Objects found in Soundings," and described Mr. Dale's process for disposing of the tallow by means of highly-rectified benzole, which is most effectual. The benzole,—called benzine by French chemists,—being recovered, to be used again as fast as required for a dozen filters, each with its specimen in process at the same time, with only trifling loss from evaporation.

This paper, and one by Mr. Dancer, on the same subject, were ordered to be printed by the Section, for circulation amongst its members.

Mr. BROTHERS presented to the Section a very old microscope, date unknown. He also exhibited the actinophris Eichornii, melicerta, sea-weed with liparia, &c.

Mr. HARDMAN, of Davy Hulme, presented three mounted specimens of the wireworm, and a number of dissecting-needles for the use of the members. He also exhibited a mounted fly, one of the *Panorpidæ*, which, he states, feeds upon leaf-rolling caterpillars. The proboscis and feet of the insect are peculiarly adapted for dragging its victims from their concealment and holding them whilst extracting their juices, the feet being provided with combs similar to those of the spider.

Mr. R. D. DABYSHIRE presented a quantity of mud, &c., from the washings of shells from the raised seabottoms at Uddevalla, in Sweden.

Mr. DANCER exhibited a new three-inch object-glass, with a large and flat field of view; also specimens of gold quartz from Wales, large curculia, and other objects.

Mr. WHALLEY exhibited some specimens of injections obtained from Germany, which were considered the best yet exhibited.

Mr. LATHAM exhibited various specimens of sand and mud from the East Indies, portions of which were distributed amongst the members.

ROYAL DUBLIN SOCIETY.

Monday, January 21, 1861.

The Rev. Professor GALBRAITH, F.S.C.D., in the Chair.

Mr. DAVID MOORE, Curator of the Dublin Botanical Gardens, read an Account of a Botanical and Horticultural Tour made by him through portions of Germany, Holland, and Belgium, in the autumn of 1860.

The Rev. Professor HAUGHTON brought under the notice of the meeting a Catalogue of a Geological and Geographical Collection of Minerals from Greenland, from Cape Farewell to Baffin's Bay, written by Sir Charles Giesecke,

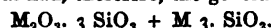
formerly Professor of Mineralogy to the Royal Dublin Society. The value of this topographical catalogue can hardly be over-estimated. It contains a detailed account of 356 minerals from 219 localities. Mr. Haughton said that he had been unable to find a map of Greenland containing the names mentioned in this valuable catalogue, which were all in the Esquimaux language, but he hoped to be able to determine the localities through the kindness of Sir Leopold McClintock, and to publish a detailed map with the catalogue of Giesecke. The minerals of Greenland, he said, were unique, and a detailed list of them, with a map, compiled by so competent an authority as Sir Charles Giesecke, would form a most valuable addition to science. He (Mr. Haughton) regretted that the Royal Dublin Society had not performed this duty during the lifetime of their distinguished Professor of Mineralogy.

The Rev. Professor HAUGHTON also read a paper on the Composition of a Leek-gum Feldspar from Greenland in the collection of the Society. It consisted of silica, alumina, sesquioxide of iron, lime, magnesia, soda, and potash, in the following quantities:—

Silica	64.40
Alumina	18.96
Sesquioxide of iron	1.04
Lime	0.45
Magnesia	0.14
Soda	2.35
Potash	13.07

100.41

This mineral had, therefore, the general formula,—



CORRESPONDENCE.

Effect of Oxalate of Ammonia upon the Precipitation of Prussian Blue.

To the Editor of the CHEMICAL NEWS.

SIR,—On repeating the experiment of Professor Arnand on the influence of oxalate of ammonia in modifying the action of prussiate of potash on a salt of peroxide of iron, by which the precipitation of Prussian blue is prevented, I found that not only was this the case, but also that if the Prussian blue were first precipitated, the addition of oxalate of ammonia caused it to re-dissolve. I therefore prepared a quantity of Prussian blue, which was well washed by decantation, and afterwards on a filter. A saturated solution of oxalate of ammonia dissolved a considerable quantity of it by the aid of heat, and yielded a clear greenish-blue solution. On being evaporated and set aside, numerous crystals of oxalate of ammonia were formed, but at the same time, at the edges of the solution, were minute dark blue crystals, of which a larger quantity was obtained by evaporating the mother liquor. They were quite soluble in water, and the addition of a drop of acid caused a copious precipitate of Prussian blue. The quantity I obtained altogether, however, was not large enough to enable me to experiment further upon it, but I am preparing the solution on a larger scale for that purpose.—I am, &c.

A. DUNCAN, Jun.

Dalmarnock Print Works, Bridgeton, Glasgow.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Crystalline Form of Chloride of Potassium.—Mr. J. M. Maisch (*American Journal of Pharmacy*, vol. xxxii, p. 521) evaporated a perfectly neutral solution of chloride of potassium under the boiling point, and found that it crystallised in perfect cubes. As soon as

these began to grow together he decanted the mother liquor and continued the evaporation at a summer heat. He now obtained four-sided prisms. The liquid drained from these was set aside, and the salt was then found to crystallise in irregular pyramids, composed of numerous small crystals. This experiment was repeated several times with precisely the same results, and in no case did the author obtain the octohedrons said to be deposited from alkaline solutions. He thinks, therefore that chloride of potassium must be regarded as dimorphous in two different systems.

Paramorphism of the Chlorides of Potassium and Ammonium.—A solution of chloride of ammonium, says the same author (*Ibid.* p. 522), when evaporated, yields feathery crystals which on close examination prove to be composed of small cubes. When the salt is sublimed, a fibrous mass is obtained which has a crystalline structure, though perfect crystals cannot be separated. The author thinks that the fibres must be considered as four-sided prisms. The prismatic chloride of potassium behaves in a somewhat similar manner. When heated, the prisms split chiefly lengthways, just as the fibrous crystals of chloride of ammonium do under the pestle. Both chlorides may, therefore, according to Mr. Maisch, be regarded as dimorphous, each crystallising in the regular and rectangular system, and isomorphous in both forms.

Crystallised Phosphates and Arseniates.—M. Debray (*Comptes-Rendus*, t. lii. p. 44) has continued the investigations, some account of which our readers will find in the *CHEMICAL NEWS*, Vol. i. p. 203. In addition to the facts there mentioned, the author now mentions that water exerts a decomposing action on phosphates which tend to change them into more basic compounds. Thus the phosphates of manganese $2\text{MnO} \cdot \text{PO}_5 \cdot 7\text{HO}$ obtained at 70°C . when heated in water to 100° splits up into a phosphate with 3 equivalents of manganese and into a soluble phosphate $\text{MnO} \cdot \text{PO}_5 \cdot 2\text{HO}$. Sometimes water removes phosphoric acid only from the salt. This happens with the phosphate of copper $3\text{CuO} \cdot \text{PO}_5 \cdot 3\text{HO}$ and the arseniate of copper $3\text{CuO} \cdot \text{AsO}_5 \cdot 3\text{HO}$ which at a sufficiently high temperature yield crystallised Libethenite $4\text{CuO} \cdot \text{PO}_5 \cdot \text{HO}$ and Olivenite $3\text{CuO} \cdot \text{AsO}_5 \cdot \text{HO}$, the liquid in which they have crystallised becoming acid. Solutions of salts of copper assist these transformations. The phosphate of lime $2\text{CaO} \cdot \text{PO}_5 \cdot \text{HO}$, the corresponding arseniate of lime and phosphate of lead $2\text{PbO} \cdot \text{PO}_5 \cdot \text{HO}$ are not altered by water alone at any temperature; but in the presence of chloride of the same base, and at a temperature of 250°C . they are changed into chlorophosphates. In this way the author says he has obtained apatite and a corresponding arsenic and lead compound. Chlorophosphates cannot be procured with the phosphates of manganese and magnesia. In the original memoir the author describes a large number of phosphates, a list of which will be found in the place we have indicated.

II. ORGANIC CHEMISTRY.

Saccharine Matter in Acid Fruits.—At first, says M. Buignet (*Comptes-Rendus*, t. li. p. 894), this is cane-sugar, $\text{C}_{12}\text{H}_{22}\text{O}_{11}$. As the fruit ripens it changes into the "inverted" sugar, $\text{C}_{12}\text{H}_{22}\text{O}_{12}$. When quite ripe, some fruits (grapes, currants, and figs) have nothing but the inverted sugar, while others (apricots, peaches, apples, pears, &c.) contain a mixture of the two sugars, in various proportions. It is difficult to separate the two sugars, but the process suggested by Peligot answers the best for isolating the cane-sugar. It consists in forming a saccharate of lime, which is separated by boiling, and then decomposed by carbonic acid. The author could not find starch in the green fruits, and, therefore, does not regard it as the source of the sugar. He found, however, a peculiar principle, which formed a colourless compound with iodine. It seems allied to tannin by many of its

properties, and can be separated from the juice of a green fruit by adding iodine until a precipitate is formed. This precipitate is collected and carefully washed, and then it can be shown that, under the influence of dilute acids, and at a proper temperature, sugar will be produced. In green bananas both starch and tannin are found, but as the fruit ripens the quantity of each diminishes, and when perfectly ripe not a trace of either is discoverable. The sugar found in their places is cane-sugar. A curious fact pointed out by the author is, that while cane-sugar is produced in bananas which ripen on the trees, it is the inverted sugar which is formed when they ripen after having been detached from the trees.

ANSWERS TO CORRESPONDENTS.

. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

. All Editorial Communications are to be addressed to Mr. CROOKES, and Advertisements and Business Communications to the Publishers, GRIFIN, BOWN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the *CHEMICAL NEWS*, containing a copious index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

Volume I.—Add pure hydrate of baryta to your mixture of alcohol and nitric acid and shake together; all the free nitric acid will unite with the baryta, forming a nitrate insoluble in alcohol. After standing, the clear liquid can be decanted from the insoluble nitrate and hydrate of baryta.

Notice was given of the scarcity of Nos. 2 and 3 as also of the increase in price of the back numbers as early as possible. The above numbers can still be obtained bound up in Vol. I.

J. B.—We now issue Monthly Parts bound in a wrapper. We also stitch the weekly numbers before issuing them. By these means the difficulty of the two loose leaves is obviated.

R. S.—We think that Fresenius' Quantitative and Qualitative Analysis are the best works on the subject in this language. The former work was recently reviewed in our columns.

A Young Manufacturing Chemist.—Heat about 8 grains of the finely pulverised mineral with six times its weight of a mixture of pure nitre and carbonate of soda in equal proportions. Heat in a porcelain crucible till the mass is red hot and in a state of tranquil fusion. When cool dissolve and determine the sulphuric acid in the solution by the usual means.

Wm. P.—We do not know at present, but will endeavour to give you an answer in a week or two.

P. M. H.—Received.

Books Received.—"On Food," by E. Lankester, M.D., F.R.S. London: R. Hardwicke, 192, Piccadilly. "Report on the Presence of Bisulphide of Carbon in Gas," by F. Vermann, F.C.S. London. "Proceedings of the American Pharmaceutical Association, 1860." Philadelphia: Merrihue and Thompson, Lodge Street. "How to Work with the Microscope," by Lionel S. Beale, M.B., F.R.S. London: John Churchill, New Burlington Street.

Received with enclosures.—C. Weeks—A. Gow—J. Young—W. Butler—H. M'Leod—Royal College of Chemistry—N. Butler and Co.—F. A. Abel—C. Cochran—Lack and Co.—Professor Hofmann—H. Matthews.

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THE CHEMICAL NEWS

VOL. III. No. 63.—February 16, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On Numerical Relations between Equivalents, by M. CAREY LEA.

IN papers on this subject, published in the January and May Numbers of this Journal for 1860,¹ I endeavoured to show that a large number of so-called elements could be arranged in seriated groups, the members in each series differing from each other by a common quantity, in most cases the number 44, or one approximating to it. I endeavoured to show that not only were these groups natural groups, but that the chemical properties of the members of each group corresponded in many cases with their position in it. These observations seemed to favour the view at present gradually gaining ground, that those bodies which we have as yet failed to decompose, we have not proved to be elementary.

An interesting and elaborate paper, by Gustav Tschermak, published in the "Proceedings of the Academy of Science of Vienna," and extracted, in an abridged form, in Knopp's *Centralblatt*, July 4, 1860, on the subject of the law of volumes of liquid chemical compounds, affords a support to the views above expressed, from a new source. The author therein shows that many of the substances usually classed as elements comport themselves in the physical properties exhibited by their combinations as compound bodies, and that it is possible from these physical properties to determine, hypothetically, the number of "physical" or absolute atoms which he supposes to be contained in a chemical atom of such body or pseudo-element. He endeavours to show that it is possible to calculate the specific gravity of a liquid from its atomic weight and the number of simple (chemical) atoms in its compound molecule, as data, but that the results lead to the immediate inference that each chemical atom contains, with few exceptions, several physical atoms.

For particulars of his theory I must refer to the original paper, but some of his results are as follows:—

	Physical atoms to each chemical atom. ²
O (O = 16)	2
S (S = 32)	4
F	2
Cl	4
N	2
P	4
As	5
Sb	6

¹ *American Journal of Science and Arts*, vol. xxx.

² See *CHEMICAL NEWS*, vol. i. p. 169.

³ These numbers are taken from the table of mean numbers, p. 508 of *Centralblatt*, [and are those subsequently used by the author for determining the "physical atomic weights" = $\frac{m}{n}$.

If now we arrange the first six of these substances in parallel series we shall find

	Atomic weight.	Physical atoms.
Sulphur	32	4
Oxygen	16	2
Difference	16	2
Chlorine	35.5	4
Fluorine	19	2
Difference	16.5	2
Phosphorus	31	4
Nitrogen	14	2
Difference	17	2

Thus, a common difference in each case amounting to 16-17 corresponds with a difference of two of the physical atoms into which Tschermak divides the chemical atoms.

If now we put O = 20, Cl = 201, P = 2p, the approximate difference between S and O, Cl and F, &c. (16-17) = $2\Delta'$, the difference (48) between S and Se = Δ and the difference (44-45) between the terms of the nitrogen series = Δ , we can express the whole of three important series in terms of these six quantities, so that at one and the same time both the numerical value of the atomic weights and the number of Tschermak's physical atoms shall be correctly expressed.

	Symbols.	At. weights.	Physical atoms.
A. Oxygen group:—			
Oxygen	O_2	16	2
Sulphur	$\text{O}_2\Delta'_2$	32	4
Selenium	$\text{O}_2\Delta'_3\Delta''$	80	5
Tellurium	$\text{O}_2\Delta'_3\Delta''_2$	128	6
B. Chlorine group:—			
Fluorine	F_2	19	2
Chlorine	$\text{F}_2\Delta'_2$	35.5	4
Bromine	$\text{F}_2\Delta'_3\Delta$	80	5
Iodine	$\text{F}_2\Delta'_3\Delta_2$	127	6
C. Nitrogen group:—			
Nitrogen	N_2	14	2
Phosphorus	$\text{N}_2\Delta'_2$	31	4
Arsenic	$\text{N}_2\Delta'_2\Delta$	75	5
Antimony	$\text{N}_2\Delta'_2\Delta_2$	120.3	6

In which table the number of radicals by which the chemical atom of each body is expressed corresponds with the number of Tschermak's physical atoms, while their numerical value is equal to the atomic weight of the body.

Thus, tellurium $\text{O}_2\Delta'_3\Delta''_2$ would have two each of three radicals, in all six, agreeing with the number of physical atoms assigned to it, while their value $2 \times 8 + 2 \times 8 + 2 \times 48 = 128$, at. wt. of tellurium.

These observations of Tschermak, taken in connection with the numerical relations which exist between atomic

weights, give rise to very interesting results, and if the conclusions which he arrives at from his experiments should be confirmed, they cannot fail to exercise a very important influence on the progress of chemical science.

*On the Action of Oxygen on Aniline, by
Professor C. F. SCHÖNBEIN, of Basle.*

ANILINE, for so many reasons one of the most remarkable organic compounds, possesses a special interest, on account of the peculiar relation in which it stands to the three modifications of oxygen.

Action of Negative Oxygen.—A piece of white filtering-paper, moistened with colourless aniline, when introduced into strongly ozonised air, is coloured first brownish-red, then brown-red, rapidly passing into deep brown; and I may here remark that this rapid and powerful browning of the aniline is one of the most delicate reactions of ozone, and fully equals, in this respect, pyrogallie acid and hæmatoxyline. The result of this oxidising action of $\ominus$ on aniline is a solid, deep brown substance, which is scarcely soluble in water, but is readily taken up by aniline itself, as well as by alcohol and ether, whereby the fluids are coloured deep brown.

It is scarcely necessary to observe that aniline through which a stream of ozonised air is passed becomes rapidly coloured brown, in consequence of the production of the before-mentioned resinous body. It is, however, very probable that during the action of the ozone upon the aniline many other products of oxidation result which deserve investigation. For the information of anyone who may take this subject up, I may observe, that the brown, resinous body is destroyed by the prolonged action of $\ominus$, inasmuch as a piece of paper, moistened with aniline, if left long enough in an atmosphere powerfully ozonised, will be eventually bleached again, and then contains a substance which powerfully reddens litmus-paper, and which is probably oxalic acid.

Combined $\ominus$ acts upon aniline like free $\ominus$; for we find that this substance is resinsified at ordinary temperatures by all the ozonides, just as it becomes brown by the action of free $\ominus$, the ozonides, of course, losing their $\ominus$ at the same time. This may be conveniently illustrated by placing in watch-glasses small quantities of the oxides of gold and silver, the binoxides of lead, manganese, &c., and pouring over them a few drops of aniline, which immediately becomes brown. If a solution of permanganic acid or permanganate of potash be shaken up with aniline, it gives immediately a deep brown precipitate, consisting of oxide of manganese and the brown resin, which latter may be extracted by alcohol.

NO_2 —according to my view, $\text{NO}_2 + 2\ominus$ —is remarkable among the ozonides for the energy with which it acts upon aniline, and may be compared in this particular with ozone itself. If so small a quantity of NO_2 vapour as to be scarcely perceptible either to the sight or smell is introduced into a bottle, a piece of paper moistened with aniline will be coloured precisely as if ozone itself were present: and if a quantity sufficient to slightly colour the vessel be present, the paper becomes immediately deep brown, with the production of white vapours, which surround the paper.

It is well known that $\ominus$ is able to combine as such with Guaiacum resin, and to form an organic ozonide, which is coloured deep blue, and is soluble in alcohol. Aniline greedily abstracts $\ominus$ from this as from the inorganic ozonides, so that tincture of Guaiacum, of the deepest blue, becomes instantly yellowish-brown when mixed with aniline.

Action of Positive Oxygen.—While, as I have shown, both free and combined $\ominus$ powerfully oxidises aniline, all the compounds containing $\ominus$ are quite inactive. Thus, for instance, $\text{HO} + \ominus$ may remain for days in contact with aniline without either the one becoming decomposed or the other resinsified. Neither have $\text{KO} + 2\ominus$, $\text{N}_2\text{O}_2 + \ominus$, or $\text{BaO} + \ominus$ any action upon aniline, inasmuch as they leave it quite uncoloured. The so-called ozonised oil of turpentine contains its disposable oxygen in the $\ominus$ state, and is, therefore, an organic antozonide. As is the case with $\text{HO} + \ominus$, &c., so is this turpentine oil chemically inactive towards aniline, however much it may be laden with $\ominus$. The two fluids, when mixed, remain quite colourless, and no $\ominus$ is taken up by the aniline. These facts seem to me to prove beyond doubt, that positive oxygen behaves towards aniline precisely as it does towards pyrogallie acid,—that is, $\ominus$ produces no oxidising effect upon this substance, otherwise so greedy of oxygen.

Action of Neutral Oxygen.—Neutral oxygen acts upon aniline apparently as such, as it is well known that aniline in contact with pure, or atmospheric oxygen becomes coloured and resinsified, which is, without doubt, an oxidising action. I find, however, that, *ceteris paribus*, this change occurs more rapidly under the influence of light than in darkness. 50 grammes of colourless aniline, shaken up in a pint bottle with atmospheric oxygen, appeared to be coloured a deep brown, after standing for four days in diffused light, while aniline, under otherwise perfectly similar circumstances, but kept in the dark, appeared to be very little changed.

According to my observations, the resinsification of aniline and the charging of oil of turpentine with $\ominus$, takes place in the presence of light very much faster than in the dark; in fact, illuminated oxygen generally acts as $\ominus$, and produces oxidations which, in darkness, either do not take place at all, or, at most, very slowly. I am, therefore, inclined to assume that oxygen, as such, has no oxidising influence upon aniline.

From these investigations, it is clear that facts are rapidly accumulating which tend to show that the chemical relation of oxygen to other bodies is intimately connected with its allotropic state; and I have no doubt that the number of these facts will daily increase. We must, therefore, be prepared to modify, or entirely relinquish, our previous conceptions of "chemical affinity." I often feel that it is impossible to obtain a clear notion of, for example, the words, "The affinity of oxygen for phosphorus," &c., if we wish to express anything more than the mere fact. I must confess that the prime cause of chemical combination is still to me a deep mystery.—*Journal für Prakt. Chemie*, bd. lxxxi. p. 5.

TECHNICAL CHEMISTRY.

On Coal Gas, by the Rev. W. R. BOWDITCH.

(Continued from page 84.)

Coal-gas, quite free from sulphide of hydrogen, when passed through hot lime, blackens lead-paper, showing that masked and hitherto irremovable compounds have been so altered as to be easily removable. The lime does not take up sulphide of hydrogen, but becomes gradually, yet very slowly, darkened by the deposition of tar and carbon from vapour of bisulphide of carbon. The reaction with previously dried slaked lime commences at 108°F ., and continues through the whole

range of temperature up to redness. At a red heat the sulphur of the bisulphuret of carbon and other sulphur compounds unites with the lime and forms sulphide of calcium. Practically very high temperatures are useless, as the hydrocarbons of gas begin to be decomposed about the melting-point of lead, and to deposit their carbon upon the hot lime. Fortunately injurious temperatures are not required. I have frequently freed gas from every trace of sulphur, so that upon combustion no sulphurous acid was generated, by employing lime so heated as not to deposit any carbon, and removing the sulphuretted hydrogen evolved in the hot tube by ordinary hydrate of lime.

The same gas passed through hot clay gradually darkens the clay by forming sulphide of iron, and, when the blackness has reached the end of the tube containing the clay, lead-paper is blackened by the passing gas. The clay treated with an acid evolves sulphide of hydrogen. Carbonic acid is evolved in both cases. It is thus proved that bisulphide of carbon, in the presence of hydrogen passing over hot hydrate of lime, is decomposed, and that its sulphur becomes united to hydrogen. Coal-gas always contains a considerable quantity of hydrogen, so that, if it contain vapour of bisulphide of carbon, the process I have the honour to describe will effect its removal. The same process will break up the impure naphthalin compound and convert its sulphur into sulphide of hydrogen; and the employment of clay in the ordinary purifiers, before the gas passes through the hot ones, will so arrange the elements of certain other sulphur compounds as to enable the manufacturer to remove their sulphur as sulphide of hydrogen. Sulphocyanide of ammonium is decomposed by the heated lime, and its sulphur is liberated as sulphide of hydrogen. The only requisite for complete success was that no injury should be done to the light-giving materials of gas while removing the impurities. I have passed the principal illuminating constituents of coal-gas through the hot lime and clay, and find that they are not injured. The temperature which suffices for purification is not high enough for injury. The photometer shows that coal-gas is not injured.

The quantity of tar in gas as supplied to consumers, and the evil of its presence as a source of sulphur, are not considered as, I think, they deserve to be. It is exceedingly rare to find gas free from tar, and I never yet met with tar which did not contain both nitrogen and sulphur. Part of this tar is combined with ammonia in some manner, and may be obtained by passing gas through a bottle containing pebbles moistened with hydrochloric acid. Part is united to naphthalin, as I have already mentioned; part is united to benzole vapour, and part to other hydrocarbon vapours, such as paraffin, if two instances within my own knowledge be sufficient to justify a statement in reference to gas in general. In one instance I passed gas through a metal vessel filled with a number of wire-gauze diaphragms and kept below 32° F. Some cakes of solid paraffin were found floating upon the water which had been placed in the vessel before commencing the experiment, and a mixture of tarry oils which had deposited the paraffin. In another instance, an old gas-holder was about to be replaced by a new one, and on the water of the tank in which the old gas-holder had worked, there was found upwards of a thousand gallons of a dark-coloured fluid. All but two carboys were sold to a tar distiller. These two carboys were left exposed to the air without corks for some time, and when the manager of the gas-works went to get me some of the fluid for

examination, he found that the whole contents had evaporated. I had previously, however, obtained about half-an-ounce of the mixture. It contained paraffin, naphthalin, and the oils which accompany paraffin. Nearly a fifth of its weight of solid pitch was obtained by distilling off the hydrocarbons. A quantity of sulphide of hydrogen and ammonia were evolved during the distillation, and some of the most stinking compounds I ever met with produced from coal. From these two instances it is clear that some, or perhaps all, of the volatile hydrocarbons in gas possess the power of upholding tar with them in their vapours; and it is proved that this tar is no inconsiderable source of the sulphurous acid produced by the combustion of gas as at present purified. I have obtained tar containing sulphur from every specimen of commercial benzole I have examined; and as this will evaporate at common temperatures without leaving a residue, we are justified in the presumption that tar thus united to benzole exists in gas.

The best method of showing the tar in gas is to pass it through or over well-purified coal-oil, and subsequently through a good condensing arrangement. I have known colourless coal-oil become of a dark mahogany colour, and have separated sulphuretted hydrogen, ammonia, and solid pitch by distillation.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE I.

Chemical Affinity—Mode of its Action—Distinction from Other Powers of Matter.

I cannot commence the present course of lectures without a word of apology to a considerable proportion of my hearers on account of the very elementary character which I shall find it necessary to give to the first lectures of the course. It seems to me that one of the objects of such a course as this is to afford to beginners in chemistry an opportunity of making themselves acquainted with the fundamental and elementary principles of that science, so that they may be able, without difficulty and with pleasure to themselves, to listen to the discourses upon the higher phenomena and recent discoveries of science which peculiarly characterise this Institution. I trust, therefore, I may claim the kind indulgence of a considerable proportion of my more instructed audience, whilst, in the first few lectures, I treat of those very elementary matters with which some of you are quite as familiar as myself.

The material objects by which we are surrounded are constantly undergoing changes of a more or less striking character. Thus, water assumes, when it is exposed to the cold of winter, the condition of ice, and, when submitted to a higher temperature, becomes converted into steam. Frequently the blue sky becomes suddenly darkened by masses of cloud, from which issue thunder and lightning. The farmer and the gardener cast into the earth seeds, which spring up and gradually develop themselves into the stems, branches, and leaves of plants. In the autumn these leaves fall to the ground, and there they undergo decay and gradually disappear altogether; whilst the iron nails by which some of those plants were trained to the wall have become covered with a red material which we all know as the rust of iron. Now, all these changes

are produced upon matter by certain agencies which we term *forces*. Thus, the change in the condition of water is supposed to arise from the action of two antagonistic forces, namely, *heat* and *cohesion*. The phenomena of the thunder-cloud are due to *electricity*. The springing up of seeds into the leaves and branches of plants is due to an aggregation of forces, to which the very inappropriate term of "*the vital force*" has been applied; whilst the decay of the leaves after they have fallen to the ground by the attraction of *gravity*, and the rusting of the iron nails are due to the *chemical force*. This is the force we shall have to consider, it is closely associated with the other great powers of matter; and it is not always easy to distinguish between the operations of the chemical force and those of the other powers. Both chemistry and physics—the latter being the science which treats of the remaining forces of matter,—deal with the changes which bodies undergo, with especial reference to the *causes* of those changes, and, consequently, these two branches of science are very closely associated together.

The phenomena of the chemical force are, however, of such a character that, with a little preliminary explanation, we shall be able to distinguish them from those of the other forces.

In the first instance, it is absolutely necessary for the operation of the chemical force that we should have two different kinds of matter. No two portions of the same matter are capable of causing the manifestation of this force. Iron brought into contact with iron manifests no chemical action. Sulphur brought into contact with sulphur likewise produces no chemical effect. But if we bring together iron and sulphur, these two bodies, under favourable circumstances, are capable of exerting chemical force, or "*affinity*" as we term it. Now, let us examine some of the peculiarities of this force, and especially some of the marks by which it is distinguished from the other powers of matter. In the first place, with regard to the chemical force, allow me to refer you to these memoranda [pointing to a diagram], which may be regarded as the heads of my lecture. In the first place, the chemical force always changes the properties of the bodies submitted to its influence. We have always a change of a more or less striking nature,—a permanent change, so to speak, impressed upon matter upon which this force acts. For instance, we have here the bright metal, iron, and here we have enclosed in this jar the element oxygen, one of the constituents of our atmosphere. These two substances are capable of uniting chemically, but in doing so they undergo such a complete change that it would be impossible for any one, without knowing the fact, to recognise either the one or the other in the compound—rust, or oxide of iron,—which is formed of these two elements. In the same way this bright liquid metal, mercury, unites with oxygen, and produces this beautiful solid red binocide of mercury, which is as unlike the substances of which it is composed as possible, the one being liquid, and the other an invisible gas.

This change of properties never takes place when any force other than the chemical force is exerted upon matter. If, for instance, I expose a piece of this platinum wire to the influence of heat, I get a phenomenon which is termed incandescence; the platinum wire becomes heated to redness, and may be maintained at this temperature for any length of time. At a higher temperature I could fuse it and run it into a single globule, or mass, but when cooled it possesses its original properties, quite unchanged. It possesses the same metallic properties; and, even if we were to fuse it to a globular mass, we might draw it again into wire of the same character as this. Here is another metal, not quite so brilliant on the surface, perhaps, as the platinum wire, and I will expose this to the same influence. You see this wire presents perfectly different phenomena from those we observed in the platinum wire. We have brilliant com-

bustion, and we have the wire converted into a friable material here, which it is almost impossible to collect,—into a substance which is termed magnesia; we have converted, in fact, our bright, silver-like metal into magnesia. In the latter case we have had an exercise of the chemical force, which changes the properties; in the former, only heat was exerted, which left the matter with its original properties unimpaired. Now, no mere mechanical mixture of two portions of matter can produce a body possessing properties completely different from the two substances so mixed. I have here, for instance, a mixture of sulphur and iron in a state of fine division. The mixture is very intimate indeed, although, when examined closely, we may certainly perceive the particles of the two different substances; but unless we closely examine the mixture we do not readily detect the separate constituents of which it is composed. This mixture, however, possesses properties intermediate between these two substances; or, rather, I should say, it possesses the properties of both constituents. We have still in this mixture sulphur, as such, and iron, as such; and we could manifest their properties by a little patience, and, by a magnifying glass, we could take out the particles of iron from the particles of sulphur with which they are mixed. We can do this, however, much more effectually by means of a magnet,—for instance, by means of this powerful magnet, which will, I hope, enable us to separate these two elements from each other. [The Lecturer then spread a portion of the mixture upon paper, and placed it over the poles of a powerful electro-magnet.]

We are now about to submit the mixture to the action of magnetism; and you see how the little particles of iron rush away from the sulphur, and gather over the poles of the magnet. We can thus separate the iron better than by other means. Now, such a separation as this, either by taking up the particles under the microscope, or by submitting the mixture to the action of the magnet, cannot be effected upon a true chemical compound. Here I have a compound of these same elements, *united* in the same proportion in which they were there *mixed*. Here they are chemically combined—there mechanically mixed. Now, we will see what the difference will be in the deportment of these two substances over the poles of the magnet. You see we have not the slightest tendency to separation or accumulation over the poles of the magnet. These two substances are so completely mingled that this magnetic attraction is utterly unable to separate them from each other. Further, we can very readily ascertain the difference in properties, with regard to the magnet, between the mixture and the chemical compound. Here we have two equal weights in these two scale-pans, the one on this hand [pointing to the right], consisting of the mixture, the weight in the other pan consisting of the chemical combination of the sulphur and the iron. Let us try now whether these bodies—you will recollect that in both scale-pans we have exactly the same weight of iron, and also the same weight of sulphur in both cases—let us now try whether there is any difference with regard to the magnetic attraction which this large piece of apparatus is capable of exerting upon these two portions of matter. [The Lecturer then attached the scale-pan, containing the mechanical mixture of the sulphur and iron to the end of the lever.] You see at present, before the contact of the magnet with the battery is made, this scale pan exactly balances the long lever at the other side of the point of suspension. Now we will put upon the lever a weight, to ascertain the amount of attracting force. [The contact was then established, and the scale pan was attracted down to the magnet.] There is now an amount of attraction exerted sufficient to sustain this considerable weight upon the lever. I will now introduce our mechanical mixture of the same elements in exactly the same proportions as they exist in the chemical compound. [The pan containing the mechanical mixture was

then detached, and the one containing the chemical compound was suspended from the lever.] As we have equal weights here, you see this other portion of matter is capable of exactly balancing that rod. And now when the magnetic force is applied, you see we have no attraction here; it has all disappeared, and it would be useless to suspend the weight on the other side of the point of suspension. So you see where chemical action has been exerted upon the two different bodies, the magnetic attraction has undergone a change; it is now attracted with a scarcely appreciable amount of force when the chemical union has taken place.

This change of properties manifests itself also in a variety of other ways. Sometimes it is seen in a change of state of aggregation; when for instance, one form of matter is converted into another form. Sometimes liquids are converted into solids when they are brought under the influence of this particular force. Here I have two perfectly limpid liquids, which I dare say on mixture will assume very nearly, if not quite, a solid form. [The liquids were then poured together.] You see the mass is gradually thickening. Now it has so far solidified, that I dare say we may invert the vessel without any part of the mixture falling out. [The vessel was inverted with the result anticipated.] Even under the influence of this peculiar force, gaseous matter itself, colourless, invisible gas, may be condensed into a solid form. Here are two colourless and invisible gases, which we may, in like manner, bring into contact; and we know that between these two gases, there exists a certain amount of chemical affinity, which will enable them instantly to unite, and to produce a white solid body which will gradually subside into the lower of the two vessels, constituting a layer of solid matter very much resembling snow. The two gases are, in fact, hydrochloric acid and ammonia, and produce the well-known solid chloride of ammonium.

This change of properties is also sometimes characterised by an alteration in the colour of bodies. Here, for instance, are two bodies, between which the chemical force is capable of acting. I pour them together, and you see we get, at first, a yellowish body, which afterwards changes into salmon colour, and will, I dare say, ultimately change to scarlet. [The two colourless solutions were then poured into one vessel.] These two solutions were at first colourless, and you see we have a very striking colour produced.

Change of temperature is also a very frequent—indeed, I may say, a constant—accompaniment of the action of the chemical force. There is no case of chemical action in which a change in the temperature of the particles does not appear. Generally the change is in the direction of elevation of temperature. I will endeavour to render this visible to you by a little apparatus here, which I shall, perhaps, have frequently to use in these lectures. I will, therefore, describe it now. It is the thermoelectric pile, which shows minute differences of temperature in such a manner as to make them visible to a number of persons at once. This is the instrument to which I refer—it consists of bars of antimony and bismuth; and when heat is applied to the ends of these bars it causes the needle of the galvanometer to deflect. If I breathe upon one end of this pile, or expose it to any sort of heat, I at once get a deflection of that needle; and if you notice the direction in which the deflection of that needle takes place, it will enable us to appreciate with the same instrument a difference of temperature in the opposite direction,—I mean when we have cold produced. When we have got this needle again to zero, I will place in front of this pile a mass of ice; and I think we shall find that the needle will then deviate from its original position in a direction opposite to that which it took in the first case. As the difference in the temperature, however, here is, perhaps, scarcely so great as in the former case, we shall, perhaps, not get so strong a manifestation, and it is, there-

fore, important that the needle should be brought as nearly as possible to zero before we expose this end of the pile to the ice. [A piece of ice was then applied to the end of the pile, but the deflection was not very marked.] I think we shall get our result better if we bring the ice nearer to the little bars of bismuth and antimony. [The ice was placed nearer.] You see, now, that is the opposite direction to which the needle moved before. If a heated substance is placed at this end of the pile the needle then moves in one direction. When a cold substance is placed there the needle then moves in the other direction.

Now, we will first try to get the effect of elevation of temperature by the action of this chemical force. Here I have two liquids which possess a very powerful affinity for each other—namely, concentrated sulphuric acid and water. I will mix them together, and I believe the vapour arising from the mixture, which is very considerably heated, is almost sufficient to show elevation of temperature. But we will not be satisfied with that. I will put the mixture in front of the pile, as I want the experiment for comparison with the subsequent one. Here, then, we have a source of heat, for we have really obtained an elevation of temperature, and you see the needle obeys the impulse and travels round in this direction, indicating that the mass placed in front of the pile here has a temperature higher than that of the surrounding air. Now, there are some circumstances under which the opposite effect is apparently produced. It is, I believe, only apparently so; but it appears to a very considerable extent in some cases. If I mix this powdered salt—the sulphate of soda—and hydrochloric acid, I have great chemical action exerted between these two substances; in fact, the sulphate of soda becomes to a great extent broken up in its chemical constitution, new compounds are formed, and a very marked depression of temperature takes place in the mixture. The mixture has now become very much colder,—much colder, in fact, than ice,—and the deflection of the needle towards the cold side will be considerably more powerful than it was in the case of the block of ice. Let me try it. We will place our mixture, which we expect to be reduced in temperature, in the same position as that previously occupied by the other vessel, and let us see if we get any result. We must remember, however, here, that our mixture is placed as the block of ice was at first,—at a considerable distance from the extremity of the pile. It is just possible we may have to place it nearer. [The galvanometer indicated a reduction of temperature.] No; I see there is no occasion for that; the effect is visible even at this distance. The red end of the needle is travelling towards you, indicating a depression of temperature in the mixture, and if we were to remove this and bring it nearer to the bars of bismuth and antimony, that effect would be greatly intensified; but it is visible enough now. In these cases, however, where a depression of temperature results, apparently, from the action of chemical affinity, I do not believe it is so in reality. I believe, in this case even, we have some elevation of temperature, and that in every case where the chemical action ensues we have heat evolved; but by the peculiar chemical action which ensued in this case a certain quantity of matter is liquefied—a certain quantity of solid water, for instance, in this salt becomes liquefied; the heat is rendered, as it is called, latent, causing a depression of temperature in the whole mass of matter; I believe in all these exceptional cases heat is evolved, and it is only because, under some circumstances, there is a greater amount of heat rendered latent than evolved that we have this apparent depression of temperature.

A change in the taste and odour of substances is another result of the action of the chemical force. This, however, we may pass over.

Change in the medicinal properties of bodies is also a very frequent occurrence when chemical action takes

place. For instance, chlorine is an exceedingly poisonous and suffocating gas, so that two or three cubic inches of it taken into the lungs would produce almost instant death; it is one of the most poisonous substances there is, when taken into the lungs in a concentrated form. Sodium is also a substance which would produce the most injurious effect if taken internally. But these two energetic substances produce, by combination, common salt, one of the most innocuous bodies with which we are acquainted. Arsenic, when in chemical combination with iron, forms an innocuous compound, which may be administered to animals with impunity. Also the elements of our atmosphere form, by chemical combination, a substance possessing the most corrosive qualities.

Another peculiarity of chemical affinity is its incapability of acting at a distance. Most other forces are capable of exhibiting their effects and exerting their influence through considerable portions of space. Here I have a little collodion balloon. If I just rub it with my hand I render it electrical,—that is, I excite a certain amount of electrical force. It will follow my hand when held at a short distance from it. Thus the electricity acts through space. You are warmed by standing in front of a fire, although the intermediate air may be below the freezing point. A magnet also can exert its power through air or space filled up with glass, or through other solid substances; but the chemical force is quite incapable of acting in this way. It cannot act through an appreciable space. Let us put this to as severe a test as, perhaps, we possibly could do. I have here a material which is capable of exerting a very powerful chemical action upon this liquid with which I have moistened the glass rod. I will now endeavour to bring the two substances—the liquid upon the glass rod and the powder upon the plate—as near as possible without touching. I have a drop upon the rod which might possibly fall upon the powder. [Shakes off the superfluous drop, and replaces the rod over the powder.] Now it is so near that the space between is hardly visible. I can hardly see it myself. It might remain in this position for days or years; it is only by coming into contact that the force is capable of being exerted between them, and the energy of it is at once rendered evident. [The Lecturer then allowed the moistened rod to touch the powder, which at once ignited.]

In addition to actual contact between bodies, which we saw to be necessary even in the case of very powerful affinities, there is also required a certain amount of mobility between the particles. This is another peculiarity of the chemical force,—in addition to contact it requires mobility of particles. As a necessary consequence of this, it follows that solid bodies cannot act upon each other chemically; and although there may be a few apparent exceptions to this general law, yet they are, I believe, most of them *only apparent*, and if closely looked into, will be found not to be exceptions, but to obey this law.

I have here two solid substances which possess a very powerful affinity for each other, but which do not act by being simply brought into contact,—not even by being reduced to a very impalpable powder and then thoroughly mixed together: they may be mixed ever so intimately, but still they do not unite. They are sulphur and iron. They do not unite now, and they would never unite even if kept together for any length of time; but it is possible by causing fusion, or by liquefying one of the substances to induce chemical action. I am now going to show you an experiment which is one of the apparent exceptions to this law. I have two solid bodies here, and I shall show you that they do act chemically upon each other. [The two substances were contained in a mortar, and the Lecturer exploded a portion of them by pressure with the pestle.] By means of the pressure which I apply to them I probably liquefy minute portions of the

sulphur, and the two bodies then combine with very great readiness. The two substances I have in the mortar are chlorate of potash and sulphur; and there can be no doubt that where the pressure is applied, liquefaction of the sulphur takes place, and that the sulphur then acts upon the chlorate of potash, and the heat then caused is sufficient to liquefy the particles around.

In consequence of this necessity for mobility of particles, the liquid and the gaseous conditions of matter are the most favourable for the exertion of chemical affinity; because there we have complete mobility of particles. The particles are free to move in any direction, and that intimate union which is so essential for the exertion of the chemical force can with readiness take place between them. It is not very essential that both forms of matter should be in the liquid or gaseous state; it is very frequently quite sufficient that one should be in this state. For instance, I have here dilute hydrochloric acid which can act upon marble; the one is solid, and the other liquid. You see if I drop some of this upon the marble, the marble is acted upon and carbonic acid gas is evolved. Likewise a gas may act upon a solid. You see if I introduce into this jar of chlorine some powdered antimony, it immediately takes fire when it comes in contact with the gas, thus showing that one of the bodies may be in the gaseous condition, the other remaining solid.

It is perhaps scarcely necessary for me to remark, therefore, that if a solid and a gas are capable of uniting by virtue of the mobility of the gas alone, two gaseous bodies which possess an affinity for each other can similarly be made to combine. Here we have two gases by which we may exemplify this. [The gases were contained in separate jars, one of which the Lecturer proceeded to invert over the other, bringing the mouths of the jars together.] We will bring the gases in contact. You see they immediately combine energetically, producing a dense, reddish brown vapour, which is a compound of two substances, nitrogen and oxygen. It has very generally been supposed that the gaseous condition is inimical to chemical combination; but wherever we have indubitable evidence of two substances having an affinity for each other, we find their affinities are exerted in the gaseous state. It has been said of oxygen and hydrogen, that the distance between their particles, when they are in the gaseous state, is an obstacle to the exertion of their affinities for each other; but if so, when we expand their particles still more, they ought then to exhibit a still less tendency to combine. But this is not the case. If we take, for instance, a mixture of these two gases, and plunge into it a heated substance, they will readily unite. I had intended to plunge into this mixture [exhibiting a soda-water bottle containing a mixture of hydrogen and oxygen] a heated bar of iron, but it is not ready. A flame will do, or this incandescent taper. The heat of this will expand the gases still more. [The incandescent film was introduced into the mouth of the bottle, and the gases united with a loud explosion.] Therefore it is not because the particles were in the gaseous state that they did not unite at first, before the heat was applied; because here we separate the particles still farther, and they combine more readily.

Now, whenever we have to combine two bodies only one of which possesses the necessary mobility of particles, it is very necessary that the other one should be in the most minute state of division; the finer the division, the more ready will the chemical action be. You all know carbon in its several different forms of diamond, of graphite, of coke, and of wood charcoal. Now, it is very difficult to burn a diamond in the air. If you remove the source of heat the combustion immediately ceases. In like manner the combustion of graphite is very difficult. Coke, although capable of burning, requires a powerful draught to keep up the heat. Charcoal will more readily ignite, and if we put several pieces together, it will continue to

burn until the whole of the particles are consumed; but even that will not burn so readily as tinder, which is another form of the carbon. This tinder, by which we used in former days to obtain our lights, before the invention of lucifer matches, will ignite by the smallest spark, and continue to burn until all is consumed. You see in the diamond we have a very dense form of carbon; in the graphite it is in a less dense form; in coke the carbon is still less dense; in the charcoal the particles are still further apart; and here, as tinder, we have it still more minute. But here is some charcoal which has been prepared in a peculiar manner, and which is capable of exhibiting its affinity for oxygen at the ordinary temperature, without any application of fire at all. If I shake it from this tube upon the paper it ignites upon coming in contact with the air, and continues its combustion after.

Of the two conditions in which particles of matter are mobile the liquid is undoubtedly best; and as this condition is more favourable, one or both of the two substances between which we want this chemical force to act must be usually converted into this condition. This is done in two distinct ways—by what chemists call the *humid*, or wet way, that is, by solution, and by the *hot*, or dry way, that is, by application of heat. I may give you a familiar example of the wet mode in the case of the common effervescent powder, tartaric acid and carbonate of soda. They may be kept together for any length of time in a dry state without any change; but, when dissolved in water, the chemical action takes place. Here, again, we have an instance of liquefaction produced in the dry, or hot way. Here is a powder which contains constituents possessing a powerful affinity for each other, but which affinity cannot be exerted as long as the particles remain in the solid condition. We will apply heat to it, and as soon as the liquid condition has been induced upon the matter a violent deflagration—perhaps an explosion—will take place. The liquid condition is that generally employed for the production of the chemical action between those substances which do not combine in their natural form of aggregation.

The fourth, and a far more distinguishing characteristic of this force, is that it is only exerted between fixed and invariable quantities of matter. We have it here stated in our diagram. We can cause heat to act upon any weight of matter, or upon any different forms of the same matter; but in the chemical force, only certain fixed quantities take part in the reaction. In consequence of this, it follows that every chemical compound possesses a uniform and invariable composition; that the same body always possesses the same elements, and that the same substances unite in precisely the same proportions. Thus, if you examine water,—we will suppose, water that has been condensed from the form of steam, water that has been formed by the melting of ice, water which falls in the form of rain, and, finally, water produced by the combustion of hydrogen with oxygen, you will find that all these different samples possess the same composition,—1 of hydrogen, 8 of oxygen. If you take, too, common salt, taken either from the brine springs of Germany, the mines of Poland, or the salt beds of Cheshire, it always contains about 23 parts of sodium and 35½ of chlorine in one hundred parts of common salt. This fact constitutes the first great law of chemical combination which is expressed here [pointing to the diagram], that the composition and properties of bodies are fixed and invariable.

The second law is also important. The relative quantities in which bodies unite may be expressed by proportional numbers. Those numbers which have been ascertained for nearly all the elementary bodies—for all the simple forms of matter out of which material nature is constituted so far as it has been investigated,—are all recorded upon these three tables [referring to the diagrams containing a list of the chemical elements] opposite to the names of the elements themselves; and we shall probably have these tables suspended here during

the whole course, so that I need only refer to the atomic weight—the combining proportion—of one or two of the elements in passing. These numbers mean that carbon, for instance, when it unites with another element, does so in the proportion of six parts by weight of carbon or some multiple of that weight. If it unites with oxygen, which is 8, we shall find that such a compound consists of 6 parts by weight of carbon and 8 parts by weight of oxygen, and so with the other elements.

Some elements are capable of combining in more than one proportion, and when this is the case the relation between the proportions is always in a very simple one; or, as it is expressed on the diagram,—“When one body, A, unites with another body, B, in two or more proportions, the quantities of the latter united with the same quantity of the former bear to each other a very simple ratio.” For instance, let me represent the atoms to you with these blocks. This atom of hydrogen which has the weight 1, is capable of combining with an atom of oxygen which weighs 8, forming water. It does, however, unite with an additional atom of oxygen, which always unites in the proportion of 8. The two atoms of oxygen make 16, forming, with the atom of hydrogen, binoxide of hydrogen, or oxy-water. The atom of hydrogen can also unite with three atoms of oxygen. The weight of the three atoms of oxygen would be 24. We have no intermediate links between these compounds. It must be always 1 to 8, or 1 to 16, or 1 of hydrogen to 24 parts of oxygen. We have no compounds between those numbers. You will see, however, that it is possible that two combining proportions of one element may unite with three of another; or three of one with four of another, forming proportions which are more complex, but still simple if you remember this principle.

The remaining law is a simple deduction from the first three—namely,—that the equivalent of a compound is the sum of the equivalents of its constituents. If any of these compounds is capable of combining with any other substance, it does so in the proportion of the sum of the equivalents of the elements of which it is composed.

Finally—chemical affinity affords the most perfect means of storing up force. In no other condition can force be so conserved for any length of time. What is our present source of mechanical power? Is it not a store of force residing in our coal, which is nothing more than a mass of carbon and hydrogen, which was previously combined with oxygen, until the solar heat and light of past ages effected a divorce, and rendered them magazines of force, from which we now draw our heat? Setting out with the chemical force stored up in zinc—and in the coal, too, for without that we could not obtain the zinc from the ore,—we have Grove's beautiful arrangement of the voltaic battery. We will first of all evoke a current of electricity, which is capable of passing through these conducting wires. We will pass the current through a portion of smaller wire here, so that we shall be able to get from this electricity a certain amount of heat. We can then pass it through water, and by that means decompose the water. It may afterwards pass around this magnet, and produce there a magnetic effect of very great intensity. You see now our current is passing. The chemical force which was previously stored up in the zinc is passing as a current of electricity through these wires. Here are two gases, which have been torn asunder from water—oxygen and hydrogen—by means of this force. [After a few seconds the contact of the wires with the battery failed.] I had intended, if our contact had lasted longer, to have caused this coil of platinum wire which became red-hot, to act upon the thermo-electric pile. We should then have had a portion of heat, causing again a current of electricity upon these wires, and producing there a magnetic effect upon that thermo-multiplier,—that galvanometer—and which would have been rendered visible as a mechanical effect. You see, then, when we have this force stored up

in coal, or zinc, or iron, how readily we can apply it to the production of any of these other powers which we may find it necessary to evoke.

GENERAL MONTHLY MEETING.

Monday, February 4, 1861.

W. R. GROVE, Esq., M.A., Q.C., F.R.S., Vice-President, in the Chair.

Nicholas P. Leader, Esq., was elected a Member of the Royal Institution.

Sir Henry Holland, Bart., M.D., F.R.S., was elected a Manager of the Royal Institution, in the room of Sir Charles Fellows, deceased.

The presents received since the last meeting were laid on the table, and the thanks of the members returned for the same. Twenty-two persons were proposed for election at the next meeting.

SOCIETY OF ARTS.

Wednesday, January 30, 1861.

W. H. BODKIN, Esq., Assistant Judge for Middlesex, Vice-President, in the Chair.

(Continued from page 94.)

Butter, too, is one of those articles which affords a striking proof of the extent to which adulteration is carried in this country, both the "salt" and "fresh" varieties being sophisticated very largely. According to Hassall, the amount of water in the fresh butters examined by the "Lancet Analytical Commission," varied from 4.18 to 15.43 per cent., while the quantity of salt averaged from 0.30 to 2.91 per cent. In the salt butters, the extremes of water were 8.48 and 28.60 per cent., and those of salt, 1.53 to 8.24 per cent. My own more recent results show a wider difference, and I have found as much as 49 per cent. of water in some samples of salt butter. From a little pamphlet on adulteration, by Mr. John Postgate, of Birmingham, I learn that, in 1857, butter was sold in Liverpool to the poor, which contained only 48 per cent. of that article, 24 pounds of a mucilage (probably a kind of lichen), and 28 pounds of water making up the remainder of this delightful compound. Butter is also adulterated with flour and other substances, and one instance has come under my notice in which the silicate of soda, or soluble glass, was the adulterant employed. Butter should be examined by melting a weighed quantity, in a graduated tube or jar, when the relative proportions of fatty matter and water can be roughly estimated.

Lard is adulterated in much the same manner as butter, and is itself sometimes used as an adulterant of butter.

Cheese is not generally adulterated, although instances have come to my knowledge. Some very fine samples of this article, and also of butter, have been kindly contributed by Mr. Osborne, of Ludgate Hill. I recommend these specimens to your notice.

Infused beverages must be our next consideration, commencing with tea as the most important. Here, I feel that I am treading upon rather delicate ground; after the able paper read in this room last week by Mr. Leonard Wray, it would be presumption in me to say very much upon the subject. I have to thank Messrs. Phillips for a small but interesting collection of genuine teas,—I mean genuine as imported; for of the 21 samples here exhibited, from India, China, Java, and Japan, three have been coloured at Canton. Specimens of the Assam Tea Company's importations are also exhibited here. Some of these specimens are very fine as may be readily perceived, and I am indebted to Messrs. Phillips no less for their kindly accorded information on many points than for the standard samples now presented to your notice. That tea

is very largely adulterated in the Chinese ports before shipment, and during the first preparation of the leaves also, I think the evidence is very convincing; but as to the proportional amount of adulterated tea imported into this country, we have little or no data to go upon. Taking the average of the black teas sold retail in London, I believe that out of 100 samples, about 61 would be found more or less impure or adulterated, while of green teas the percentage of adulterated samples would be approximately 78. Teas are adulterated with a variety of substances, but the list is too long to enumerate. A good way of examining teas is to sift them over a white, smooth sheet of paper, when the colours, or "facing," will be presented in a convenient form for analysis. When burnt, tea should leave a white ash—a coloured one indicating mineral adulteration. To detect the presence of other leaves in tea, hot water should be poured upon a small portion, and the unfolded leaves can then be inspected under a small lens, and compared with standard specimens of teas, and the leaves of other plants. Here again, however, the microscope presents the only accurate method of determining the true nature of broken leaves. Diagram 1 is a rough outline representation of the leaf of the ordinary variety of tea (*Thea viridis*); No. 2 being the kind grown in Assam, or, perhaps, more properly Assam. Paraguay tea is shown in the next diagram; and the leaf of coffee-plant, used in Sumatra and elsewhere as a substitute for tea, and sometimes imported into this country as an adulterant, is sketched in diagram No. 4. The leaf of the *Gaultheria procumbens*¹ is employed in North America as an infused tea; it is there called Mountain Tea. In this country I have once detected it as an adulterant. For the actual leaf specimens from which these diagrams were drawn, I am indebted to Mr. J. de C. Sowerby, of the Royal Botanic Society. Some interesting specimens of "brick-teas," "tea-lozenges," and other curiosities lent by the Council of the Royal Asiatic Society, are contained in the glass-case before you.

Coffee is not often very injuriously adulterated, but it is painful to reflect how people, especially poor people, are at once defrauded of their money and robbed of their health by the vendors of ground coffee, both in London and the provinces, for, out of every 100 samples obtained from retail dealers, I do not believe that more than eight will be genuine, unsophisticated coffee of the quality inquired for.

Coffee, in all its varieties and conditions, may be distinguished from chicory, or any other roasted roots and beans, with the aid of the microscope. The pure article should leave a white ash when burnt, and should not immediately impart a coloration to cold water, as a "mixture of chicory and coffee" will do. Chicory, in its turn, is adulterated with inferior roots, brickdust, and Venetian red.

The cocoas and chocolates of commerce are adulterated to an extent which I must simply term frightful,—at this moment I hardly know a wholesale or retail establishment selling the pure ground nuts. On this account I have not accepted specimens of cocoa from anyone,—for I need hardly tell you that the samples of food-products before you have been kindly contributed by various manufacturers and importers of whose high character and general respectability I had previously assured myself.

Time will not permit me to describe at length the adulterations of wines, spirits, and beer; their detection, too, is often so laborious a task that a non-scientific man would utterly fail in attempting it; so he had much better, before "ordering in a quantity," take Dr. Lankester's advice of last week, and hand the sample over, with a fee, to a professional chemist.

On one point relating to beer I will make a single observation, viz., that I have actually detected strychnine in two samples of bitter beer. Specimens of porter, double stout, and pale ale, from the brewery of Messrs.

¹ The plant from which "oil of winter-green" is obtained.

Gasthorpe and Co., of Westminster, are on the table; I can bear testimony to their remarkable purity.

I have thus briefly and imperfectly laid before you a few of the leading facts connected with the heartless system of adulteration practised with our daily food, and I beg to apologise for the incomplete nature of my illustrative specimens in some sections, arising from circumstances over which I had no control. The provisions of the recent Act for preventing adulteration are so well and so generally known that I need not enumerate them here. Two remarks, however, may not be out of place. The people of this country, I hold, are not indifferent to the subject, as is constantly asserted by interested parties, but do not know in what way to act, or how to obtain redress for their wrongs; deeply and cruelly do the people feel how hard it is that their daily bread, for which they all work, for which, too, let us hope they all pray, should be falsified and deteriorated with impunity, knowing too that as long as the monster Adulteration exist, so long will it bring death and disease to them and those dearest to them, and pour a golden stream into the purses of its worshippers.

Still, the difficulty may be mastered in one way, and to the possibility of that way being opened up, thousands of our countrymen are now looking forward in anxious expectation. Need I say that what science unaided cannot do, what legislature as it stands cannot attain, may be accomplished through the instrumentality of the "Society for the Encouragement of Arts, Manufactures, and Commerce."

I do not hesitate to assert, and I entreat the Council of this Society to give this suggestion of mine their best and most earnest attention, that if a Special Committee were appointed on the adulteration of food and drink, the evil might be greatly lessened in three years, and almost annihilated within six or seven.

Among the members of such a committee, trade and manufactures, commerce and law, chemistry and medicine should be well represented; and among the more important of their duties would be—

1st. The adoption of a series of standards of those goods whose legitimate composition is at present indefinite, such as vinegar, cocoa, currie-powder, &c.

2ndly. The consideration of what colouring-matters should be permitted to be used by confectioners and others, and of those which should be prohibited altogether.

3rdly. The extension and improvement of detective analysis in points where it is now deficient.

4thly. The consideration of the best steps to be taken to prevent the importation of adulterated articles. And,

5thly. To give all possible aid to William Scholefield, or any other man, who will endeavour to introduce a more stringent measure for the suppression of adulteration than is now in existence.

In conclusion, I have most gratefully to acknowledge the kind assistance afforded to me by Dr. Forbes Watson and Sir Emerson Tennent, in furnishing me with certain statistical information; by Mr. W. Neal, of the Royal Asiatic Society; and by my brother, Mr. C. A. Scott; to these, and several other gentlemen, I beg to tender my best thanks.

Mr. LEONARD WRAY said the paper they had just heard read mentioned the adulteration of tea, and thus afforded him an opportunity of referring to some assertions that he made at the last meeting on that particular point. In the paper which he had the honour of reading on that evening, he stated that seven-eighths of all the tea imported from China to the United Kingdom, of the last crop, was adulterated; and he gave, as his authority, the minutes of a meeting of tea merchants, held at Canton, on the 18th of April last. He had not, however, at that time the pamphlet containing those "minutes" but was speaking from the letter of a gentleman who had been for upwards of 20 years connected with the wholesale tea trade of London, and who, by referring to those

"minutes," led him (Mr. Wray) to believe that the statement, as to the amount of adulteration, emanated from that meeting. He had since obtained the pamphlet, and although he certainly was in error in this one particular, yet it appeared from the testimony of this gentleman, that there really was no exaggeration in the statement that seven-eighths of the last crop of tea imported into the United Kingdom was more or less adulterated. In the *Morning Star and Dial* of Monday, the 28th inst., that gentleman, Mr. William Green, of Forest Hill, wrote as follows:—

"The Society of Arts, through the assistance of Mr. Wray, has done a great public service, in making known the extent of adulteration of tea in China, when intended for the English market. A correspondent of the *Star and Dial*, of Friday, doubts the correctness of the statement that seven-eighths of the tea imported here is adulterated. That, I believe is rather below the fact; and that a gentleman of practical experience for 22 years should be startled by the announcement, surprises me. The character of the last five years' importation, however, powerfully sustains the averments in Mr. Wray's paper. The initiated, as well as the general public, will permit me to say that the adulteration—for convenience—may be described as of two classes, the positively spurious and the sophisticated. The former is known in the trade as Taysan Congou; it also represents most of the pekoes, capers, and Canton greens. In their manufacture is employed exhausted leaf; also leaves from three plants, *Gynura auriculata*, *Ardisia crispata*, and a common species of mint. These teas are mostly from Canton and its locality, and their quality for the most part is execrable. Those which I have denominated as 'sophisticated' are from Foo-choo-foo; and for purity they rank higher than the former. The adulteration has, however, gradually extended and increased for the last five years. The new crop of this class has just arrived. It is nearly all refined and re-manipulated, having had large quantities of old and common leaf incorporated, though imported and sold as new and fine tea, &c., &c."

Now, if this testimony had emanated from an inexperienced man, he (Mr. Wray) should certainly not bring it before the meeting; but Mr. Green had had very great practical experience; he was in one of the largest wholesale tea warehouses in London, and, for the last twenty years of his life, had had daily experience in the article of tea. He could have no hesitation, then, in giving him as an authority. Mr. Green had promised to attend there to-night, if possible, in order to support in person the assertions which he had not shrunk from publishing to the world through the newspapers. The main point, then, in that part of his (Mr. Wray's) paper, which treated of the enormous amount of adulteration practised by the Chinese on the teas they sold to our merchants, remained as he had stated it to be. They must all, however, be glad to know that our highest merchants in Canton had pledged themselves not to purchase any teas which they knew to be adulterated; although the British Consul, Mr. Winchester, distinctly told them that success could scarcely be hoped for so long as the foreign buyers continued, in the hope of gain, to purchase these adulterated articles of the Chinese. The object of this Society must be to elicit and to disseminate truth, so that whatever subject it had in hand might be presented to the public in the clearest possible light, and free from all suspicion of incorrectness. Let this question of tea, then, be thoroughly and impartially investigated.

Mr. W. J. BLAND wished sincerely that the same fairness which characterised this Society was displayed also by the members of the public press, who had chosen to insert the views of one side of the question, but utterly refused to publish the other side of the case. His letter to the *Star* newspaper, in reply to the strong assertions of

Mr. Green, had been denied publication in that journal. If an opportunity were afforded him, he was prepared entirely to disprove the statements which had gone before the public in the *Journal* of this Society. He would take upon himself to say, from the past history of the tea trade, that, so far from seven-eighths of the tea which came over to this country being adulterated, not one-eighth of our annual imports of that article was adulterated. As an old member of the Society, he had in former years brought forward facts upon this subject in the presence of Mr. Twining, Mr. Gibbs, and other leading members of the tea trade; and now, looking to the statements put forth in this room at the last meeting, he asked that the gentlemen who had made those assertions as to the alleged amount of adulteration of tea should bring forward evidence in support of them. If, as was alleged at the last meeting, seven-eighths of the tea now in our bonded warehouses was adulterated in the manner described, let them bring samples here, and he would undertake to prove that it was not adulterated or sophisticated. He claimed that this should be done, because the last speaker had urged upon the meeting to give an opportunity for candid investigation into the question, and, as he had previously stated, he was ready at any moment to come forward with proofs in support of what he had stated—and he would add that at no time within his own recollection were the public ever so safe upon the subject of tea as they were at the present time, for they now had teas of better character than was the case under the former régime of the East India Company. He unhesitatingly made these statements, and would be prepared to substantiate them.

Mr. Wm. HAWES confessed that he could not commence his observations upon this paper by characterising it, as it was generally in his power to do, as an able and useful one. He thought when charges of adulteration of food were dealt out in the wholesale manner in which they had been that evening, much more was required than the simple statement, before such charges would be received by an assembly such as he now saw before them. To say that 87 per cent. of the bread they ate was adulterated, was, in his opinion, a misrepresentation. To say that seven-eighths of the thousands of tons of teas in our warehouses was adulterated to the extent alleged, was in his opinion a misrepresentation. What did adulteration mean? Did it mean that seven-eighths of the article so called was not tea? or did it mean that some processes were employed by which a different character was given to it (let them call it adulteration if they would), but which did not affect either the wholesomeness or the quality of the tea? If it was intended to take refuge under that statement, he would say that the assertion that this was practised to the extent of seven-eighths of the total quantity of tea imported into this country was a misrepresentation. If seven-eighths was not tea at all, then it was but fair that the promulgators of such a statement should describe what the article really was. When it was alleged that the dealers in corn were guilty of such fraud that they put 12 inches of good corn on the top of the sacks, and 20 inches of bad corn at the bottom, it ought to be proved by bringing a sack of corn bought in Mark Lane into that room. In his opinion, the great mass of traders in this country were not so thoroughly dishonest as this paper would lead them to believe. That there were fraudulent merchants and traders they all knew; but, when they went through the whole list of articles forming the chief aliments of the population, and asserted that not one-half of the things they bought were pure—not half were the articles they were represented to be, he repeated that very much more proof was required before they would consider those charges substantiated. The fashion of dealing in these assertions was too common. They had a recent Act of Parliament to protect them against adulteration of

food. This was, in his opinion, a part of that paternal system of legislation which he thought had in this country passed away for ever; and it did virtually pass away when the House of Commons gave up the assize of bread, and testing for the purity of a variety of other articles of food. The public would generally take care of itself in such matters as these, and the less Parliament interfered with those petty details of life the more secure the public would be. What were the effects of the Excise laws, to preserve them from adulteration? None whatever. Let them legislate upon this subject as much as they would, all that legislation did was to show what the dishonest trader must avoid, and to drive him to some other means of arriving at the same end. Legislation on such a matter was all thrown away. The paper summed up the long list of grievances by telling them the means by which the evils complained of could be remedied. What were those means? The Society of Arts was to appoint a Committee from its own body, which in three years was to lessen adulteration very considerably, and in six years to destroy it altogether. The whole world had been trying in vain to effect this object; but the Society of Arts, by a Committee appointed by itself, with no public responsibility whatever, was to drive out this great bugbear of adulteration, and in six years! Was it possible that if seven-eighths of their tea was adulterated; that if twelve inches of good corn would conceal twenty inches of bad corn; that if pickles were adulterated with copper; if deleterious admixtures were compounded with our pepper and mustard; and if, indeed, every article sold by the butcher, baker, grocer, fruiterer, and greengrocer, were sophisticated or adulterated, that all this could be remedied in six years, by a Committee of this Society—a committee of traders, chemists, and dilettanti members—men appointed by this Society! He could never conceal his opinion upon such questions as those. He believed legislation upon them acted perniciously. He believed this microscopic examination of food—though very beautiful, perhaps, in itself—was of little practical service to the public, and worse than useless if it led to such statements as had been made that evening. He disagreed with the author of the paper in many of the points brought forward. He believed the results arrived at were exaggerated, and the remedies suggested were of the most futile kind.

Dr. LANKESTER confessed he had not expected to hear such a speech as that he had just listened to. He was surprised that a member of the Council should turn into ridicule the application of science to the practical arts of life; and he felt that, even supposing some indiscretions had been committed by Mr. Wentworth Scott, nothing could warrant the severe remarks which they had just heard. He wished this subject could be discussed without any unpleasant feeling, and he would endeavour to bring back the meeting to look at the scientific facts which the paper had brought before them. Were they to believe Mr. Scott's assertions as an honest man, or not? Were they to credit his statement that 60 per cent. of the bread he had tested was adulterated with alum or not? Mr. Scott was prepared to stake his reputation upon the matter, and on that ground he (Dr. Lankester) believed him. This was not the first time they had had this subject before them. They all recollected the analyses of food which were given in the *Lancet*, and yet he never heard it said that Dr. Hassall had given publicity to that which was untrue, or that he had unduly libelled the tradesmen of the country. He (Dr. Lankester) did not for a moment pretend to say that all persons who sold adulterated goods were aware of the extent to which the adulteration existed; but let them not encourage men to believe that they might put deleterious substances into food, and as long as this could not be detected, they were to go free. Mr. Scott, by the aid of chemistry, had shown the public what they were taking,

and had warned them what they ought not to take. He had shown that the public required more scientific knowledge upon these subjects. He had brought before them a series of facts which showed the necessity, not only of the community being instructed in the elementary details of science, but also that tradesmen who wished to deal in an honest article should know something of the nature and scientific character of the products which they sold, and something of the laws by which human life was regulated. If a tradesman received a chemical education, he would be able to detect many of these adulterations. There, for instance, was a bottle of pickles which was manifestly adulterated, and which stood side by side with an unadulterated article. The vendor of that article knew it was not genuine—he knew the West Indian pickle was adulterated, and anyone who ate freely of it at supper would find this out in the course of the night too. Such a thing should be put down, and if they were unable to effect this through the medium of the ordinary influences which operated upon society, they might fairly ask for the help of the Legislature in the matter. He was surprised therefore to hear a gentleman whom he knew advocated the amendment of the laws of this country, condemn legislation on this subject. With equal reason they might condemn legislation for the criminal who robbed his employer, as for those who systematically sold bad articles of food over the counter. Surely the Legislature, which was effective in the one case, would be effective also in the other case; and he contended that the Hon. Member for Birmingham, who carried that Bill through the House was right in endeavouring to set legislation in motion on such a subject. What he complained of was that, like too many Acts of Parliament, it was insufficient, and could not be practically worked. The fee that was fixed for the analysis was too small to insure a proper investigation by the chemist; it would not pay him for the time that was occupied by it. In the next place, the person who sold the goods must be proved to have a knowledge that he was selling an adulterated article, and unless they brought that guilty knowledge home to him, he could not be convicted. He hoped that further legislation would be really effective. The existing Act had, however, been the means of calling attention to the fact that there was something like legislation going on, and that there were means by which these practices could be prevented. With regard to the question of tea, it was one which could be answered by the analytical chemist alone. Let them not permit any gentleman who was only a dealer in tea, and knew nothing of chemistry, to say that tea was not adulterated; but it should be put into the hands of the botanist to say whether there were other leaves in it than those of the tea plant; and into the hands of the chemist to say whether there was anything besides the natural product. He would not take the opinion of a man who had dealt in tea for 25 years, if he knew not how to distinguish between the leaf of tea and the leaf of any other plant. Let them not, upon such evidence as that, decide that tea dealers were too honest to sell bad tea. Passing over the subjects of the paper, he would say that he thought Mr. Wentworth Scott had brought before them a number of practical hints by which persons with a limited knowledge of chemistry might assist themselves in judging whether an article was adulterated. With regard to bread Mr. Scott's process for detecting alum was, perhaps, too complicated for general application, for alum was not easily detected, and he warned persons against concluding from such evidence as Mr. Scott had brought forward as to the presence of alum in bread. He (Dr. Lankester) thought the test of the blow-pipe was more satisfactory in that case. Mr. Scott had spoken of the adulteration of meat. There was an astounding thing! In the newspapers of that day there was an account of hundreds of pounds of meat having been seized a few days ago by the sanitary

authorities of the City. There was the fact of the unwholesome meat being exposed for sale in the market. He knew how common it was to bring into the markets of London mangled pork, which, once seen and known, would always be recognised as diseased; which, when eaten by human beings, resulted in the production in the stomach or bowels of a worm, which was the pest of life ever after. Ought the public to be protected from such things as these? That pork, if not sold in joints to the public, found its way to the sausage-makers; and were they not to interfere between her Majesty's subjects and the horrible tape-worm? Mr. Scott had also referred to the adulteration of pepper and other spices. There was always more or less difficulty in detecting adulteration in powders. These ginger, pepper, and mustard powders might contain almost anything, for what the public knew, and it was not easy for them to ascertain what they really were. Upon this part of the subject he need only refer them to the great Liverpool pepper case, which occurred within the last twelve months. They saw from that how very difficult it was to detect the adulteration of those spices. It was frequently to the interest of tradesmen to adulterate even only to the extent of 5 or 10 per cent., to enable them to undersell their neighbours to that extent; so that there seemed to be good reason why the Government should be alive even on this matter. Even in cases where there was no Excise duty to be protected, there was an equivalent of value which no Government ought to overlook. Human life was money value. A slave in America would be worth from \$100 to \$200, and surely the life and labour of a human being in this country was worth that amount. Therefore, as a mere money question, it behoved the Government to see that the most stringent measures were carried out to detect the adulteration of food.

Dr. KIRKE wished to mention the fact, that whilst staying with a friend in Derby, last summer, he had the most convincing proofs afforded him that plaster of Paris was used by tons for the adulteration of mustard. It was sent in very large quantities to some of the largest mustard manufacturers in the kingdom.

Mr. WM. GREEN had attended solely for the purpose of confirming the statements he had made in print, as alluded to by Mr. Leonard Wray, upon the subject of the adulteration of tea, which had occupied a large share of his attention during the last five years. His experience in one of the largest houses in the kingdom for twenty years had been, that teas were very largely adulterated; and he was prepared to stake his reputation upon the statement he made, that of the tea which had been imported into this country, during the last three years, seven-eighths was adulterated. Startling as that statement was, he might, perhaps, add an explanation, which would, in some measure, qualify it. Mr. Wray had correctly stated, he believed, the extent to which the tea was adulterated in putting it at seven-eighths, and that only one-eighth was absolutely pure. Out of the whole quantity one-eighth might be regarded as a spurious material altogether; that left three-fourths of the stock to be more or less adulterated or sophisticated. That three-fourths might be taken to be adulterated to an extent varying from 1 to 50 per cent.; and if Mr. Bland (who might be looked upon as one of the fathers of the tea trade) could convince him that no adulteration took place, he should be happy to acknowledge himself mistaken. He felt it was impossible, whatever a man's knowledge of tea might be, to detect adulteration in it when carried to a comparatively small extent. He would say that of the entire crop of Foo-choo-foo tea which had just arrived, he believed there were but few lots that were absolutely pure. He had obtained some lots of that tea, and he found that, although it was only three months old, it had already deteriorated in commercial value to the amount of 6d. per lb. There, with regard to the black tea of Congou, the orange pekoe,

and other varieties, he believed the assertion that seven-eighths was adulterated was borne out by the facts. He believed the quantity of tea exported from this country amounted to about 10,000,000 lbs. annually, shipped principally to northern ports; and it was some consolation to know that the tea so exported consisted chiefly of the most inferior qualities. He had waited anxiously to hear some suggestion by which this confessedly great evil might be remedied or overcome; and it appeared to him that, as long as they carried on a war with China, as long as they exacted compensation for such a war, and as long as they taxed the produce of that country at home, it was in vain to expect that they could get a true and genuine article in tea. He would urge upon those who were anxious to be delivered from this evil, that they should direct their attention to the obtaining of teas direct from the grounds where they were grown.

(To be continued.)

PHARMACEUTICAL SOCIETY.

Wednesday, February 6, 1861.

T. N. R. MONSON, Esq., President, in the Chair.

Mr. D. HANBURY, jun., made a communication respecting "*Balsam of Peru*." Having read in the *American Journal of Pharmacy* that by some Bulls of the Popes Pius IV. and V. Balsam of Peru was allowed to be substituted for Mecca Balsam in the preparation of the Chrism used in several of the sacraments of the Catholic Church, Mr. Hanbury wrote to his friend Signor Vicenza Sanguinetti, a Professor of Botany at Rome, to procure for him, if possible, copies of the Bulls in question. On examination it was discovered that the Bull of Pius the Fourth did not exist in the archives of the Vatican, but that of Pius the Fifth was found, a translation of which Mr. Hanbury read to the meeting. After reciting the usual formulæ, it went on to say, that, as it had been represented that in some parts of the world the true Balsam tree, or oil to be used in preparing the Chrism, was not to be found, but that another oil of a similar character existed in the country, the Pope conceded full licence and permission to the Bishops and Archbishops sojourning in those parts to freely and lawfully use this other oil in preparing the Chrism, and declared that the same efficacy should follow. The Bull is dated the 2nd of August, 1571, the sixth year of the pontificate of Pius V. Chrism, from *χρίσμα*, oil, is defined by Dr. Hook as "oil consecrated in the Romish and Greek Churches by the Bishop, and used in baptism, confirmation, orders, and extreme unction. . . . There are two sorts. The one is a composition of oil and balsam made use of in baptism, consecration, and orders; the other is only plain oil consecrated by the Bishop, and used for catechumens and extreme unction."

The CHAIRMAN said the paper was of more historical than pharmaceutical value; but such matters as it referred to were of great interest, and the introduction of them greatly enlivened the meetings.

Mr. HANBURY said he should like to know how Mecca Balsam was obtained for use in this country. It was extremely rare. He had seen but one good sample, and that was in the Exhibition of 1851.

Mr. ALLCHIN then read a paper on the "*Purification of Gum Resins*." Mr. Allchin objects that the process given in the London Pharmacopœia for the purification of these bodies—viz., boiling them with water, straining, and then evaporating the water away—is in all cases prejudicial to the substance. In the case of those resins which contain a volatile oil, that portion of them is lost, while others, such as assafœtida and ammoniacum, are so much altered in their physical condition as to be rendered unfit for some medicinal purposes. Ammoniacum in tears, which consist of the pure juice of the plant, makes a good emulsion with water, so does the mass, which consists of

the tears probably agglutinated together by heat. But ammoniacum purified by the Pharmacopœia process cannot be used for making the *mistura ammoniaci*, as it will not form an emulsion. Assafœtida, also, when purified, becomes hard and friable, and will not suspend in barley water, as ordered for the *enema assafœtida*. The only object of the purification is to get rid of mechanical impurities; but if this cannot be effected without causing some loss or injury to the gum resins, the author thought it would be better to use them in their natural condition.

Mr. HANBURY said he also had been struck with the objectionable nature of the Pharmacopœia process, but confessed that he did not see his way out of the difficulty. With Sagapenum he had tried aqueous and spirituous extraction, and had then re-combined the two extracts; but he had not been satisfied with the result. In Germany it was customary to expose the gum resins until they became hard and brittle, and then to powder and sift in order to separate the woody particles mixed with them. The powder was then put up in paper cornucopias, which were placed in tin cases. In this way the powder kept very well, and did not agglomerate. Ammoniacum, however, could always be bought pure.

The CHAIRMAN said the best way to purify gum resins would be to treat them alternately with ether, alcohol, and water. In this way no part of the substance would be lost, but probably the cost of the process would be objected to.

Mr. HILLS and another MEMBER urged the Council of the Society to use their influence in endeavouring to procure the collection of pure articles. Senna and scammony used always to be adulterated, but were now to be had quite pure.

Mr. HANBURY said that in the case of some of the gum resins nobody knew where or by whom they were collected.

The CHAIRMAN said pure articles could generally be had if people would only pay the price for them. It was well known that pure otto of roses and pure musk were unsaleable in this country because of the cost.

Professor REDWOOD then read a paper "*On the Preparation of Liquor Potassæ*." Simple as the process for the preparation of liquor potassæ is, it admits of deviations which cause considerable variations in the product. When properly made with pure carbonate of potash and pure lime, it is a transparent, colourless solution of potash in water. If prepared with ordinary salt of tartar, however, it will contain sulphate and chlorides, which are allowed in the London Pharmacopœia, and if carelessly made it may contain undecomposed carbonate of potash, silica, alumina, oxide of iron, or lime. It was in the middle of last century that Dr. Black first discovered the true cause of the causticity of lime and alkalis. His contemporary Meyer supposed that potash deprived the lime of an acid to which he gave the name *Causticum*, or *Acidum Pingue*; but Black showed that the true reason was the loss of a gas, to which he gave the name of *fixed air*. The London Pharmacopœia of 1746, ten years before Black's discovery, gave the following directions for the preparation of what is there called *Lixivium Saponarium*:—"Take equal weights of Russia potash and quick-lime, and throw water upon them by degrees until the lime is slaked; then throw on more water, and stir all together that the salt of the ashes may be dissolved: after some time pour the liquor, filtered through paper, if needful, into another vessel. A true standard wine-pint of this liquor measured with the greatest exactness ought to weigh just sixteen ounces; if it is heavier, for every drachm that it exceeds that weight, an ounce and a-half of water in measure is to be added to each pint of the liquor; but, if it is lighter, it must be boiled until the like quantity of water is carried off, or else must be thrown upon fresh lime and ashes." Now,

as the wine pint of pure water weighs 7291 grains and sixteen ounces troy = 7680 grains, the specific gravity of the Lixivium Saponarium would be 1.053. In 1788 the name was changed to Aqua Kali Purum, and a new process was given. The present Pharmacopœia orders only half the weight of lime to the potash, and it is directed that the carbonate of potash shall be dissolved in one portion of the boiling water, the lime to be slaked and mixed with the other portion, and the two be shaken together until cold. The Edinburgh and Dublin Colleges direct that the carbonate of potash be dissolved, and boiled in the greater part of the water, and that the lime be slaked with the remainder, and added gradually during ebullition. The latter is the process recommended in most chemical books, and the advantage of the boiling is that the lime becomes more aggregated, precipitates denser, and absorbs less of the liquid. The Professor said the questions he proposed to discuss were:—1. Which of the three processes given above was the best? 2. Whether the lime should be weighed in the form of quick or slaked lime? 3. Whether the strength of the solution might not be altered with advantage? Taking the last question first, he said that the specific gravity of the solution ordered in the London Pharmacopœia was 1.063, and contained 64 grains of hydrated, or 55 of anhydrous potash in 100. The Dublin solution (sp. gr. 1.068) and the Edinburgh (1.072) are rather stronger. Neither of these three strengths is expressed in whole numbers, nor in easily divisible decimals. The most convenient would be a 10 or 5 per cent. solution, but there was an insuperable difficulty in the way of raising the strength, although none in decreasing. It had been found that to make a ley it was necessary to have ten times the weight of water to the carbonate of potash—more would take up lime, and less would not remove the carbonic acid from the carbonate of potash. The best strength then would be a five per cent. solution, which would have a specific gravity of 1.053, exactly that of the Lixivium Saponarium of 1746. This would have 5 grains of potash in 100 grain measures of solution, half-a-grain in 10, 3 grains in a drachm, and would deviate but slightly from the present strength. The next question discussed was, which was the best form in which to weigh the lime, as quick or slaked? Various impurities are found in commercial lime. Some has not been causticised, and cinders and stones are always found after slaking. The Professor recommended, therefore, that 2 pounds of compact, massive lime should be slaked with 1 pound of water, and when the aqueous vapour had escaped and the heat had subsided, it should be sifted through a sieve with about 20 apertures to the inch. The resulting powder had a definite composition. What quantity of this should be used? It was important to have more than was necessary to take the carbonic acid from the carbonate of potash, so it would be found most convenient to take equal weights of the slaked lime and the carbonate of potash. These should be mixed with cold water as directed in the Pharmacopœia of 1746, and then pure liquor potassæ would be obtained. Alumina was always found in the liquor which had been boiled. The following was the precise form recommended:—Take pure carbonate of potash, slaked lime, of each seven ounces and a-half, water half-a-gallon, mix the lime with three pints of water in a green Winchester quart bottle; dissolve the carbonate of potash in the remainder, and add it to the lime by degrees, shaking well after each addition. Afterwards shake the whole well until no carbonic acid escapes from the clear liquor on the addition of an acid. Filter rapidly through calico.

Mr. WHIPPLE said that the processes of the Pharmacopœia were intended to prepare useful medicines, and not pure chemicals. The liquor potassæ of the Pharmacopœia, was a remedy and not a test, and some of the so-called impurities, chloride of potassium for example, greatly increased its value as a medicine. [Amid some demon-

strations of impatience on the part of the audience, we understood him to express his belief that *pure* medicines had caused a vast destruction of human life.]

In reply to a question from Mr. HANBURY, Professor REDWOOD stated that non-effervescence on the addition of an acid was a sufficient indication of the absence of the carbonate. Lime-water was a more delicate test, but when it was used, it was necessary to dilute the liquor potassæ with about as much distilled water.

THE ROYAL INSTITUTE OF BRITISH ARCHITECTS.

At the meeting of the Royal Institute of British Architects on Monday evening, the adjourned discussion on the processes adopted for preserving the Houses of Parliament from decay was resumed, Mr. DIGBY WYATT in the chair.

The proceedings commenced by the reading of several communications which had been received since the previous meeting, among which was a letter from M. Szerelmey, in reply to some observations that had been made respecting the want of success of his "Zopissæ" process in the Speaker's Court. M. Szerelmey denied that the stone had exfoliated, and he maintained that the application of his process had indurated the surface, and been quite successful. Mr. Tite also read some letters he had received, and he repeated the experiment of mixing water-glass with a solution of muriate of lime, which produced a hard insoluble substance; and he observed that if it were possible to devise the means of combining that substance intimately with the surface of the stone it would be an effectual preservation.

Mr. SMITH, one of the Commissioners appointed for the purpose of selecting the stone for building the Houses of Parliament, explained the proceedings of the Commission, and made some revelations that excited much surprise. He stated that the Commissioners, of whom he was the only survivor, after numerous investigations and consultations with competent authorities, reported that the stone found at Bolsover Moor and the neighbourhood would be most suitable for the purpose. He said it was a mistake to suppose that the foundation of the Houses of Parliament had been built with stone from the quarry of Bolsover; for, though that quality of stone was preferred, it could not be quarried in blocks sufficiently large for the purpose; and he alone went down to examine in what part of the neighbourhood the same kind of stone could be procured. He found the same, or a similar, bed of magnesian limestone at Aston, and he brought a slab of it, six inches by nine, to London, where it was submitted to the other Commissioners, who decided that that stone should be used. He represented that it would be necessary that some one should be on the spot to see that the quarrymen got the stone from the bed selected, and it was arranged that he was to go down once or twice a-week to inspect the working, for which he was to have received a salary of £150 per annum. But when he required to know by whom that salary was to be paid, no one would take the responsibility of paying it; consequently the arrangement was not carried into effect, and the men were left to quarry the stone as they liked. To that circumstance Mr. Smith attributed the unsound quality of a great portion of the stone that was employed. In building the Museum of Practical Geology with the same kind of stone the workmen were properly inspected, for Sir Henry de la Beche said he was determined that the stone of that building should stand. The care bestowed in the quarrying had had that effect, for whilst the Houses of Parliament are rapidly crumbling, the Museum in Jermyn Street exhibits not the least appearance of decay.

Mr. WARRINGTON, the Operative Chemist of Apothecaries' Hall, explained the chemical action of Mr.

Ransome's silica process. It consists in applying to the stone in dry weather several washes of a weak solution of silicate of soda, commonly called water-glass, which enters the pores of the stone. A solution of chloride of calcium (muriate of lime) is afterwards applied, and these combine together in the stone and form silicate of lime, the insoluble substance produced in Mr. Tite's experiment. Mr. Warrington said that as the interior of the stone contains a quantity of moisture, an efflorescence would appear on the surface, though the particles of the stone might be indurated; and any process that stopped up the pores of the stone must produce exfoliation of the surface.

Mr. GILBERT SCOTT observed upon the generally bad building stones employed in London, and attributed it to the want of care in selecting the stone from the proper part of the quarry. Portland stone, which has been so extensively used in London, varies very much in quality; that which is the whitest, and is generally preferred from its appearance, is a very soft stone, while the upper bed is one of the best building stones that is used. Even granite, unless properly selected, may soon decay. He remarked on the various processes that had been adopted for the preservation of stone—viz., that of Mr. Dane, which consists of sulphur and boiled oil, that of M. Szerelmey, which he endeavours to keep secret, and that of Mr. Ransome. He spoke hopefully of the two latter processes, and he trusted that a committee would be appointed to consider the whole subject.

Mr. GODWIN mentioned the complete failure of Mr. Dane's process as applied to the statue in Caen stone of Mr. Coram, in the front of the Foundling Hospital.

Professor ANSTED said that the characters, not only of the same stone, but of the same bed of stone, were often greatly altered by the superincumbent strata, and by other circumstances, which rendered it impossible to determine, without examination in the quarry, whether any stone is or is not fit for building. He expressed the opinion that the preserving processes, to be effective, should be applied soon after the stone is quarried; for when it has begun to crumble it was impossible for any application to be of permanent utility.

Several other gentlemen wanted to address the meeting, therefore the discussion was again adjourned.

The CHAIRMAN announced that instead of appointing a committee, as at first proposed, it was intended to petition for the appointment of a commission, to consist of chemists, civil engineers, and architects, to consider the subject.

The meeting then adjourned.—*Mechanics' Magazine.*

CORRESPONDENCE.

The Adulteration of Food.

To the Editor of the CHEMICAL NEWS.

SIR,—The last number of your journal contains a report of a paper read before the Society of Arts, by Mr. W. S. Scott, "On Food; its Adulterations, and the Methods of Detecting them," subjects, which all must concur with Mr. Scott in regarding as of "grave importance." I cannot but think, however, that in referring to matters so deeply interesting to all classes of the community, it would have been more judicious to have prefaced, by some cautionary remarks, the publication of such extraordinary statements as those advanced by Mr. Scott, since it behoves all who really desire to see the Adulteration Act efficiently carried out, to have a care that no manifestly absurd or unauthenticated assertions are put forward.

Mr. Scott informs us that adulteration consists, (3rdly,) in "The employment of various colours, essences, &c., for the purpose of rendering the articles more attractive to the eye, or to the senses of taste and smell."

Are colours and essences of every description to be henceforth decried?

Again: "The cause of adulteration is simply that fraudulent tradesmen wish to acquire in an indolent manner more money than the honest man can obtain by years of toil and application."

In reference to bread we are informed that "in most cases, as supplied in the form of half-quarters, it is but a very frail reed indeed, for a working man to lean upon." That it is adulterated with chalk, gypsum, alum, and bone-dust. Mr. Scott expresses his "well-founded opinion" that "Mr. Salmon is almost the only baker in the district of Chelsea who sells perfectly pure and unadulterated bread!"

"That 87 per cent. of the bread in London is more or less adulterated!"

Can Mr. Scott for a moment imagine that such sweeping assertions as these, unaccompanied by a statement of the grounds upon which they have been made, will be credited? Will nearly nine-tenths of the bakers of London quietly submit to be told that they are "fraudulent tradesmen?"

Surely we ought at least to have been favoured with some particulars as to how many samples have been examined; from how many shops such samples have been procured, and what was the quantity and nature of the adulterating material employed in each case. It is also of great importance that we should be informed what was the actual process adopted by the analyst for their detection:—Was "crumbly texture" taken as evidence of the presence of potatoes, or "brittleness of structure" regarded as a proof of rice flour? and, more especially, was the presence of alum proved by the process described?

Does Mr. Scott mean to assert that water in which pure and genuine bread had been soaked would not produce a "cloudiness" on the addition of a salt of barium? or that the ash of genuine bread, after being treated as directed, would not yield a precipitate with ammonia, even if no alum had been used in its manufacture? Possibly if Mr. Scott had furnished us with the quantity of the alum found, we might be able to place more reliance on his results; but, in default of any information of the kind, the inference is unavoidable that the facts upon which he has arrived at such startling conclusions are far from trustworthy. As regards bone-dust, I am aware that it has frequently been stated to be one of the materials employed in the adulteration of bread; but I have hitherto, in spite of numerous enquiries, not succeeded in meeting with any one who had actually found it, and shall therefore be much interested in learning under what circumstances, and in how many instances, Mr. Scott succeeded in detecting it.

Passing on from bread, however, we find statements equally astonishing with those already alluded to.

Oats adulterated to the extent of "25 per cent. with brewers' or distillers' grains!" and rape cake alluded to as an article of food usually given to horses!

By what process can brewers' grains be made to bear any resemblance to oats? In what part of the country are horses fed with rape cake?

Again: "Of the meat disposed of at the London markets nearly 10 per cent. is more or less unfit for human consumption!" And "Arsenical solutions are employed without hesitation" to counteract the effects of decomposition or disease; and this practice is "quite common" with game and poultry.

Surely some proof should have been forthcoming to substantiate so serious a charge as this. How long has the discovery of such wholesale poisonous practices been made? Why were not the Inspectors of Meat immediately informed, and the offending parties brought before a magistrate? Why is the first publication of the discovery of such scandalous practices reserved for a meeting of the Society of Arts?

Mr. Scott has met with vinegar to which oxalic and formic acids have been "purposely added." Certainly a very bold assertion unless backed by the most unequivocal proofs. What possible motive could there be for the use of either of these acids?

Mr. Scott proceeds: "About 91 per cent. of the pickles sold in London are more or less adulterated," without, however, giving any statement of the figures whereby he arrives at so precise a computation.

"Mustard is never" to be met with pure, and he has purchased a sample so largely adulterated with plaster of Paris as to set when mixed with water!! Surely such a sample must have been sold to Mr. Scott as a good joke to pass upon an analytical chemist, for we can scarcely be expected to believe that any man would substitute plaster of Paris for mustard to such an extent with a view of deceiving a customer who had the use of four out of his five senses.

Mr. Scott appears to have a very different idea of the term adulteration from that generally entertained. The law already sufficiently protects us against cases of gross swindling and frauds which become manifest by mere inspection. By exercising ordinary care we can prevent ourselves from being taken in by "wooden nutmegs," by the substitution of barley for wheat, and there can be no difficulty in detecting marble in lump sugar, or brewers' grains in oats. I cannot conceive that such frauds as these fairly come within the category of adulterations in food.

We must all agree that the admixture of poisonous matter in any shape with food cannot be too strictly prohibited; such, for example, as the use of copper-salts in pickles, chromate of lead or orpiment, in buns, arsenical colouring matter in confectionery, &c. Neither should the sale of mixed articles as genuine (*e. g.*, arrowroot, if mixed with other starches) go unpunished; but we must not forget that a good cause is sometimes most injured by its would-be friends. It seems to me that Mr. Scott's suggestions as to the course that should be adopted to protect ourselves against the ill effects of an improper dietary would do away altogether with any but the best materials for food. For example, he says "it is to be regretted that the sale of impure brown sugars of commerce should be permitted at all." On the same grounds, would Mr. Scott argue that unsound wheat should not be permitted to be sold for human food; and would he banish it from our markets? Would he support the seizure of all potatoes touched with disease? Does he mean to imply that the bread upon which our agricultural population thrive—a bread in the manufacture of which other grain than wheat is frequently employed—and the wheat often of an inferior character—is a "very frail reed for a working man to lean upon?" or does he mean to assert that the bread consumed in London by our working men is very inferior to it in nutritive properties?

I cannot conclude without urging most forcibly on all your readers that the fault of adulteration does not rest solely on the tradesman or manufacturer.

So long as the suicidal cry for cheapness is everywhere heard, so long will adulteration exist, notwithstanding the most stringent legislative enactments. Let the public recognise the fact that anything below a fair remunerating price can only co-exist with sophisticated or adulterated food, and an incalculable advance will be made towards the attainment of a better state of things, than that which we must all acknowledge now exists.—I am, &c.

VERAX.

Copper in Bread and Flour.

To the Editor of the CHEMICAL NEWS.

SIR,—Dr. Hassall in his able work on Adulteration, as well as Mr. Wentworth Scott in his Lecture before the

Society of Arts (January 30, 1861), both state that copper, although said to be used in some places on the Continent, has never, to their knowledge, been found in bread or flour in this country. My experience, however, leads me to suppose its existence oftener than many persons would imagine. Having just completed a series of experiments with articles purchased in this locality, from which, in more than one instance, I extracted copper; in fact, the microscope readily revealed the presence of the sulphate in the flour.

From half-a-pound of bread I obtained a solution of copper sufficient for 12 demonstrative experiments. Iron was also present in large quantities, showing that the salt of copper used was adulterated.

Were such things practised in Turkey, the guilty individual would be summarily dealt with; in fact, nothing short of hanging, or nailing his ears to a tree, awaits him.—I am, &c.

JOHN HORSLAY, F.C.S.

Cheltenham.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Preparation of Chloride of Gold.—According to M. Fodors (*Repertoire de Chimie pure et Appliquée*, liv. xii. p. 173, 1860) it is impossible to obtain a neutral chloride by dissolving gold in aqua regia and evaporating the liquor to drive off the excess of acid. By this means, says the author, a hydrochlorate of hydrated chloride of gold, $\text{AuCl}_2 \cdot \text{HCl} + \text{Aq.}$ is produced, which is the compound ordinarily met with in commerce. When this is heated even in a water bath to drive off the last equivalent of hydrochloric acid, protochloride of gold is produced, and the salt obtained is not completely soluble in water.

II. ORGANIC CHEMISTRY.

Pyroglycerine—Explosive Glycerine.—Dr. Sobrero gives (*Ibid.* p. 400) the following directions for the preparation of this compound. He makes a mixture of two parts sulphuric acid and one of nitric acid, and when it has cooled he adds one-sixth of its volume of syrupy glycerine. It is not necessary, he has found, to operate at a lower temperature than the ordinary. The glycerine at first dissolves in the mixed acids, but the mixture soon becomes cloudy, and a quantity of yellowish transparent drops collect on the surface. The whole is now poured into a glass containing 15 or 20 times its volume of cold water. The pyroglycerine separates immediately, and falls to the bottom of the vessel; the acid water is poured off, and the product is washed until the washings are no longer acid. The glycerine may then be dried under the air pump. This compound, it seems, is one of the very limited number of explosive liquids, a fact which Mr. Ferris (*American Journal of Pharmacy*, vol. xxxii. p. 524) discovered at nearly the cost of his eye-sight. It is strange that no notice of this dangerous property should be given in Gmelin's *Handbook*, vol. ix. p. 501, where some account of the compound is given. The detonation is extremely violent, a large drop making a report as loud as a gun. Mr. Ferris had not perfectly recovered his hearing three weeks after exploding about half-a-drachm. An experiment may be made without danger by placing a small drop on a watch glass and touching it with a red-hot wire. When dry it will detonate on percussion. Pyroglycerine oxidises phosphorus, potassium, and copper. With hydrochloric acid it separates chlorine and produces, says Dr. Sobrero, a sort of aqua regia.

Chemical Notation.—M. F. de Lastelle has sent a memoir to the Academy of Sciences, which, however, has not been published in the *Comptes-Rendus*, in which he professes to have reduced all formulæ so as to represent only one volume of vapour. The formulæ he proposes

show, he says, besides the atomic composition, the percentage composition and also the composition by volume. Further, they satisfy the law of isomorphism, and the author believes that of the capacity for heat as well. Lastly, he says, the density of gaseous bodies and their chemical equivalent may be represented by the same number. Let us hope that we shall soon hear more of M. de Lastelle's memoir.

MISCELLANEOUS.

Royal Institution.—The following is the arrangement of Lectures for the ensuing week:—Tuesday, February 19, at 3 o'clock, Professor Owen "On Fishes." Thursday, February 21, at 3 o'clock, Professor Tyndall "On Electricity." Friday, February 22, at 8 o'clock, Professor Faraday, "On Platinum." Saturday, February 23, at 3 o'clock, Dr. E. Frankland "On Inorganic Chemistry."

Adulteration of Food Act.—Mr. John Postgate writes:—"I think that some expression of opinion by the Society of Arts would tend materially to remove a misconception which prevails respecting the provisions of the recent Act of Parliament for the Prevention of the Adulteration of Food or Drink. The first clause of that Act makes two offences; one, that of selling knowingly articles adulterated with substances injurious to health, and would require proof of such knowledge before conviction; but the other goes to the root of the mischief, and places a remedy in the hands of the public, for it expressly states, 'every person who shall sell, as pure or unadulterated, any article of food or drink which is adulterated or not pure, shall for every such offence,' &c., forfeit and pay a penalty not exceeding £5, with costs; and for a second offence the name and residence of the offender may be advertised at his expense in the newspapers. It is, therefore, perfectly clear, under this Act, that purchasers have only to ask for the pure article and request the seller to label it as such,—and equally clear is it that impure and adulterated articles of food or drink must be sold as such; but, even these may bring the seller under the law, should they contain any material hurtful to health, as the magistrates may refuse to admit a plea of want of knowledge of his business by the seller, or that no means of knowing (an Analyst being appointed in his district) existed within his reach. Respecting the Public Analysts appointed, it is of the highest importance that they should be, not only well skilled in chemistry and the use of the microscope, but also persons of well-established reputation for integrity and honourable conduct, otherwise the power placed by this Act in their hands may be used for very bad purposes; and, I think, in all cases where analysts are appointed by local authorities, the inspectors of meat and markets should be ordered by those bodies to make purchases, submit them to the analyst, and initiate the proceedings before the magistrates,—a work quite within their duties, and one that may be carried on under this Act of Parliament. It now rests with Corporations to protect the community against the great evil of adulteration, and avert such calamities as occurred at Bradford and other places. I am glad to find, as the originator and worker of this question for several years, that the Society of Arts has taken this important matter up."—*Journal of the Society of Arts.*

ANSWERS TO CORRESPONDENTS.

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"* In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

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Per-hydrate of Alumina.—R. P. wishes to know if this substance is an article of commerce, and if so, where it can be procured.

Gas Purifiers.—H. N.—Mr. Bowditch mentions that the apparatus for his process of testing gas with hot lime, can be obtained of Mr. John Savile, jun., Wakefield, for the price of 18s.

The Regularity of Natural Forms.—An article on this subject is declined with thanks.

P. M. H.—Received.

Colouring Matter from Wood.—A correspondent wishes to know the name given to the active colouring matter obtained from wood by exhaustion in alcohol and precipitation by water. By being raised to a temperature of 250 deg. Centigrade, in a steel cylinder, when cooled, the principle deposits in the form of yellow crystals on the surface of the cylinder.

B. P.—Your suggestion of saturating the stone alternately with two salts which will precipitate an insoluble compound in its pores, is by no means new; nor are the salts proposed (chloride of calcium and sulphate of soda) the most suitable for that purpose. Mr. Ransome's process of using silicate of soda and chloride of calcium alternately, is the most perfect according to theory; but there are difficulties in the way of its successful application to stone, owing to the pores of the latter being usually filled with moisture.

Books Received.—"A Manual of Elementary Chemistry." By G. Fownes. Edited by Dr. H. Bence Jones and Professor Hofmann, Eighth Edition. London: J. Churchill, New Burlington Street. "A Manual of Photographic Chemistry." By T. F. Hardwich. Sixth Edition. London: J. Churchill, New Burlington Street. "The American Journal of Science and Art," No. 91.

THE ADULTERATION OF FOOD.

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THE CHEMICAL NEWS

VOL. III. No. 64.—February 23, 1861.

SZERELMEY ON THE PRESERVATION OF STONE.

EVERY Englishman who takes the least interest in the topics of the day knows the cruel fate of Barry's Gothic masterpiece. It has been the fashion among many, unable to comprehend its beauty, to lavish every depreciatory term upon it. The epithets "gingerbread," "gimcrack," "toy," &c., have not been spared. The expense of the erection has been a fruitful topic of declamation at Debating Societies; the style of architecture has been considered unsuited to the country; the locality has been ill-chosen; in short, there is no fault that has not been found with it, no term of ridicule that has not been applied to it. Nevertheless, all this would have had its day; the amateur political economist who, at his Debating Club, fulminated invectives against the "costly toy" (with an energy always inversely to his pecuniary stake in the affair), would have lived to show it with pride to his country cousins. But, alas, the grumblers have the best of the argument, for, absolutely before its completion, it is beginning to decay. In a century it will be—but we must not attempt to prophecy *what* it will be, for the probabilities are, rather, that it will have *ceased* to be altogether. The fact cannot be palliated that a cruel blunder has been committed, and the question becomes, Is there no remedy? The importance of the question, and the vast amount of money involved, has encouraged a host of inventors to endeavour to profit by the occasion. Many of their proposed processes are absurd, a few injurious, and a very few tolerably feasible. The processes proposed may briefly be summed up in the three classes of the Greasy, the Dirty, and the Siliceous. We need not remind our readers of the controversies that have taken place upon the matter: how some have recommended one process and some another. All processes have had their advocates, particularly the more absurd.

Among the inventors who have come forward, M. Szerelmey occupies a prominent position. He has chosen the doubtful policy of standing forth as the champion of a secret process, which he states to be a revival of one of the methods employed by the ancients. Secrecy, in this country, and in the present state of science, always begets suspicion; but truly it is not easy to know what course is the wisest for an inventor to pursue. The present patent laws utterly fail to protect in the majority of instances, and in an almost equal number enable unconscientious patentees to harass and persecute *bona fide* inventors. M. Szerelmey, aware that the feeling in this country is unfavourable to the proprietors of secret processes, and that we are perhaps too liable to class them with the genus *Dousterswivel*, has published a pamphlet¹ with the view of directing the public attention to his invention, and proving its value. In

the present article we shall confine ourselves to two principal points, discarding, for the time, all comparisons with other writers; these are—1st, A general review of M. Szerelmey's pamphlet, and 2nd, Does his pamphlet prove his process to be successful?

We may at once state that the impression left by the little work before us is not pleasing. As the work of a foreigner, we forgive the spelling, which is so wretched that one is almost tempted to fancy it written by the dictation of a "medium" at a "spiritual" demonstration. But we cannot so easily forgive, in a pamphlet written for a commercial purpose, such a perpetual affectation of the *savant*, and such a frequent allusion to the author's "travels and archaeological researches." The author, in his Preface, says:—

"Preferring the practical to the theoretical, the positive to the speculative, I have avoided as much as possible, the notices and comments of ancient writers, which, on the subject of the Encaustic processes, are far from being satisfactory, contenting myself with explaining, so far as is politic and just to myself, the nature of my discoveries, and their close approximation to the usages of the nations of antiquity."

Nevertheless, twelve pages out of twenty-nine are chiefly occupied by a vague account of the principal features of ancient art as applied to architecture and decoration, and the rest of the work is taken up with objections to the use of the siliceous process, and praise of his own. He does not, however, give one single *proof* of the value of his own method. Not one conclusive experiment is cited as having been submitted to a competent ordeal; and, as he does not indicate in any specific terms the nature of his invention, we contend that the promise in the Preface, to explain the nature of his discoveries, "and their close approximation to the usages of antiquity," has not been fulfilled. We never perused a work where assertion was more frequently made to take the place of proof. That M. Szerelmey is deficient in chemical knowledge of the kind most required in carrying out an investigation like the present is evident in numerous places in his pamphlet. He talks of earthy pigments being "mixed up with a solution of wax or other bituminous matters, or otherwise laid on as water colour, and subjected the third time to the Zopissa process. He speaks of ingredients being of the like "bituminous or carbonaceous nature." What will our mineralogical and chemical readers say to the mistakes and confusion of ideas contained in the following extract:—

"Previous to my travels, I had familiarised myself with the preserving qualities of certain bituminous products, such as the entombment of flies, ants, &c., indurated or fossilised in gum, amber, mellenite (*sic*), or honeystone, &c., the fossilisation of wood and other organic substances, and had studied the conditions under which these events take place; the causes being evidently a high temperature, not sufficient to destroy, but enough, with the aid of surrounding bodies, so to deprive the concrete, or plastic

¹ "On The Encaustic and Zopissa Processes as Applied by the Ancients for Indurating and Preserving Stone, Cement, Brick, Timber, Sculpture, and Paintings: with some Account of the Revival of these Processes." By N. C. SZERELMEY.

matter, of its aqueous element as to consolidate its parts and cause its induration, in which state it is enabled most effectually to resist atmospheric influences and other destroying causes for ages. It is not for me to say what the relationship is between silica and carbonaceous bodies, or how it is animal and vegetable matters are converted into siliceous aggregates, as fossil-wood, fossil-oils, amber, resin, wax, and honey; but the indurating properties of these and other bodies of a like nature have proved to me, in practice, the best preservatives of wood, stone, and concretes against atmospheric and other destroying influences."

Further on, silica is said to be

"A compound of homogeneous elastic qualities, having the tendency to reject all other earths, and only uniting with them under high influences of heat; neither can any of the chemists of the day, by synthesis, return the silica to its original state."

If M. Szelcmeijer will tell us what its "original state" was, we will try to solve the required problem. With the extracts given, which are, we think, quite sufficient for the purpose, we conclude; for, not knowing what our Author's process is, we cannot speak for or against it; but this we can say, that the pamphlet does not prove its efficacy.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Influence which Sulphide of Arsenic exerts upon the Solubility of Sulphide of Antimony in Hydrochloric Acid, by FREDERICK FIELD.

THERE are few chemists, it may be presumed, who have not attempted to separate the sulphide of arsenic from the sulphide of antimony by means of hydrochloric acid. The solubility of the latter in this menstruum seems so likely to afford a good result, the arsenic compound being so insoluble when the acid is dilute, as to cause no surprise that many experiments to effect the separation of arsenic from antimony by this means have been resorted to. The quantitative analysis of these two elements when combined with sulphur, by hydrochloric acid, either dilute or concentrated, seems, however, to be impossible. The subject was investigated by me, some years ago, with entirely negative results, and the same researches have been lately resumed with no better success: their publication, however, may prove interesting, as showing the great and singular influence which the sulphide of arsenic exerts upon the sulphide of antimony.

5 grains of very pure tersulphide of antimony were dissolved in dilute hydrochloric acid at a gentle heat, water added slightly acidulated with tartaric acid, and the solution left to cool. When cold, a stream of hydro-sulphuric acid was passed through the liquid, and, after standing some hours, the precipitate was thrown upon a tared filter, washed, dried at 212° , and weighed. The weight of the sulphide proved to be 5.02 .¹

5.00 grains of the same compound were dissolved as before, and treated precisely in a similar manner. The precipitate upon the filter was washed into a flask by a stream of liquid, consisting of one part of moderately

concentrated hydrochloric acid and three parts of water. The quantity of fluid employed was as nearly as possible four ounces by measure. The flask was placed upon a small sand-bath, heated by a gas-burner, and in eight minutes the liquid entered into ebullition. After boiling four minutes, the sulphide was entirely dissolved and the contents of the flask perfectly clear. It was removed from the sand-bath and allowed to cool. When perfectly cold, a few ounces of water were added, and a stream of hydro-sulphuric acid again transmitted. The sulphide now weighed 4.980 grains, showing that little or no chloride had been volatilised, the small difference being easily accounted for as inevitable in the analysis.

5.00 grains of arsenious acid were dissolved in a weak solution of potassa, hydrochloric acid added in slight excess, and the sulphide precipitated by sulphuretted hydrogen. The precipitate, dried at 212° , weighed 6.19 , calculation requiring 6.21 .

The same quantity of arsenious acid was subjected to similar treatment, and the sulphide of arsenic washed into a flask by acid of the same strength and in the same quantity as that employed with the antimony. The flask was placed upon the sand-bath and heat applied. as in the former instance, the liquid began to boil in eight minutes, and, after boiling four minutes, the heat was removed. The sulphide of arsenic was then filtered, washed, dried at 212° , and weighed 5.89 grains = 4.74 arsenious acid.

The filtrate, oxidised by chlorate of potash and hydrochloric acid, and gently heated until the oxides of chlorine had been entirely expelled, was mixed with an excess of ammonia and a small quantity of solution of sulphate of magnesia. After standing for twenty-four hours, the arseniate of magnesia and ammonia was placed upon a filter of known weight, washed with ammoniacal water, dried exactly at 212° , and weighed.² Weight of arseniate of ammonia and magnesia, $(2\text{MgO}, \text{NH}_4\text{O}, \text{AsO}_3, \text{HO})$, $0.37 = 0.192 \text{ AsO}_3 = 0.237 \text{ AsS}_3$, from which it appears that little or no chloride of arsenic was volatilised, as the following resumé of the above experiments will show:—

Taken.

Found.

$\text{AsO}_3 - 5.00 = 4.93 \text{ AsO}_3 = 6.12 \text{ AsS}_3$

These preliminary experiments being undertaken in order to prove, not only the purity of the substances employed, but also to examine the action of dilute hydrochloric acid upon the sulphides individually, 5.00 of AsO_3 and 5.00 SbS_3 were brought into solution as before, and the liquids placed in one flask. After precipitation by sulphuretted hydrogen, the washed sulphides were treated with hydrochloric acid in the same quantity and same strength as they were separately, and heated for the same length of time. The mixed precipitate, which, although somewhat lighter in colour than when originally precipitated, still had a bright orange tint, indicative of the presence of antimony in considerable quantity, was found, after drying, to weigh 10.11 grains. The sulphides were thoroughly oxidised, ammonia and nitrate of ammonia added, and the arsenic precipitated by sulphate of magnesia. The ammonio-magnesian arseniate weighed 9.08 grains = 5.88 AsS_3 . The antimony was precipitated from the filtrate by sulphuretted hydrogen,

¹ It is well known that precipitated tersulphide of antimony holds a little water with great tenacity at 212° , and even at a temperature considerably beyond this. To dehydrate it effectually, the sulphide must be heated in a tube through which a stream of dry carbonic acid is passed, and maintained at 300°F .

² Particular attention must be paid to the exact temperature in the desiccation of the ammonio-magnesian arseniate. It is only at 212° , or, at most, a degree or two lower or higher, that the compound $2\text{MgO}, \text{NH}_4\text{O}, \text{AsO}_3, \text{HO}$ can exist, as it loses the atom of HO at about 215° , and below 212° it contains more water than that indicated in the formula.

after acidification, and the sulphide oxidised and weighed as antimoniate of teroxide of antimony (SbO_4), SbO_4 , 3.82 grains = 4.23 SbS_3 .

It thus appears that scarcely 0.3 of sulphide of antimony were dissolved when sulphide of arsenic was present, although the acid liquid would have decomposed the whole amount of precipitate had arsenic been absent. The filtrate from the sulphides was oxidised and the arsenic estimated by magnesia.

2 MgO , NH_4O , AsO_5 , HO , 0.38 grains = 0.193 AsO_3 = 0.239 AsS_3 , proving that, although the sulphide of arsenic had affected the solubility of the sulphide of antimony, the latter had exerted no influence upon the solubility of the former.

In order to observe the action of strong hydrochloric acid upon the sulphide of arsenic and a mixture of that compound with sulphide of antimony, 5.00 arsenious acid were precipitated by sulphuretted hydrogen, and the sulphide digested for an hour in two ounces of hydrochloric acid, fresh acid being added to compensate for evaporation, in such a manner as to keep the liquid at about the same level in the flask during the whole time of ebullition. After filtration, the sulphide of arsenic weighed 1.85 grains = 1.48 arsenious acid. The filtrate, oxidised as before, gave 1.64 2 MgO , NH_4O , AsO_5 , HO = 1.06 AsS_3 = 0.85 AsO_3 . Thus, more than half the arsenic in the arsenious acid had, by prolonged boiling, been volatilised as chloride.

5.00 arsenious acid and 5.00 sulphide of antimony were dissolved, precipitated, and treated with two ounces of strong hydrochloric acid as before. The residue weighed 3.87 grains, and, oxidised by chlorate of potash and hydrochloric acid, gave, on precipitation by ammonia and sulphate of magnesia 2.82 grains 2 MgO , NH_4O , AsO_5 , HO = 1.83 AsS_3 , which, deducted from 3.87, leaves 2.04 grains as the weight of the sulphide of antimony. The strongly-acid filtrate from the sulphides was oxidised as usual, and the arsenic estimated as arseniate of magnesia and ammonium. 1.72 (2 MgO , NH_4O , AsO_5 , HO) = 1.12 AsS_3 = 0.901 AsO_3 . The antimony, precipitated as sulphide, and oxidised by strong nitric acid, was ignited and weighed as SbO_4 . Weight of SbO_4 , 2.32 = 2.57 SbS_3 , which, added to 2.04 = 4.61 SbS_3 .

We learn from the above facts how infinitely more volatile chloride of arsenic is than chloride of antimony under these circumstances, and also what a strange protective action the arsenic compound exerts over the sulphide of antimony, rendering it partially insoluble, and preventing its decomposition, to a great extent, in the presence of hydrochloric acid at a boiling temperature. The case is different with some other sulphides. I have shown¹ that antimony and mercury may be separated with a considerable degree of exactness by heating the sulphides of those metals in hydrochloric acid, the antimony dissolving, the mercury being left intact. It would almost appear, although the conclusion may seem hasty, that when solutions of arsenic and antimony are precipitated together, or even when the two sulphides, after individual precipitation, are mixed and digested in hydrochloric acid, that the sulphide of arsenic plays the part of an acid with regard to the antimonial sulphide, a sulpharsenate or sulpharsenite of sulphide of antimony being formed, which is insoluble in hydrochloric acid. Be this as it may, there is no doubt that when even a very small quantity of antimony is precipitated

with a great excess of arsenic, as sulphide, it becomes very insoluble in hydrochloric acid. I have made no quantitative determinations upon this point, but have always detected the presence of antimony in the residue.

With regard to the solubility of sulphide of arsenic; this compound, although described in most chemical works as almost insoluble in hydrochloric acid, is sensibly decomposed by that re-agent, even when highly diluted, after digestion with it for some time. Moreover, it even gradually suffers decomposition in pure water, especially if the liquid be slightly warmed, sulphuretted hydrogen being evolved and arsenious acid produced. Dr. Odling made the same remark to the Fellows of the Chemical Society in a discussion which followed Mr. Bloxam's first paper upon the Detection of Arsenic by Electrolysis, and I have since seen it affirmed by other chemists, as well as noticed it in my own experiments.

On the Chemical Analysis of the Solar Atmosphere.

KIRCHHOFF has communicated some further results of his remarkable investigations on the constitution of the solar atmosphere. The author maintains that the sun has an ignited gaseous atmosphere, which encloses a core of still higher temperature. If we could see the spectrum of this atmosphere, we should detect the bright lines which are characteristic of the metals existing in it, and should recognise the metals themselves from these. The more strongly luminous body of the sun does not, however, permit the spectrum of his atmosphere to appear. It inverts this spectrum; so that instead of the bright lines which the spectrum of the atmosphere alone would exhibit, dark ones make their appearance. We see, therefore, only the negative image of the spectrum of the sun's atmosphere.

In order to study the solar spectrum with the requisite degree of accuracy, Kirchhoff procured from the workshop of Steinheil an apparatus consisting essentially of four large flint-glass prisms and two telescopes.

With this apparatus the spectra are seen in a hitherto unattainable degree of distinctness and purity. It exhibits in the solar spectrum thousands of lines, with such clearness that they are easily distinguished from each other. It is the author's intention to draw the whole spectrum, as seen with his apparatus, and he has already done this for the portion which lies between Fraunhofer's lines D and F.

This apparatus exhibits the spectrum of an artificial source of light with the same distinctness as the solar spectrum, provided only that the intensity of the light is sufficient. A common gas-flame, in which a metallic compound evaporates, is usually not sufficiently luminous, but an electric spark gives with the greatest distinctness the spectrum of the metal of which the electrodes consist. A large Ruhmkorff's induction-coil yields electric sparks in such rapid succession that the spectrum can be observed as easily as that of the sun.

A very simple arrangement permits the comparison of the spectra of two sources of light. The rays of one of the sources may pass through the upper half of the vertical slit, while those of another pass through the lower half. When this is the case, one of the two spectra is seen immediately beneath the other, and it is easy to determine whether coincident lines occur in both.

In this manner the author satisfied himself that all the bright lines peculiar to iron correspond to dark lines in the solar spectrum. In the portion of the spectrum

¹ Quarterly Journal of the Chemical Society, vol. xi. 1859.

between D and F, about seventy particularly well-marked lines occur, resulting from the iron in the sun's atmosphere.

Iron is remarkable on account of the great number of distinct lines which it produces in the solar spectrum. Magnesium is interesting because it produces the group of Fraunhofer's lines lying in the green denoted by Fraunhofer by *b*, and consisting of three very strong lines. Very distinct dark lines in the solar spectrum correspond to the bright lines produced by chromium and nickel, and we may, therefore regard the presence of these substances in the sun's atmosphere as proved. Many other metals appear, however, to be wanting in the sun's atmosphere. Silver, copper, zinc, lead, aluminium, cobalt, and antimony have extremely brilliant lines in their spectra; but no distinct dark lines in the solar spectrum correspond to these.

Many metallic compounds do not give in a gas-flame the spectrum of their metal, because they are not sufficiently volatile. In these cases the spectrum may be made to appear by means of the electric spark. It is true that in this case the spectrum of the metal of which the electrodes consist and that of the air in which the spark passes is also seen. To avoid the difficulty arising from the very great number of bright lines of which the spectrum of every electric spark consists, it is necessary to have recourse to a particular arrangement. The electric spark is allowed to pass at the same time between two similar pairs of electrodes, the light of one spark being allowed to pass through the upper, that of the other through the lower half of the slit, so that one spectrum is seen above the other. When the two pairs of electrodes are clean, the two spectra are perfectly similar; when, however, a metallic compound is placed upon one pair, the corresponding spectrum immediately shows the lines belonging to the metal introduced. The author has satisfied himself that in this manner even the metals of the rare earths, yttrium, erbium, terbiu, &c., may be recognised most quickly and certainly. It is, therefore, to be expected that, by the help of Ruhmkorff's apparatus, the spectral method of analysis may be extended to the detection of all metals. The researches which the author has undertaken, in connection with Bunsen, will, it is hoped, determine this point.—*Journ. für Prakt. Chemie.*

Notes on Furfural, by Dr. C. ULRICH.

In the *Annalen der Chemie und Pharmacie*, band xiv., p. 63, Schwanert states that Professor Schulze, of Rostock, succeeded in converting furfural into pyromucic acid by treating it with oxide of silver. This change can be made to take place in the following very simple manner.

To an alcoholic solution of furfural a solution of potassa in absolute alcohol is added; the mixture becomes hot and solidifies almost immediately into a paste of scaly, shining crystals. These are pressed between folds of blotting-paper, and the brown coloured mass is then repeatedly crystallised from very strong alcohol. Thus the potassa salt of pyromucic acid is obtained in colourless plates of mother-of-pearl lustre. These crystals are very soluble in water and in dilute alcohol; but very slightly soluble in absolute alcohol. 0.223 grammes decomposed by sulphuric acid gave 0.128 grammes sulphate of potassa; hence the potassa present was 31.03 per cent. The formula $C_{10}H_6O_5 \cdot K_2O$ gives 31.42 per cent. If the potassa salt is decomposed by hydrochloric acid, evaporated to dryness, and heated for some time

on the water-bath, in a porcelain basin covered with paper, the pyromucic acid sublimes, and is deposited on the under surface of the paper in shining colourless needles, which may be an inch long.

When burnt, 0.1950 grammes gave,

Carbonic acid 0.3840 grammes
Water 0.0690 "

These numbers correspond to

C 53.69 per cent.
H 3.92 "

The formula $C_{10}H_6O_5$ gives,

C 53.57 "
H 3.57 "

The crystals melt at 129° C. (without correction).

By the action of potassa on furfural the alcohol of this substance is most probably formed, just as from bitter almond oil, by similar treatment, the alcohol of benzoic acid is produced. I am at present engaged with experiments to prove this conclusively.

On the Existence of a Fourth Member of the Calcium Group of Metals, by F. W. and A. DUPRÉ.

In the last Number of the *Philosophical Magazine* appears a paper on this subject by the above gentlemen. During their recent examination of London waters by the beautiful method of Kirchhoff and Bunsen, they several times noticed a faint blue line, not due to strontium or potassium, or to the lately-discovered caesium. Having since worked with larger quantities of the deep-well water, which gave this line most distinctly, they believe they are now justified in stating that the group of calcium, strontium, and barium, like that of the alkali metals, contains a fourth member. This new metal gives but one blue line,¹ situated between the lines $Sr\delta$ and $K\beta$, about twice as far from the former as from the latter. In brightness and sharpness it is quite equal to the line $Sr\delta$. The method which, after repeated trials, they found most advantageous for obtaining it in a state of comparative purity is the following:—The deposit formed by boiling the water is dissolved in hydrochloric acid, and a small quantity of sulphuric acid added to the clear solution. The precipitate formed, consisting principally of sulphate of calcium, but containing some sulphate of barium, sulphate of strontium, and sulphate of the metal under consideration, was collected, washed, dried, and fused with carbonate of sodium. The fused mass was then boiled with water and the insoluble carbonates of the above-named metals collected and treated several times successively in the same manner as the original deposit. By these means the lime was gradually removed, the carbonates becoming proportionally richer in barium, &c. Owing to the small quantity of substance at their command, they did not attempt to remove the lime entirely. The carbonates finally obtained were once more dissolved in hydrochloric acid, and the solution, after considerable dilution, mixed with a few drops of sulphuric acid. After standing at rest for twenty-four hours, the slight quantity of deposit formed, consisting of almost pure sulphide of barium, was filtered off and some alcohol added to the filtrate, by which a further deposit was obtained, composed chiefly of the sulphates of strontium and the new metal, though not quite free from sulphate of calcium. These sulphates were again converted into carbonates, which were then dissolved in hydrochloric acid, and the

¹ Having been unable to separate it completely from calcium and strontium, they are not quite positive whether or not it gives any lines at the red end of the spectrum.

solution evaporated to dryness. When a portion of this dry residue is brought into the flame of the apparatus the spectra of calcium and strontium appear, and, in addition, beyond the line $\text{Sr}\delta$, in the position indicated above, a blue line, rivalling the strontium line in brilliancy and distinctness of outline.

As far as they have at present been able to ascertain, the carbonate, oxalate, and sulphate of the new metal are insoluble in water, the last-named possessing about the same insolubility as sulphate of strontium. The chloride of the metal does not seem to be hygroscopic, resembling in this respect that of barium rather than those of strontium and calcium.

On the Maturation of Fruits, by MM. BERTHELOT and BUIGNET.

THE changes which fruits undergo during ripening constitute the most remarkable phenomena of vegetable physiology. Nothing is more interesting than to see fruit lose, little by little, its astringent and acid flavour, and acquire the sweet and agreeable taste which renders it fit for food. This change is especially worthy of attention in the case of a fruit detached from the plant on which it was developed. In this case, in fact, the fruit constitutes a complete medium, which derives nothing more from the plant which formed it, all the ulterior changes it undergoes resulting from the reciprocal metamorphoses of its own materials. It is under such conditions that we may hope to study most simply the play of these metamorphoses; to comprehend, for instance, the laws presiding over the formation of vegetable acids and over the saccharine principles.

We have undertaken to make a series of inquiries on this subject, the first results of which we now lay before the Academy. The results were obtained two years since, during the winters of 1858 and 1859. Our reason for mentioning these dates is to show how slow and laborious such researches are. They exact innumerable analyses and experiments, which can only be carried on from year to year, because certain conditions of these experiments are supplied by Nature and cannot be produced at will. In this first paper we propose mainly to develop, by a special example, the methods we employ and the end we have in view, not, however, pretending as yet to give definite results.

We have experimented on the common orange, one of the fruits which most distinctly shows the period of maturation. The structure of the orange, the clear distinction between its various parts, the thickness of its rind, which effectually isolates its juice, and, lastly, the relatively rapid period of its artificial maturation, offer especial facilities for observation.

The chemical composition of the juice of the orange adds to these facilities. In fact, the juice is principally composed of citric acid, fermentable sugars, and nitrogenous principles. The gelatinous substances analogous to pectin, which play so important a part in the maturation of certain other fruits, as M. Fremy's researches have shown, exist in the orange, on the contrary, in very insignificant proportions.

The following is our method of operating:—We take a certain number of green oranges, susceptible of the ripening process, and distribute them in two series, one comprising the riper oranges, the other, those less ripe, putting aside a few from each series for analysis. We then leave them for a few weeks in a dry place, maintained at a gentle temperature. At the expiration

of this time, if the maturation is effected, we repeat the analysis.

In each of these analyses we have effected the following determinations:—

1. Weigh each orange.
2. Separate it into four parts, viz., rind, seeds, juice, and pulp. Weigh these four parts, and determine the quantity of water and residue, fixed at 100°C , they contain.
3. Determine how much azotised and mineral matter, soluble in ether, the dried rind contains.
4. Determine how much azotised and mineral matter is contained in the pulp.
5. Pursue the same plan with the seeds, after determining the number and the mean weight.
6. Estimate the quantity of water, citric acid, inverted sugar, cane sugar, and azotised and mineral matter contained in the juice.

Relying on these determinations, the total composition of the orange may be ascertained with sufficient exactitude, and the variations undergone by the principles it contains compared. This comparison leads to divers interesting results, to which we shall presently advert, with further details. The only facts we desire at present to adduce are those relating to the sugary principles. We give them, moreover, as the results of the two series of comparative analyses which we have just indicated, not, however, pretending as yet to generalise them. The questions are too delicate to be decided upon without certain reservations.

1. The orange, either before or at the period of its maturity, contains, at the same time, cane and inverted sugar.
2. The relative proportions of these two sugars change during maturation; the quantity of inverted sugar, which was previously greater than that of the cane sugar, ceases to preponderate; the relations change, and the cane sugar is found to be the most abundant.
3. The weight of inverted sugar varies little.
4. The weight of cane sugar augments relatively to the total weight of the orange.
5. It increases equally, whether compared with the total weight of the juice or with the weight of the fixed matter contained in the juice.

From these facts, it is easy to understand why the orange becomes sweeter during maturation. The formation of cane sugar in the ripening fruit is here the most striking phenomenon. It is the more interesting, since it is effected in an acid medium; not only does the citric acid appear not to act in inverting the cane sugar already formed,—an inactivity which could be foreseen as a result of the experiments made by one of us,—but it does not oppose the augmentation of the cane sugar itself.

We are now continuing our experiments, in order to strengthen these first results by new studies, and to seek out at the expense of what principles cane sugar is formed.—*Comptes Rendus*, vol. li. p. 1094.

TECHNICAL CHEMISTRY.

On the Solvent Properties of Bichloride of Tin, by M. GERARDIN.

THE solvent properties of bichloride of tin are strikingly analogous to those of bisulphide of carbon. It dissolves nearly the same bodies, but in less proportions.

At its boiling-point it dissolves considerable quantities of octahedral sulphur, iodine, and ordinary phosphorus.

When the solution cools the sulphur and iodine are deposited in beautiful crystals. The phosphorus separates in the liquid state, and solidifies in a mass, but without crystallising.

It dissolves amorphous sulphur after prolonged boiling. The crystals which form on cooling are octahedral.

Red phosphorus is perfectly insoluble in it.

It dissolves bromine and bisulphide of carbon in any quantities.

Silicium, tellurium, arsenic, antimony, bismuth, tin, and metallic oxides and chlorides are insoluble in it.—

Comptes-Rendus, vol. li. p. 1097.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE II.

Oxygen—Its Preparation and Properties—Chemical Combination and Decomposition—Combustion.

THE materials composing the solid portion of the globe we inhabit, as well as its liquid and gaseous envelopes, consist, so far as investigation has gone, of only a certain number, and comparatively a small number, of elements or first principles. There are known at the present time sixty-three of these elementary principles, or kinds of matter, which cannot be resolved into anything more simple—which we cannot tear asunder into two or more different forms of matter. It is true that the number of elements is constantly undergoing change; sometimes we are decomposing substances which have hitherto been considered as elements. By the application of new processes we are resolving elements into other constituents, sometimes into two constituents that were already known; although this operation has become a comparatively rare one. On the other hand, we are sometimes increasing the list of the elements by the discovery of substances hitherto unknown. Within a very short time the number has increased from sixty-two to sixty-three by the discovery of the element Cæsium by Professor Bunsen. Of these sixty-three elements, all forms of matter with which we are acquainted are composed.

It would be easy for anyone, were these elements placed before him, to divide them at once into two great classes. He would, first of all, see a community of property, which we distinguish as metallic character, possessed by a great number of these elements—a lustre on the surface; and, if he went deeper into the matter, he would find they possessed the property of conducting heat and electricity. Another class, much smaller, of these elementary substances would be found to possess none of these properties, or to possess them, at all events, in a very inferior degree. Such bodies as sulphur, for instance, with which you are all very well acquainted, phosphorus, the gaseous constituents of the atmosphere—these bodies do not possess this prominent feature of metallic lustre, nor do they possess the other qualities which we attribute to metals. This classification of the elementary substances into two great divisions, termed respectively, metals and metalloids, the latter including all that do not possess these essentially metallic characteristics—this classification, I say, has been very extensively adopted up to the present time, but it is essentially an unphilosophical one. Although there are certain prominent features belonging to these elements which classify them in the way I have just mentioned,

still when we come to study them more intimately we find that this line of demarcation is not drawn in the manner in which their prominent qualities would lead us to draw it. We find, for instance, that although antimony possesses strictly the metallic characteristic of lustre, and that it conducts heat and electricity very readily indeed; still, when we come to study the chemical properties of this metal, it is essentially associated with the non-metallic bodies; it is associated with nitrogen and phosphorus, two substances which possess in the least degree, perhaps, the properties of metals; so we must either transfer antimony, and also arsenic and tellurium, to the class of metalloids, or we must transfer phosphorus and nitrogen, and one or two other metalloids, into the class of metals. There is, however, a more convenient mode of classification, which we shall adopt in these Lectures. The chemical force may be considered as being a dual force, like electricity; and if we look at it from this point of view we find that one part of this force—which we may term the positive portion—accumulates upon one kind of element; and the negative portion of the force upon another kind or class of elements. These two qualities of the chemical force connect these elements in an electrical manner with each other; and so closely does this connection obtain that some chemists have not hesitated to declare that the chemical force and the electrical force are indeed identical. The beautiful discovery of Faraday, that an equal quantity of electricity is required to decompose each of a large number of compounds, shows that there is some close connection between these two forces. Without committing ourselves, however, to this connection more than is necessary, some of the elements are endowed with what we may term *positive energy*, and some are endowed with what we may call *negative energy*. I have drawn up here [pointing to a diagram] a table exhibiting this kind of classification. We find that all the metals, for instance, are endowed with positive energy; that this positive portion, so to speak, of the chemical force, when these bodies are torn asunder, accumulates itself upon the metals, hydrogen, carbon, boron, silicon, phosphorus, and nitrogen; while the negative energy accumulates itself in like manner upon oxygen, fluorine, chlorine, bromine, iodine, sulphur, and selenium. Now, if you omit the metals from the list, you have before you the names of the metalloids.

Although the positive elements can combine amongst themselves—for instance, the metals can combine with hydrogen, hydrogen can combine with carbon, and so on; and whilst the negative elements can also combine amongst themselves—oxygen with chlorine, or with fluorine, or with sulphur,—they are far less characterised by that energy of action which we saw in so many cases in the last Lecture, than when an element belonging to one of these classes, combines with one belonging to the other class. It is only when a member of the positive family combines with a member of the negative family that we have manifested those energetic characteristics of the chemical force to which I have already directed your attention.

Now, amongst all the changes that occur through all these manifestations of the chemical force, let me just remark in passing that one change never occurs, and that is expressed in the uppermost sentence of our memoranda there. *Chemical affinity never alters the weight of matter.* We have seen that it will alter the force with which matter is attracted by the magnet, and that it brings about many other changes; but, however great these changes may be, the total weight of the matters acted upon by the chemical force is never in the slightest degree affected.

Chemical affinity may be exerted in four principal ways, which it will be well for us to note before proceeding to study its action upon certain of the elements.

The first and most simple mode in which it acts is that

of directly combining two elements with each other. If for instance, we take the two elements, phosphorus and iodine—and you see we select them from the opposite sides of our table, the element phosphorus being among the positive, whilst the iodine belongs to the negative class;—and if we want to produce any effect which shall be visible from a distance, without refined modes of observation, it is quite necessary that we should select elements from opposite sides. [The iodine and phosphorus were then brought into contact, the phosphorus immediately taking fire.] You see the two bodies immediately unite forming iodide of phosphorus. This is the most simple case of chemical union. I might refer to it as an instance in which two solid bodies unite chemically; but if we look further into the matter we find that it is not really solid bodies that are operating. They are both volatile bodies, and being placed together, their vapours commingle, heat is produced, and one of the bodies is liquefied; and, consequently, the process of combination takes place after all between a liquid and a solid, or, possibly, between two liquids.

The next mode in which chemical affinity acts is by what we term *displacement*. Here, for instance, I have a solution which contains copper in combination with sulphuric acid; and if I plunge into this solution a bright blade of iron, I effect a substitution of iron for copper in the original compound. You see my blade of iron has become completely plated with copper: it appears now to be, in fact, a blade of copper. Now, what have we done here? In the first place we had sulphate of copper, consisting of one equivalent or combining proportion of sulphur, four equivalents of oxygen, and one of copper. Now, when I have plunged this blade of iron into the copper solution, I simply substitute an equivalent of copper by an equivalent of iron, the copper which is set free depositing itself upon the iron blade, and we obtain a certain quantity of the sulphate of iron.

The third mode in which chemical affinity is exerted is in an exchange of constituents between two compounds. I ought to mention, perhaps, that the second mode I have just alluded to is commonly termed *single elective affinity*, because the chemical force is assumed to make a selection between the copper which is first combined and the iron which is afterwards added to it. Now let me illustrate this third process by mutual exchange of constituents between two compounds. Here we have a solution of chloride of mercury, to which I add a solution of iodide of potassium. [The solutions were poured into one vessel.] Immediately a change takes place,—a new body is obviously produced, and that new body is iodide of mercury; whilst the chlorine, which was formerly in combination with the mercury, has gone over to the potassium of the iodide of potassium. We have in one case chlorine and mercury, and in the other case potassium and iodine. Now, the exchange was simply effected between these two compounds in this manner (illustrating the change by means of cubical blocks representing the elements in question), so we have now the compounds of iodine with mercury and chlorine with potassium. This mode is sometimes called *double elective affinity*.

Finally, the fourth mode in which chemical affinity is exerted, consists in the re-arrangement of the constituents of a compound. This re-arrangement—this mode of the exertion of chemical affinity—is one which occurs comparatively very rarely in inorganic chemistry. We shall, perhaps, meet with one or two instances of it; but it is a change which frequently occurs in organic chemistry. I need only refer to one example; namely, the transformation which takes place with sugar in contact with yeast, by which it is converted into alcohol and carbonic acid.

Having thus noticed these four peculiarities of the force with which we have to deal, let us now make ourselves acquainted with some of the more important elementary substances themselves. Amongst these the element

oxygen, demands to stand in the foremost rank. It is not only the most abundant, but it is also the most important element. In every nine tons of water existing upon our globe, there are eight tons of this element oxygen; and, at least, one-third of the total weight of the solid crust of the globe consists also of this element; whilst one-fifth of the volume of the atmosphere is also made up of oxygen gas. The greater part, however, of this element—in fact, we may say nearly the whole of it, exists in combination. Its affinities are so powerful, that scarcely anything can exist upon the surface of this globe, or in its interior possibly also, uncombined with oxygen, so long as there is any of it to combine with. Therefore, as might be expected from the qualities of this element, it is nearly all in combination—all that is not in the atmosphere, with the exception of a certain quantity dissolved in the waters of the ocean,—not *combined* there, but *dissolved*. It is interesting to reflect that the oxygen which is left out of combination is only the portion over and above that which is necessary to saturate all the other elements. After the other elements have taken all the oxygen they require, a certain quantity has remained over and above in excess of their demands; and if this quantity had not been left over and above, our globe could not have been inhabited by beings such as now people it, nor could it have been the seat of vegetable life. Now, the quantity of oxygen which is thus left out of combination is exceedingly small, being only one two-millionth of the total amount; hence, if there had been among the constituents of this globe one two-millionth part less of oxygen than there is, it would have made just the difference between an inhabited and an uninhabited globe; for there would not have been any oxygen in the atmosphere, and consequently the existence of animal and vegetable life would have been impossible.

There are several processes by which we can obtain this element, oxygen. They are all of them, however, based upon the plan of extracting a portion of this free oxygen that exists in the atmosphere. We cannot directly extract the oxygen from the air, owing to its affinities being more powerful than that of the nitrogen with which it is associated; but we have a number of processes for withdrawing the oxygen from the atmosphere, and entrapping it, as it were, into very feeble compounds, from which we can extract it very easily. Nearly all the processes for preparing this element depend upon this principle of entrapping it into a combination from which it is very easily separated, and then expelling it from that compound. The most interesting and perhaps the most simple process is the one that was first employed by Priestley for the preparation of this important gas,—the mode, in fact, in which he discovered oxygen to be a separate and independent substance. If we take metallic mercury and heat it just below the boiling point it absorbs oxygen, and is converted into red oxide of mercury; and if we afterwards heat this to a higher temperature in the air it gives out all the oxygen with which it had united in the former case whilst it was near the boiling-point. When the compound is thus heated, it is resolved into these two elements, and the oxygen is set at liberty. That is one process of preparing oxygen; it is not the process we ordinarily adopt, because it is a slow mode of extracting it from the air, and the process is consequently far less available for preparing large quantities of this substance. We may use, in place of the oxide of mercury, a salt termed chlorate of potash, which is manufactured in large quantities, and which is employed in numerous manufacturing operations, and it is therefore available for our purpose in large quantities. This salt requires only to be heated to a temperature rather lower than that of the oxide of mercury in order to resolve it into chloride of potassium and oxygen. Let us see what we do in this case; the chlorate of potash is a salt which contains a very large quantity of oxygen. It con-

sists of one equivalent of potassium, one of chlorine, and no less than six equivalents of oxygen. [The Lecturer illustrated the proportions by means of cubical blocks, representing the various atoms, placing the potassium and chlorine below, and the oxygen at the top.] See what a pile of oxygen we have combined with this comparatively small amount of matter at the base. By the temperature to which we are exposing this salt in the glass, we are resolving it into chloride of potassium and free oxygen gas. All the volatile oxygen gas passes off, leaving the non-volatile salt (chloride of potassium) behind. Now, this method is scarcely sufficient for the preparation of the gas in large quantities; and we have a very decided improvement upon it. This improvement consists in mixing with the chlorate of potash some metallic oxide, such as binoxide of manganese. These metallic oxides do not suffer any change themselves, but they very greatly facilitate the transformation into the chloride and free oxygen. That is the process we are using here [pointing to a third apparatus], and you may see how very much more rapidly the gas is coming off from this mixture than from the chlorate of potash when heated alone; and finally, you may see that the process by the oxide of mercury affords us a still smaller quantity of oxygen than either of the other two plans. There is still another process of obtaining oxygen which has recently been proposed. I had intended to show it you at work, but it was an operation not exactly suited to the lecture-table, inasmuch as without very great care and good ventilation it is liable to evolve a great quantity of the fumes of sulphurous acid into the air. The oxygen from the air must be entrapped into a compound from which it is easily obtained. Sulphuric acid is a material which contains such entrapped atmospheric oxygen. The process of making sulphuric acid consists in allowing sulphur to burn in the air. It is burnt into sulphurous acid, which contains less oxygen than the sulphuric acid, and then this sulphurous acid is made by a more complex process to take up more oxygen from the air, and to become sulphuric acid. It then consists of one part of hydrogen, four of oxygen, and one of sulphur. Now, if we pass this mono-hydrated sulphuric acid, as we term it, through a red hot tube, it is resolved into sulphurous acid, water, and free oxygen. It splits up; it is resolved into sulphurous acid [representing the compound and the changes by means of the cubical blocks]; free oxygen; and into this body, which you see possesses the composition of water. Now, the sulphurous acid is readily removed from the oxygen by passing the mixed gases through water. This is the process which has recently been proposed to be used commercially for the purpose of obtaining oxygen on a larger scale for illuminating purposes, to which I shall have presently to refer. It is one which is said to yield it at a cheaper rate than any which has before been devised.

Let us now just glance for a moment at the mode by which we are here collecting the gas. You see we first fill this vessel [a gas jar] with water, and then allow the gas to bubble up. As it rises the water is forced down. If we were to pass the gas simply into one of these vessels filled with air, we could not in this sharp and definite manner compel the oxygen to displace the air. The gas and the air would mix together even more rapidly than spirit and water, so it would not be possible to get it pure. Sometimes a gas, unlike oxygen, is absorbed by water, and then we have to substitute for this liquid, metallic mercury. In addition to this method for collecting small quantities we have forms of apparatus for collecting it in larger quantities. This [referring to a sectional drawing of the ordinary collecting apparatus used at gas works], is the common gas-holder, or gasometer as it is sometimes improperly called. It consists of a common bell-shaped vessel floating in a tank of water, and counterbalanced by a chain passing through a pulley which is not shown in

the diagram. Here [pointing to the model of a gasometer] we have an apparatus of this kind on a small scale, fitted up exactly as this in the section. The gas is introduced by one of these tubes, both of which pass down through the water and then up into the bell. The gas is subsequently allowed to pass out at the other tube. The bell is made sufficiently heavy so that it will press the gas out when required for use. Then this is another form of gas-holder. It is more usually employed for experimental purposes than the other. It is called "Pepys's Gas-holder." There you have the actual instrument; and there is the section of it. It consists of two vessels, the upper one open above, and communicating, by means of two tubes, with the lower vessel. The gas is contained in the lower vessel. The upper one is filled with water, and when it is required to transfer this gas into another vessel, it is done by filling the latter vessel with water, and inverting it over this shorter tube, which only descends to the upper surface of this lower vessel. The stopcocks are then opened, and the water flowing down through the long tube creates a pressure by which the gas is driven up through this shorter tube; and thus a vessel of gas can at any moment be very conveniently collected.

This gas, oxygen, is perfectly colourless and transparent, and perfectly free from odour and taste, so that in examining a vessel of it in any of these ways, you would not be able to distinguish it from air. It is, however, heavier: nine cubic feet of oxygen weighing about the same as ten cubic feet of air. Another property which was first pointed out by Professor Faraday is its power of being attracted by the magnet; and I will endeavour to render this visible to you by the apparatus I have placed in front of the table. We have here a large electro-magnet; it is the electro-magnet belonging to the Institution, and is capable of exerting a very enormous magnetic force. Now, over the poles of this magnet we will suspend a vessel—a little collodion balloon filled with oxygen gas. At present we have a balloon filled with atmospheric air suspended over the poles, and I will exactly counterpoise this balloon so that you may see that the magnet has no effect upon it. It is a very delicate experiment, and one that requires considerable freedom from currents of air in order to make it succeed. I want to render the motion of that collodion balloon very visible to those at a distance by means of this index finger and this rough scale upon the paper behind it. You see the contact is now made between the battery and the magnet; and, if you notice, there is a little repulsion, collodion being dia-magnetic. The rod oscillated a little; perhaps it is not very visible at a distance, but you will at all events be convinced that there is no attraction, and that is all the use I wish to make of this first experiment. Now let me replace this balloon filled with air by another filled with oxygen. There is our gas-holder filled with oxygen, and here is our little balloon. [The balloon was then filled with oxygen from the gas-holder.] Here we have our vessel of oxygen, which we will now place in precisely the same position that its fellow containing atmospheric air was placed in before. Let us again get our counterpoise. You see the oxygen is a little heavier than the air, so this experiment will also serve to show you the fact that oxygen gas weighs, bulk for bulk, more than atmospheric air. [The balloon containing the oxygen was then counterpoised.] I think now we shall have the necessary conditions. You see the balloon floats there tolerably constantly at that height above the poles of the magnet. There is no connection now between the battery and the magnet, so that it is perfectly free; but the moment I connect it with the magnet, I cause a current of electricity to flow round that horse-shoe of iron, and we shall then see whether any attraction takes place. If we make the contact, you see that the balloon lowers; if I break the contact, you see it ascends slightly. You see now [establishing the contact] it goes very

decidedly down; it is now down to the glass plate. If I break the contact the weight at the other end of the rod causes it to rise. Now that magnet is no longer in action, you see it rises. So that we have evidence here of this influence of the magnet upon oxygen gas. So considerable is that influence that it has been calculated that the oxygen present in the air is equivalent to a shell of iron around the whole of the globe of $\frac{1}{10}$ th of an inch in thickness. Mr. Faraday has very ingeniously connected this magnetic property of oxygen with the diurnal variation of the magnetic needle. In the day-time, when the air has been heated by the sun's rays, the magnetic effect will decrease. In the night, when the air is cool, that effect increases, because oxygen is more magnetic the colder it is.

Now, among the other properties of oxygen, its power of supporting combustion is one of the most remarkable. Here is an ordinary wax taper, which, if plunged into this gas, burns with greatly-increased brilliancy; and even if the flame be blown out, it will be re-illuminated if there is the slightest glow left upon the wick every time it is placed in the gas. Charcoal, which does not burn very brilliantly in the air, burns very much brighter in an atmosphere of this gas. You see we have only a few angles of the charcoal lighted at the flame of the gas-light; but I dare say we shall soon get it into vivid combustion by introducing it into this gas [the incandescent charcoal was then lowered into a jar of oxygen, and the combustion became very brilliant]. This shows clearly the difference between diluted oxygen, mixed as it is in the air with nitrogen, and this element in its pure and unmixed condition. Substances which burn rather brilliantly in air, burn with almost insupportable brightness in oxygen. Phosphorus burns with some brilliancy in the air, and its brilliancy is so enormously increased in oxygen, that the light, at all events, within a moderate distance, becomes almost insupportable [a piece of burning phosphorus was then introduced into a jar of the gas, the combustion becoming exceedingly brilliant]. If we were to wait a little while for the phosphorus to get into a little more active combustion in the air before introducing it into the gas, you would see that even in the atmosphere it gives a very brilliant light; but look at it now in the oxygen. And especially when it is converted into vapour, and you have the combustion going on through a considerable mass of the gas, then it is that the light attains its highest intensity. Even substances which are incapable of burning in the air without the application of external heat readily undergo combustion in this gas. Here, for instance, is a piece of watch spring, which we should find great difficulty in burning in the air. If we can succeed in lighting it in the oxygen, which is not always easy to accomplish, you will see how readily it burns there. Now, in all these cases you have not failed to remark that, in order to bring into activity the affinities of the oxygen, we had to apply heat. We had to light our taper and our phosphorus, and our last experiment has failed because we could not light our steel-spring. This is not the case, however, with all substances. Here is a substance which is capable of burning in the oxygen at the common temperature, and even in the air. This colourless liquid I have in this tube, the moment it is released, burns in the air [releases it from the tube, combustion ensuing accordingly]. That was a compound of the element zinc with a substance called ethyl. This [exhibiting another liquid] is a compound of boron with the same organic substance, ethyl. You see it burns with a green flame.

Although we have many substances that burn with flame—with the evolution of heat and light, we have also many that undergo slow combination with this element, oxygen, with less visible action. Iron, for instance, and cotton-waste which has been imbued with oil, undergo combustion in the atmosphere, and produce

the same amount of heat as if they were burnt at once with the usual phenomena of combustion; only that heat being spread over a great length of time is not so perceptible. If you take that steel-spring which we tried to light—and we will take it once more, because I want to show you the intense heat which is produced when iron oxidises rapidly—I am not sure that we shall succeed better in this case than we did in the other; but it is possible. [The attempt to ignite the piece of watch-spring was this time successful.] Yes, you see now we have got the necessary temperature, and the steel can combine at this temperature with oxygen, which it does with brilliant combustion. Now, if we had thrown that steel-spring into the street, and allowed it to undergo the same process of combustion by rusting, it would have evolved the same amount of heat as it does in this jar, the only difference being that we have the oxidation taking place in this case in a short time, and, consequently, we have the phenomenon of incandescence; in the other case, the oxidation extends over a long period, and the phenomenon of incandescence does not appear. But if very large quantities of iron exposing a great surface are placed together, and oxidation takes place thus slowly, the heat caused is very considerable. We had an instance of this recently in the case of the Rangoon electric cable, which was to be shipped for Rangoon, and which being placed in an immense coil had, in some way, become wet; and, in consequence of the presence of the air and water the iron coating began to oxidize and a great amount of heat was evolved. If there had been only a few yards there would not have been this effect; but here there was a large mass, and the temperature was raised to such a degree that, according to the experimenters, if it did not rise even to ignition it would, probably, have been sufficient to melt the gutta-percha coating; so, that these slow actions of combustion are not to be overlooked.

Now, chemical combination in these and other cases produces compounds possessing certain definite characters which I have attempted to express here amongst our memoranda. "The combination of one equivalent of a metal with one equivalent of oxygen, sulphur, or selenium, invariably produces a compound possessing positive energy,"—a compound which we should arrange on the positive side of our list. "The combination of one equivalent of a metal with two equivalents of oxygen, sulphur, or selenium, generally produces a neutral compound, that is, one possessing, approximately, at least, neither positive nor negative energy. The combination of one equivalent of a metal with three equivalents of oxygen, sulphur, or selenium, produces a compound possessing negative energy. Compounds possessing positive energy are termed *bases*. Compounds possessing negative energy are termed *acids*. A compound produced by the union of one equivalent of an acid with one equivalent of a base is neutral, and is termed a *salt*."

Now, it is interesting to watch the effect of chemical combination, in changing the positive or negative character of a body, through the different phases of the combination. Let us take, for instance, the element manganese which, you see, belonging to the metals, possesses, according to our table there, positive energy. Now, if we unite with one equivalent of manganese one equivalent of oxygen, forming what is termed protoxide of manganese, we produce a body possessed of positive energy. What we term a "base" is produced. If we unite the manganese with two equivalents of oxygen we produce a neutral substance. We are here [illustrating the combination by means of cubes] adding, you see, negative energy to this original positive atom. The first equivalent is not sufficient to neutralize the positive energy, but the second is, and gives us a neutral compound,—the binoxide of manganese. But if we go on still farther adding oxygen, which we can do in the case of the manganese but not in

all cases. If we add a third equivalent, if our theory be correct, we shall get a negative compound, and it will be an acid: it is manganic acid.

Manganese	= + 20	
Oxygen	= - 10	
Protoxide of manganese	= + 10	Basic
Oxygen	= - 10	
Binoxide of manganese	= 0	Neutral
Oxygen	= - 10	
Manganic acid	= - 10	Acid.

Now [explaining the above diagram], suppose our manganese possesses a positive energy, which we will call + 20 [*plus* 20], and the oxygen possesses a negative energy, which we will represent by - 10 [*minus* 10], the addition of an atom of oxygen to an atom of manganese will reduce the positive energy of the manganese to 10, forming a base; then the addition of a second equivalent of oxygen takes the remaining 10 from the positive energy of the compound, and we get a neutral substance; while the addition of another equivalent of oxygen [- 10] gives a decidedly negative energy to the compound, and we have an acid.

In the next Lecture we will consider hydrogen, and the peculiar modes in which it unites with the element we have studied to-day, namely, oxygen.

ROYAL SOCIETY OF EDINBURGH.

Monday, January 21, 1861.

The Very Rev. DEAN RAMSAY, V.P., in the Chair.

THE following communication by Dr. LYON PLAYFAIR, C.B., F.R.S., and J. A. WANKLYN, F.R.S.E., "*On a Method of taking Vapour Densities at Low Temperatures*," was read. The authors refer to Regnault's experiments, which have shown that aqueous vapour in the atmosphere has the same vapour density at ordinary temperatures as aqueous vapour above 100° C.; and they bring forward fresh experiments upon alcohol and ether to show that when mixed with hydrogen these vapours preserve their normal density at 20° or 30° C. below the boiling points of the liquids, and infer generally that vapours, when partially saturating a permanent gas, retain their normal densities at low temperatures.

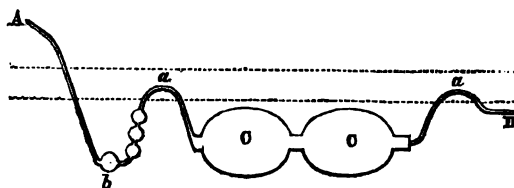
From their researches the authors deduce the consequence—remarkable, but quite in harmony with theory, that permanent gases have the property of rendering vapour truly gaseous. Stated in more precise terms, the proposition maintained by the authors is, "The presence of permanent gas affects a vapour, so that its expansion-coefficient at temperatures near its point of liquefaction tends to approximate to its expansion-coefficient at the highest temperatures."

The authors anticipate that admixture with a permanent gas may serve as a kind of re-agent to distinguish between cases of unusually high expansion-coefficient in a vapour, and cases where chemical alteration takes place. It will also be possible, by the employment of a permanent gas, to obtain vapour-densities of compounds which will not bear boiling without undergoing decomposition.

In experimenting upon substances which may be heated above the boiling point, the authors employ Gay Lussac's process for taking the specific gravity of vapours. A slight modification is, however, necessary. Previous to the introduction of the bulb containing the weighed substance, dry hydrogen is introduced into the graduated tube and measured with all the precautions belonging to a gas analysis. It will be obvious that in the subsequent

calculation the volume of hydrogen corrected at standard temperature and pressure must be subtracted from the volume of mixed gas and vapour, also corrected at standard temperature and pressure.

When the substance will not bear heating to its boiling point, the authors employ a process resembling that of Dumas in principle, but differing very widely from it in detail. Dumas' flask with drawn-out neck is replaced by two bulbs, together of about 300 cub. cent. capacity, joined by a neck, and terminating on either side in a narrow tube. One of the narrow tubes has some very small dilatations blown upon it (*b*), the other is merely bent (*D*). (See the drawing.) The apparatus, whose weight should not exceed 70 grm., is weighed in dry air, then placed in a bath, being secured by a retort-holder grasping the neck joining the large bulbs C and C. The end A, projecting over the one side of the bath, is made to communicate with a hydrogen apparatus; the end D passes through a hole in the opposite side of the bath, which is plugged up water-tight by means of putty. Dry hydrogen is transmitted through the whole arrangement, and escapes at D through a long narrow tube joined to it by a caoutchouc connector.



The bath is next filled with warm water until the bends *a* and *a* are covered. The connection with the hydrogen apparatus is then for a moment interrupted to allow of the introduction of a small quantity of the substance at A. The substance, which should not more than half-fill the small bulb *b*, is partially vaporised in the stream of hydrogen, and in that state passes into the part CC. All the while, the temperature of the bath is kept uniform throughout by constant stirring, and made to rise very slowly. When within a few degrees of the temperature at which the determination is to be made, the current of hydrogen is almost stopped, so that the bulbs C and C may contain less vapour than will fully saturate the gas at the temperature of sealing. The water of the bath is then made to subside, by opening a large tap placed near the bottom. The bends *a* and *a* are thus exposed, the bulbs CC remaining covered. Immediately the current of hydrogen having been stopped, the flame is applied at *aa*, so as to seal the apparatus hermetically. The temperature of the bath, as well as the height of the barometer, must now be observed. After being cleaned, the apparatus (which now consists of three portions, viz., the portion CC hermetically sealed, and the two ends *b* and D) must be weighed.

The capacity of the apparatus is found by filling it completely with water and weighing; but previously to this operation the volume of hydrogen enclosed at the time of sealing must be found. On breaking one extremity under water, the water will rise in the bulbs, and, after a while, will have absorbed all of the vapour, but will leave the hydrogen. The bulbs must then be lifted out of the water, without altering their temperature, and, with the water that has entered, weighed. The difference between the latter weighing and the weight of the bulbs quite full of water gives the weight in grammes, which expresses in cubic centimetres the volume of hydrogen enclosed; the pressure is the height of the barometer, minus the column of water which had entered the bulbs; the temperature is that of the water.

An example of a determination of the vapour density of alcohol at 30° C. below its boiling point is subjoined:—

Height of the barometer (at 0° C.)	. 763.09	m.m.
Temperature of the balance case	. 7.5°	C.
Weight of apparatus in dry air	. 69.959	grm.
Temperature at time of sealing	. 48°	C.
Weight of apparatus + hydrogen + vapour	. 69.5275	grm.
Weight of apparatus + water (at 5.2° C.)	. 191.76	grm.
Weight of apparatus filled with water	. 545.36	grm.
Height of water column	. 122	m.m.

From which is deduced :—

Volumes corrected at 0° C. and 760 m.m. pressure. Cubic centimetres.		
Hydrogen + vapour	406.43	weighing 0.1695
Hydrogen,	341.27	„ 0.0306
	65.16	0.1389

Therefore, 65.16 cub. c. of alcohol vapour weigh .1389 grm.
but 65.16 cub. c. of air weigh .0843 grm.

$$\text{Vapour density of alcohol} = \frac{.1389}{.0843} = 1.648$$

The authors have extended their experiments to acetic acid and other substances. At low temperatures the vapour-density of acetic acid approximates to 4.00, no matter how much hydrogen be employed. At higher temperatures, an approximation to 2.00 is obtained, but without heating so high as Cahours found necessary.

The authors are continuing these researches.

SOCIETY OF ARTS.

Wednesday, January 30, 1861.

W. H. BODKIN, Esq., Assistant Judge for Middlesex,
Vice-President, in the Chair.

(Continued from page 108.)

Dr. NORMANDY could not state what was done with the tea in China, but he could, as an eye-witness, give them some information as to the treatment it received in this country, at the hands of large, and so-called respectable dealers. They were aware that the tea came to this country in cubical boxes, enclosed in a thin sheet of lead, which was soldered in a manner so exquisitely beautiful, that no European plumber could imitate it. In the event, therefore, of that metallic sheet being ruptured, it could not be re-soldered without immediate detection. The practice, therefore, was to cut a hole in the metallic sheet sufficiently large to admit the introduction of the hand to take from the chest a sample from which the dealer decided as to his purchase. The tea having been delivered in the chests to the purchaser, then came his own process of sophistication. If the seam in the metallic sheet could be imitated or re-produced, the task would be an easy one, but as that could not be done, the chests were emptied of their contents through the hole to which he had alluded, and the tea was poured out in a room appropriated to the purpose, the floor of which was kept very smooth and clean. A mixture of various descriptions of tea was then made in a heap, turned over with wooden shovels, and the chests were re-filled through the same hole; the compression of the tea in the chest being effected by the hand in the first instance, and afterwards by the foot. In that manner the chests were re-packed with a different article from that which they originally contained, and they were then ready for sale by the dealer. He begged to state that he had seen the process carried on in one of the largest tea houses of London, and he would therefore vouch for the correctness of his statement. With respect to bread, he was in the habit of analysing many hundred samples every year, and he could say that, at one time, he never met with bread entirely free from alum. It was

not his purpose now to discuss the question whether alum so administered was injurious to the human system or not. The question was whether alum was actually present in bread or not. He had been instrumental in convicting about a hundred bakers for adulteration of bread, and he might state that, of the cases so brought forward, the largest proportion pleaded guilty to the charge. He was happy to say that his experiments in this matter showed that a decided improvement had lately taken place in the general quality of the bread submitted to him, inasmuch as out of twenty-three samples of bread sent to him, at the instance of the magistrates of Wandsworth, only nine were found to contain alum, so that it would be seen that they were improving, and it was to be hoped that legislation had done something towards mitigating the evil.

Mr. T. A. MALONE wished to set himself right with respect to what he had stated at the last meeting as to the adulteration of tea with the substance known as *Valonia*. Professor Bentley having questioned that assertion, he (Mr. Malone) would now state that a piece of *valonia* was put into his hand by the son of a large tea merchant, who told him that it was a substance used in the sophistication of tea; and it was upon that authority (which he considered a good one) he made the statement at the last meeting. At the same time, he laid no claim to having discovered any ready means by which the presence of that substance in tea could be detected. He had been told by a gentleman present, acquainted with the subject, that *valonia* could be used in such a manner as to deceive the most experienced judges of tea; in fact, he was inclined to doubt whether chemical appliances were sufficient to detect that kind of sophistication. It had been said that there was equal difficulty in detecting some methods of adulterating tobacco; and he had heard it said of an eminent analyst that he was able to adulterate tobacco in such a perfect manner that he could not afterwards detect his own work; and he believed that remark applied in a great measure to the sophistication of tea. The injurious effects of tea upon some constitutions were attributed to the astringent properties it contained. He believed those properties could be modified by the use of gelatine; and, in his own experience, he had rendered tea more agreeable to his palate by the introduction of a few strips of isinglass into the infusion. The general opinion, he believed, was that the principle in tea which was known as *Theine* acted unfavourably upon the nervous system of some persons, and the amount of that principle (which could be detected by treatment with benzol) might form some test of its action upon the human system.

Dr. RIDDELL remarked that the question of tea had been more or less discussed during the last ten years. His own experience had been principally confined to such articles as pickles and sauces. Soy was an article which contained very nutritious properties. It contained a larger quantity of nitrogen than any other kind of food used by the Chinese, and it appeared they had discovered that to be the case, and therefore used it in large quantities. People frequently now would see, in the shop windows of London, a peculiar-shaped jar, labelled "Japanese Soy," but that jar contained a cork, and if the article was what it purported to be, the wonder was how the cork got into the bottle, considering the Japanese did not use corks. A very favourite condiment in this country was mushroom ketchup, and when they purchased it, the shopkeeper could do no more than tell them that he bought it for genuine, and he hoped it was so. Very good "mushroom" ketchup was, however, said to be made from horse's liver; and a lady in India had told him she always made her "mushroom" ketchup from calves' liver. Some remarks had been made with regard to curry powder, and it was stated that it could be made without turmeric. He believed he could put his hand upon 400 different receipts for making curry powder, written in Persian, so that it would be difficult to make a universal curry powder. With regard to

pickles, he believed very little East Indian pickle was sent to this country. Girkins certainly would not be found in India. The West Indian pickles consisted chiefly of mangoes. With regard to cayenne pepper, the public taste was in favour of a red-coloured article, although it was difficult to conceive how a genuine pepper of that colour could be furnished from a dark-brown or yellow chili.

The CHAIRMAN said he had listened with satisfaction both to the paper and to the discussion upon it. It had been truly stated that the object of this Society was, upon every subject that it touched—as far as human means could do so—to promote and elicit truth; but it would be quite understood that the Society did not hold itself responsible for the views that were advanced in the papers brought before these meetings, or for the facts therein stated. It was hardly possible to conceive that a subject of this kind, arraying on the one side charges against the commercial classes of the country, and on the other side persons who naturally desired to screen themselves from the imputation of supplying unwholesome food—that a discussion of this kind would not call forth a little temper; he was glad to see it had not been carried further. The probability was that on one side there might be an exaggeration of facts, and on the other side an over anxious zeal to defend interests of which these persons were in some degree the representatives. Under these circumstances, perhaps they might look for the truth between the two extremes. There was one subject to which he would more particularly allude. With regard to the public health, he thought the statistical returns showed that to whatever degree the adulteration of food was indulged in, the value of human life had in modern times greatly increased. That was a strong fact, but there were other circumstances to be regarded, which, he thought, would not afford equally satisfactory results. During the last year he had been a visitor of one of the largest lunatic asylums in the world, where there were now 1800 and 1900 lunatics in one building, and the great increase of lunacy in this country had attracted the attention of everyone who had considered the subject. It was undoubtedly the opinion of the most eminent physiologists, and men conversant with the human frame, and the action of food upon it, that some extent, if not a considerable portion of the increase of this malady, might be traced to the effects upon the nervous system of deleterious substances, which it was to be feared were but too commonly mixed with articles of food supplied more especially to the poorer classes of the community. He thought, with respect to the paper, nothing had transpired which ought to deprive the author of it of the usual compliment which was paid upon these occasions. He would therefore take the sense of the meeting upon the proposition that a vote of thanks be given to Mr. Wentworth Scott for his paper.

A vote of thanks was then passed to Mr. Scott.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, February 5, 1861.

J. C. DYER, Esq., *Vice-President, in the Chair.*

Mr. JOHN CURTIS communicated his observations of the fall of rain in the years 1860, 1859, and 1858, and compared them with Dr. Dalton's average for 47 years, and with the mean of the last 72 and 75 years. The following is a summary of the results:—

Average from 1794 to 1860	35.46 inches.
“ from 1786 to 1857	36.398 “
“ from 1786 to 1860	36.274 “
Dalton's from 1794 to 1840	35.523 “
“ In 1858	30.53 “
“ 1859	33.09 “
“ 1860	36.24 “

Comparing the last year with 1859 it was found that the No. of rainy days in 1860 were 28 days above that in 1859.

Amt. of evaporatn.	“ was 0.92 in. below “
Mean temperature	“ was 3.13° below “
Hghst. tem. in shade	“ was 9.7° below “
Lwst. tem. in shade	“ was 19.0° below “
Hghst. tem. in sun	“ was 6.0° below “
Lwst. tem. on grass	“ was 19.9° below “
Mean of barometer	“ was 0.031 in. below “
No. of days of snow	“ were 8 more than “

Dr. CRACE CALVERT stated that in consequence of having found lead in snuff packed in leaden cases, he had examined tea, chicory, &c., but without discovering lead in them, which he attributed to the protection afforded, in some instances, by the interposition of paper between the article and the leaden case, and in others to the absence of sufficient moisture to promote chemical action.

One of the medals struck on the occasion of the coronation of the present King and Queen of Sweden, presented to the Society by the University of Christiana, excited much admiration on account of the excellence of metallurgy it displayed.

A Paper “*On the Kaloscope*,” by Mr. W. H. HEYS, was read by Mr. GEORGE MOSLEY.—This Paper was communicated by the Microscopical Section, and an abstract will be found in the CHEMICAL NEWS, No. 62, under that head.

PHYSICAL AND MATHEMATICAL SECTION.

January 31, 1861.

Mr. MOSLEY read from the *Gibraltar Chronicle* of the 8th January, an abstract of meteorological observations taken at the Royal Engineers' Observatory, Gibraltar, during the year 1860. From the results given it appeared that while the weather in England during the last summer and autumn had been unseasonably cold and wet, at Gibraltar, on the contrary, it had been remarkably warm and dry. During the six months from the 30th of April to the 1st November, the fall of rain at Gibraltar had been only 1.237 inches, whilst at Manchester, according to Mr. Vernon's returns, it had been 21.858 inches. It was also remarkable that the maximum degree of humidity of the air at Gibraltar, as determined in the usual way by the wet and dry bulb thermometers, occurred on the 30th of September during the long period of drought. The average fall of rain at Gibraltar, from eight years' observations, is 41.2 inches, but during 1860 the amount collected in a gauge on the ground was only 34.874 inches, and in a gauge 25 feet above the ground 32.358 inches. The mean pressure of the atmosphere for the year, at 50 feet above mean water level, was 30.001 inches; the mean temperature, 64.09; and the mean dew point computed was 57.04.

Mr. BAXENDELL communicated the following table of the fall of rain at the Flish, Cleator, near Whitehaven, during the last three years, drawn up from observations made by Thomas Ainsworth, Esq., a Corresponding Member of the Society:—

	1858.	1859.	1860.
January	3.495	5.415	7.672
February	0.465	4.477	2.647
March	3.665	4.507	5.030
April	2.575	4.017	2.527
May	3.855	0.400	3.660
June	2.235	2.585	4.622
July	4.000	2.937	2.222
August	3.157	6.332	6.650
September	5.667	6.247	2.030
October	6.610	4.325	9.122
November	1.400	4.737	2.012
December	4.665	4.120	5.610
Total	41.789	50.099	53.804

Mr. BAXENDALL, referring to a letter by Dr. Wolf, of Zurich, inserted in No. 1289 of the *Astronomische Nachrichten*, on the variations in the frequency of the solar spots, exhibited a diagram showing such frequency by the relative lengths of ordinates laid off from a line representing the time. Treating Dr. Wolf's data by the method of least squares, Mr. Baxendall had deduced a mean period of 11·086 years, the mean epoch of minimum frequency being 1732·96.

Mr. T. HÆELIS read a Paper "*On Meteorological Observations, and Observations of the Temperature of the Atlantic Ocean, made on runs from Liverpool to Gibraltar, and from Gibraltar to Liverpool, in September, 1860.*"

This Paper was considered by the Section worthy of being printed in the Memoirs, and will be read at an Ordinary Meeting of the Society.

In the course of a discussion which ensued on the red colour of the sky, often observed as an indication of approaching bad weather, Mr. BINNEY mentioned that Mr. Dancer had shown him an aurora borealis of a pale greenish white colour, which when observed through glass upon which moisture had been deposited, appeared red.

Mr. BAXENDALL mentioned that several fogs, especially those which had occurred in the early part of the present winter, had been observed by him to be luminous, and that Mr. Crosse, the electrician, had found many fogs to be highly electrical.

CHEMICAL SOCIETY.

Thursday, February 7, 1861.

Professor BRODIE, F.R.S., President, in the Chair.

Messrs. C. E. Beadnell and J. Davis were elected Fellows, and Messrs. E. L. Barret and C. Ulrich, Associates.

Professor FIELD read a Paper "*On the Carbonates of Copper, Cobalt, and Nickel.*" In a former communication the author had observed that when the solution of a copper salt was added to an excess of solution of bicarbonate of soda, and the resulting blue liquid boiled for some time, he obtained a green precipitate of dicarbonate of copper or malachite; that where sulphate of copper was added to a strong solution of carbonate of soda, and the mixture boiled, a black precipitate of a highly basic carbonate of copper was formed; and the same compound resulted from adding natural or artificial dicarbonate of copper to a boiling solution of carbonate of soda; and that by boiling the basic carbonates, green or black, for a few minutes in water, or for a very lengthened period in carbonate of soda solution, they become converted into oxide of copper. The author now showed that when azurite is boiled with water or solution of carbonate of soda, it undergoes the same reactions, being converted into oxide of copper, rapidly in the one case, slowly in the other. Azurite dissolves in a hot concentrated solution of bicarbonate of soda, and the liquid, by prolonged ebullition, yields a precipitate of malachite. When solution of nitrate of cobalt is added to excess of bicarbonate of soda, no precipitate is produced until a considerable quantity of the cobalt has been introduced. The solution has a beautiful violet colour, and may be boiled for some time without decomposition. When a larger proportion of cobalt solution is added to the bicarbonate, a pink coloured precipitate, scarcely affected by boiling, is produced. This compound retains carbonate of soda with great tenacity. On boiling the washed precipitate in water it becomes brown. The precipitate formed by adding nitrate of cobalt to excess of carbonate of soda is blue, instead of pink. Like the pink compound it was tolerably permanent, and could not be freed from carbonate of soda. The washed precipitate produced by the reaction of carbonate of soda and nitrate of cobalt, was boiled for

ten hours in water, whereby it became brown, and eventually black. This compound proved to be a mixture of ordinary carbonate, soluble in dilute hydrochloric acid with evolution of carbonic acid, and sesquioxide of cobalt, soluble in strong hydrochloric acid with evolution of chlorine. When nitrate of cobalt is added to bicarbonate of soda containing a little hypochlorite of soda, a bright green liquid of great tinctorial power is produced. This liquid, if not too concentrated, may be boiled for some time without decomposition. Nickel salts exhibit with the carbonates of soda much the same reactions as cobalt salts. The carbonates of nickel, like those of cobalt, cannot be completely converted into oxide; but unlike those of cobalt, do not yield any peroxide by boiling with water.

Mr. A. H. CHURCH read a Paper "*On the Oxidation of Benzol.*" Free benzol, as is well known, is practically inoxidisable; but the compound known as sulphobenzolic acid, made by dissolving benzol in strong sulphuric acid, proved to be readily oxidisable by means of bichromate of potash and sulphuric acid, into an acid having the formula $C_{12}H_4O_4$. Nitro-benzol, by the same mode of oxidation, yielded the nitro-acid $C_{12}H_3(NO_2)O_4$. The new acid belongs to the benzoic series of homologous acids, and stands immediately above benzoic acid. Toluol compounds similarly treated yield benzoic and nitro-benzoic acids. The higher hydrocarbons of the series also yielded benzoic acid. The author found that ordinary oil of vitriol diluted with about one-eighth part of its bulk of water, although without action on benzol, dissolved toluol and the higher hydrocarbons from coal-tar oil, and this liquid by treatment with bichromate of potash furnished benzoic acid in quantity.

NOTICES OF PATENTS.

Improvements in the Treatment of Madder Roots, Munjeet, or other Plants of a similar Class. By EDWARD MUCKLOW, of Bury, Lancashire.

THIS invention consists (according to the Patentee) in cleansing the madder roots, &c., more effectually than has hitherto been done, from certain impurities which injuriously affect their dyeing properties. For this purpose the roots, while in the moist state, are subjected to hydraulic pressure to remove the juices contained in them. It is the substances dissolved in these juices that the patentee considers to be injurious. If the roots have been dried before undergoing purification, the patentee moistens them before subjecting them to pressure. After removal from the press, the roots are to be dried and ground; they then constitute "refined madder."

The advantages attributed to this process by the inventor are that, when goods are dyed with madder so prepared, 1st, the whites turn out clearer; 2nd, that less soap and alkalies will be required for cleaning in the Turkey red process.

The Extraction of a certain Colouring Matter from Rags and other Waste Vegetable Textile Fabrics containing the same. By JULES ALBERT HARTMANN, of Mulhouse. (Provisional Protection only.)

THE patentee's idea is to recover the alizarine from madder-dyed goods, preferably from those which are to be used for paper-making; so that the colouring matter may again be employed for dyeing and printing.

The following process is said to answer well. The rags, &c., are first treated with water slightly acidulated with muriatic acid, for the purpose of removing the metallic oxides which have been used for mordanting. The colouring matter is to be separated from the goods by means of

a boiling solution of alum, or an alkali. It is to be separated, subsequently, by precipitation or evaporation.

The Patentee appears to consider alizarine to be the only colouring matter concerned in madder dyeing, inasmuch as he confines himself to the extraction of that substance. Practical dyers are, however, aware that madder colours cannot be procured in perfection from alizarine alone. We have no confidence in the process partly from the circumstance alluded to above, and partly because it is evident that the provisional specification was taken out before the Patentee had sufficiently matured his mode of operating.

Improvements in the Manufacture of Colouring Matters. By FREDERICK GRACE CALVERT, CHARLES LOWE, and SAMUEL CLIFT, Manchester.

THIS curious patent may be divided into two principal parts, one being the production of a green colouring matter from aniline and its homologues, and the other being the conversion of the green colour into a blue. The green they call Emeraldine, and the blue Azurine. The chemists who devote themselves to colouring matters are said to be at their wits' end for names.

The Patentees do not appear to have succeeded in isolating their new colouring matters, for it is specially stated that they are obtained by oxidation in direct contact with the yarn, &c.

In preparing the green colour, they first impregnate the goods with an oxidising agent. They recommend a solution of chlorate of potash (4 oz. to the gallon), the goods, after steeping, are to be dried and then padded or printed with an acid salt of the base. They prefer a solution of tartrate or hydrochlorate of aniline containing one per cent. of the alkaloid. After the padding or printing, the goods are aged for twelve hours, that time being sufficient for the complete development of the colour.

They also prepare the green by the following modification of the first process:—They mix the oxidising agent with the salt of aniline, and print on both together, of course thickening with starch or flour in the usual manner. They recommend the following mixture:—

"Solution of an acid salt of aniline (containing 1 lb. of aniline).	
Tartrate or chloride of aniline	3 lbs.
Starch or flour paste	60 lbs.
Chlorate of potash	1 lb.

The chlorate of potash must be dissolved in the starch-paste whilst hot, and the solution of the acid salt of aniline we add to it after cooling."

We quote the above literally because it appears to us rather vague. If we understand the directions, they mean that three pounds of a solution of tartrate or "chloride" of aniline are to be taken, which three pounds of solution are to contain one pound of aniline.

The green colour, as produced by the above method, is to be converted into a blue or purple by boiling in a weak solution of soap or alkali, the goods are then to be dried. The soap solution should contain four ounces of printers' soap to the gallon; and the alkaline bath one ounce of caustic soda to the gallon.

The patentees say that instead of the alkaline or soap bath, the goods may be passed through a solution containing one ounce of chromate or bichromate of potash to the gallon of water.

The above colours do not require any mordant.

This patent appears to us to be of great interest. We know that efforts have, for a long time past, been made to apply aniline directly to fabrics, and convert it into colouring matter in the fibre. Some of these efforts have led to disappointment. If yarn, impregnated with a salt of aniline, be passed into a solution of a chromate or bichromate, it immediately assumes a dirty green colour, which acquires a certain amount of purple tone by treatment with soap. If the aniline solution be strong the

goods become nearly black in the chromate bath. We are curious to see some of the colour produced by the above patent process.

CORRESPONDENCE.

The Adulteration of Food.

To the Editor of the CHEMICAL NEWS.

SIR,—It is to be hoped that the present discussion of the question of food adulteration will clear away many of the erroneous ideas now existing on this subject, which present such formidable obstructions to the establishment of any system, whereby the laws regulating the sale of provisions may be placed upon a satisfactory basis. It must, at all events, throw some light upon the chaos of conflicting statements which are now before the public, and thus accomplish something towards the elucidation of truth. It would be as reasonable in a sick man to hope for a cure from the prescriptions of physicians who cannot agree as to the diagnosis of his disease, as to expect that we shall derive any benefit from legislative enactments against adulteration, so long as those who are looked up to for instruction on this question, are so widely at variance amongst themselves as to the nature and extent of the evil against which such enactments are made. Let me select a few of the statements which have been published within the last three weeks.

Respecting the adulteration of Tea, Mr. W. L. Scott informs us "That of black teas, about 61 per cent. would be found adulterated, or more or less impure. That of green teas the per-centage of adulterated samples would be approximately 78 per cent. Mr. W. J. Bland says, "That so far from seven-eighths of the tea which came over to this country being adulterated, not one-eighth of our annual imports of that article was adulterated." Mr. William Green says, "That the statement that seven eighths of the tea imported is adulterated, is rather below the fact." Mr. W. G. Reynolds is "quite certain that but a very small proportion of the teas imported into this country are in any way adulterated; that tea cannot be adulterated with impunity and profit."

Respecting Bread Mr. Scott states "that very few samples are uncontaminated with alum." And in reference to a doubt expressed as to the correctness of the assertion that 87 per cent. of the bread of London was adulterated, he says that "if anything, it was rather under the mark;" and adds that of 1000 loaves purchased of as many different bakers in London, fully one-fourth of the total number would be found repulsive to an ordinarily discriminating palate. That sulphate of copper is used by Belgian and Austrian bakers." Mr. Wersmann informs us "that out of sixty-four samples of bread obtained from various shops in Whitechapel, and examined by Dr. Odling, not a single one was found to contain alum;" that "he has tested many samples of bread at Prague, for sulphate of copper, but not in a single instance could he obtain a trace of it." That "one of the largest bakers in Vienna assured him that its use is utterly unknown in Austria."

Milk, Mr. Scott tells us, is adulterated with "annatto, turmeric, gamboge, starch, mucilage, or, rarely, with the brains of some animal." Whilst Dr. Wyld tells us that "water is almost the only adulteration of milk."

Now, it is quite evident that some of these statements must be "entirely false or gross misrepresentations," and, inasmuch as the *onus probandi* necessarily rests with those who assert the existence of the adulterations, I maintain that, unless Analysts support the publication of important results obtained in investigations upon questionable points by sufficient details, their conclusions are not entitled to our confidence.

In the remainder of your report of Mr. Scott's paper, published in last Saturday's Journal, I find the following:—

"On one point, relating to beer, I will make a single observation,—viz., that I have actually detected strychnine in two samples of bitter beer."

Now, I think we might at least have expected that, after the causeless alarm created in 1852 about strychnine in bitter beer, and after the thorough investigation of the subject by Professors Graham and Hofmann, that a chemist of less established reputation, in making an assertion calculated to raise a repetition of the outcry, would, so far from confining himself to "a single observation," have prefaced so important an announcement by some allusion to prior investigations, and have furnished such particulars of the case as might have led to the corroboration of a fact so opposite in its character to the results hitherto obtained.

It is not a little curious to observe how soon a bold statement of any kind draws into its train others of a still more startling character. Even Mr. Scott's surprising announcements are already surpassed. Mr. Horsley states that he has found copper in samples of flour and bread, purchased in the locality of Cheltenham. One would have imagined that a town in which the working-classes form less than the usual proportion of the population, the favourite resort of invalids, and of those who have retired from the active business of life, where quality, not quantity, commands custom, would have been one of the last in the kingdom that an adulterator of flour would have selected for his business. Perhaps Mr. Horsley will inform us what was the actual quantity of copper found, and what steps he has taken towards securing the punishment of the offenders. Such a discovery should not be reserved for a scientific journal; but, if correct, should be published through the length and breadth of the land, with the names of the parties guilty of such practices.

Well may the Editor of the *Pharmaceutical Journal* say, in speaking of the Adulteration Act "the duties required to be performed by the Analysts are of a very responsible nature, and involve special qualifications. Where are the men to fill them? And he proceeds to say that there is a class of men who endeavour to make the agitation of this subject a source of profit, who have been seeking about for cases, construing everything that does not come up to their assumed standard, into a case of adulteration, causing unnecessary alarm in the minds of those most susceptible of such influences, creating indignation among those who consider themselves unjustly accused or suspected, and producing a tendency in men of calm judgment to counteract this injudicious agitation. These are not the men to be appointed Public Analysts." We must all regret that there appears so little probability of the recent Act producing any satisfactory results, but I cannot think that such proposals as analysing samples for 5s., issuing to the public boxes "containing suitable tests and apparatus for performing the required analyses at 20s. each," will tend to help us out of our difficulties, or will have any other effect than to plunge us still more deeply in the mire. The knowledge requisite for the examination of the many and varied descriptions of articles used as food, is not acquired in a day, but can only be attained by patient labour and long experience, however brilliant may be our talents, or high our scientific attainments, and it behoves us to take heed lest we add still another to the list of sophistications by encouraging the growth of a spurious Chemistry.—I am, &c.

VERAX.

The Progress of Meteorological Science.

To the Editor of the *CHEMICAL NEWS*.

SIR,—The unusual character of the past year has produced much speculation as to its cause, and thus, perhaps, has meteorology to some extent been advanced. As in every other branch of physical science, all real progress

must in this be founded on observation and experiment; and inasmuch as speculation in any particular is not as a rule conducted with much reference to these requirements, it contributes only in a small degree to scientific advancement.

It has properly been observed, that the influence of the atmospheric vibration, of those tides and waves which are similar to the aqueous tides and waves, and produced by the same cause, is perceptibly experienced in, or produces sensible or apprehensible effects upon, the weather. This fact is one which it would emphatically be well to investigate; and here it is only proper to observe, that, as in all other strictly meteorological questions, individual operations are only of little use. A single observer does not, and cannot, know where to begin or where to end his operations; and this, because not being able to place himself in more situations than one simultaneously, he cannot become perfectly acquainted with the phenomena he is considering, the nature of which—which characterises everything in meteorology,—is to extend over much space, strictly, indeed, the entire universe. The comparative worthlessness of single observations when directed to meteorological questions, is a fact which should be impressed upon the minds of all physical inquirers; and the effect of knowing it should not be to create indifference, but to lead those who are active in the matter to combine with others, as many as possible, that important results may ensue. The only way to bring about the advancement of meteorological science to a considerable degree—a species of knowledge which is almost proverbially slow of increase, that is, the ascertainment of general instead of merely local laws—is to establish a staff of observers in this or any other country, who shall simultaneously make observations as to the heat, moisture, pressure, and electricity of the atmosphere, and the force and direction of the wind, or feeble currents of air. It is clear that the practical data which would be the foundation of future investigations, would increase in value (because in number) with the number of the observers and the extent of country over which the observations were spread. It is of course clear that it is desirable they should be made throughout the world. They would be the foundation upon which speculation or hypothesis could philosophically be exercised, and by them advancement would be made from hypothesis to theory, and from theory to fact or certainty. As regards the force and direction of winds, I would direct the attention of those who are so interested to the description of an anemometer which I have devised, and already (*Mechanics' Magazine*, No. 1812, Old Series) submitted to public attention.

It has not yet, I think, been found that either any of the planets or fixed stars perceptibly affect either the atmosphere or aqueous perturbations. Inasmuch as gravity extends infinitely in every direction, it must be allowed that some effects of this kind, or thus produced, are always existent. Perhaps they are too minute ever to come within the range of human inspection.—I am, &c.

J. ALEXANDER DAVIES.

Copper in Bread.

To the Editor of the *CHEMICAL NEWS*.

SIR,—From Mr. Horsley's letter in the *Chemical News* of February 16th, it appears that he has occasionally discovered sulphate of copper in flour and bread.

It is of importance to trace the cause of this sophistication. It has lately become common for farmers to soak their grain in solutions of sulphate of copper previously to sowing, in order to preserve it from disease, and I know that they generally employ the cheaper qualities of that salt, which almost invariably contain iron.

It is for a more experienced man than myself to say if the adulteration complained of arises from this cause or not.—I am, &c.

A. P.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Preparation of Pure Caustic Potash.—Franz Schulze gives the following process in the *Chemisches Centralblatt*, No. 1, 1861, p. 5:—Fill a copper crucible with a mixture of pure saltpetre and three times its weight of pure peroxide of iron made from the oxalate. Carry a copper tube through a hole in the crucible-cover down to the bottom of the vessel, and, while the crucible is kept heated to low redness, pass a current of pure dry hydrogen through the tube. The nitric acid of the saltpetre is very soon decomposed, a part of its nitrogen being converted into ammonia. When the experiment is ended, the potash is washed out of its mixture with the oxide of iron. The excess of oxide of iron is required to give the mixture the necessary looseness to allow the hydrogen to pass freely. The oxide of iron can be used over and over again. The hydrogen makes this process rather expensive, five atoms at least being consumed for one atom of potash. The author calculates that 1 lb. of hydrate of potash may be prepared from 1.8 lb. of the nitrate by the consumption of 2.85 lbs. of zinc and 4.35 lbs. of strong sulphuric acid. He has sometimes, however, used twice this weight of the last article. The process is ingenious, but certainly not cheap.

II. ORGANIC CHEMISTRY.

Proximate Composition of some Plants.—G. F. Walz has found (*Neues Jahrbuch für Pharmacie*, bd. xiii. s. 288) in the leaves of the *Globularia Alypum* (a bitter, drastic purgative herb) tannin, yellow colouring-matter, an active principle, to which he gives the name Globularin, Globularetin, and Paraglobularetin. The globularin, it seems, is a glucoside, having the composition $C_{60}H_{44}O_{28}$, which may be split up into sugar, globularetin, paraglobularetin, and water: $C_{60}H_{44}O_{28} = C_{12}H_{12}O_{12} + C_{24}H_{14}O_6$ globularetin + $C_{24}H_{14}O_6$ paraglobularetin + $2H_2O$. Walz has also discovered a crystallisable body in the *Paris quadrifolia* (*Ibid.*, s. 355), a narcotic herb, of the order *Tyrlilaceæ*. This also is a glucoside, which the author has named *Paridin*. It has the composition $C_{64}H_{56}O_{28}$. By boiling with dilute alcohol and hydrochloric acid it splits up into *Paridinol*, $C_{52}H_{44}O_{16}$, and sugar, which loses two atoms of water. The mother liquor from which the paridin has crystallised out still contains a bitter principle, which is precipitated on the addition of solution of tannin. This body exists in a combination of tannin with a new bitter principle, which the author names *Parastephnin*. It is separated by precipitating the tannin with lead oxide, and it is then purified from paridin by repeated solution and evaporations. *Parastephnin* has the composition $C_{76}H_{64}O_{36}$, and by boiling with sulphuric acid it is split up into paridin and sugar: $C_{64}H_{56}O_{28}$ Paridin + $C_{12}H_{12}O_{12} = C_{76}H_{64}O_{36} + 4H_2O$.

III. ANALYTICAL CHEMISTRY.

Test for Sulphide of Carbon in Coal Gas.—Dr. C. Herzog communicates the following to *Chem. Centralblatt*, No. 1, 1861, p. 1:—Prepare a saturated solution of ammonia in absolute alcohol and a perfectly saturated solution of sugar of lead. Place in a test-tube five drops of the lead solution and about a drachm of the alcoholic ammonia, and allow the gas to bubble through the solution from a narrow glass tube just dipping under the surface. If sulphide of carbon be present in the gas the solution immediately takes an orange colour, and, after a time, a deep-brown precipitate falls. If carbonic acid be present as well a white precipitate also is produced, which gives a brighter tinge to the orange colour. For a controlling experiment, the gas may be passed for a short time through the alcoholic ammonia alone, and a couple of drops of the lead solution added afterwards, whereupon, if sulphide of carbon be present, the orange precipitate is produced as before. To free the gas from

sulphuretted hydrogen, it may be first passed through a lead solution, which does not affect the sulphide of carbon. The author remarks that the orange precipitate obtained as above, if allowed to remain in the liquid, turns white in twenty-four hours, but if collected on a filter immediately, washed a little, and then dried, it remains of a dark-brown colour. The chemical changes which take place when sulphide of carbon is passed into the alcoholic ammonia are, according to the author, rather complex and somewhat variable, but he recommends the test as very simple and practical.

MISCELLANEOUS.

Royal Institution.—The following Lectures will be delivered during the ensuing week:—Tuesday, February 26, at 3 o'clock, Professor Owen, "On Fishes." Thursday, February 28, at 3 o'clock, Professor Tyndall, "On Electricity." Friday, March 1, at 8 o'clock, Professor H. E. Roscoe, "On Bunsen and Kirchhoff's Spectrum Observations." Saturday, March 2, at 3 o'clock, Dr. E. Frankland, "On Inorganic Chemistry."

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the *CHEMICAL NEWS*, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 6d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part I. of the present Volume is now ready.

*. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

*. All Editorial Communications are to be addressed to Mr. CROOKES, and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the *CHEMICAL NEWS*, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

Z.—Our correspondent's letter will be attended to.

W. E. B.—Asbestos is a silicate of magnesia, containing lime and iron and sometimes magnesia, manganese, and alumina as impurities.

J. Wallace.—The best plan to find the amount of iron in a solution of the acetate will be to evaporate a measured portion (say one ounce) to dryness after addition of excess of nitric acid, and then to ignite the residue in a platinum crucible, keeping the temperature at full redness for five minutes after fumes ceased to be evolved. Weigh the residue, which will be sesquioxide of iron: seven-tenths of this weight will be metallic iron.

Cleaning Tarnished Silver Goods.—A. B. C. will find the following plan very successful in removing tarnish, grease, and other objectionable matter, from silver and plated goods:—Make a solution of one part cyanide of potassium in six parts of water, dip a soft hog's-hair brush into the liquid and gently brush it over the edges, handles, and feet of mounted work—such as crust-frames, candle-sticks, &c.; then rinse the article in water; now dip a piece of flannel in the solution and pass it lightly over the polished surfaces so as not to scratch them. The cyanide will at once remove the tarnish, when the article must be well rinsed: now dry it and apply a little powdered rotten-stone in the usual way, finishing with rouge. Gift or gold goods may be cleaned by dipping them for an instant into the strong solution, and then well washing them with hot soap and water, applied with a soft brush.

THE CHEMICAL NEWS

VOL. III. No. 65.—March 2, 1861.

THE TORBANEHILL MINERAL.

ASSUREDLY Science in the lecture-theatre and Science in the witness-box are two distinct beings. Of all sciences, perhaps Chemistry has, hitherto, cut the poorest figure in courts of justice. The men most distinguished in their several departments are called upon to swear to what appears a simple matter of fact, "Is the mineral dug on the lands of Torbanehill a coal or not?" And we find one-half of the chemists answering in the affirmative and the other half in the negative. On this question vast sums of money depend. A chemist patents a process for distilling coal at a low temperature, with the view of obtaining paraffine oil; but when his process is worked, it turns out that the only substance capable of being employed economically is the Torbanehill mineral, or shale. It is therefore essential to the maintenance of his patent rights to prove that the substance alluded to is a coal. Almost the whole of the more eminent chemists on the Continent are agreed that it is a shale. Indeed, that opinion is now rapidly gaining ground in this country. The following extract from the *Scotsman* shows the Continental feeling upon the subject in a very strong light:—

"By a new law the Boghead or Torbanehill mineral is now allowed to enter France free of duty, and under the name of solid bitumen or shale stone (*bitume solide ou pierre de schiste*). This information is from M. Jules Friscl, of Marseilles, sworn broker, and is conveyed in a communication dated February 19. The same mineral has for several years been admitted into the States of the Zollverein duty free, on the ground that the mineral, not being coal, is not liable to duty as such. Thus, with all Germany on the east, and France on the west, the Continent may be said to have pronounced upon the nature of the Torbanehill mineral."

We find that the Continental chemists derive their opinion partly from the mineralogical, and partly from the chemical features of the substance in question. It is said, though we are not aware upon what authority, that the Torbanehill mineral, when heated to a temperature considerably below that at which it begins to give off volatile products, becomes plastic, and may be moulded into various forms. If this be true—and the assertion can be easily refuted or proved—it must be admitted that an immense point has been gained by the advocates for its not being a coal. The fact, we believe, cannot be disputed, that the naphtha produced by distilling the mineral—even at the high temperature required for making gas—is very different in density, and chemical characters generally, from coal naphtha, and is sufficiently remarkable from containing about thirty per cent. of hydrocarbons identical or isomeric with the radicals of the alcohols.

The question is one of the greatest chemical and commercial importance, and it is, we conceive, necessary to ascertain the sense of the chemists of this country again upon the subject, so as to settle at once whether

or not we are of the same opinion as our brethren upon the Continent, and, as the Chancery pleadings say, "If not, why not."

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Supposed New Member of the Calcium Group of Metals, by WILLIAM CROOKES.

SINCE the announcement by Messrs. F. W. and A. Dupré¹ of the existence of a fourth member of the calcium group of metals, which they state they have detected by means of the spectrum, I have searched various private memoranda on the subject of the spectra given by artificially-coloured flames, which I have been investigating at intervals during the past eight years, and have found recorded that the calcium spectrum occasionally yielded me a blue line, not due to potassium or strontium, and in the position indicated by these gentlemen as belonging to their supposed new metal. I have, therefore, re-examined specimens of lime-salts with a more perfect spectrum apparatus, and find that all of them give me a blue line, situated, as described by the Messrs. Dupré, "between the lines Sr³ and K³, about twice as far from the former as from the latter. In brightness and sharpness of definition quite equal to the line Sr³."

No methods of further purification or treatment by fractional precipitation to which I have submitted some perfectly clear and colourless Iceland spar and other very pure lime compounds having succeeded in my hands in producing a salt which would exhibit a spectrum in which the brilliancy of this blue line was in the least degree either diminished or increased in proportion to that of the other red, yellow, and green lines, I am confirmed in my opinion that this line forms an integral part of the calcium spectrum. The account of the authors' process for the separation of their supposed new metal from the deposit formed on boiling the water seems also to be quite inadequate to separate lime from an accompanying similar metal.

Experimentalists in this new branch of chemical analysis should be warned against relying too much upon the chromo-lithographic drawings of these spectra, published in the *Philosophical Magazine*. Setting aside the differences in the appearance of a metallic spectrum, which are caused by variations in the intensity of the light and in the diameter of the slit, any person who has once seen these brilliantly-coloured bands through a good instrument will be convinced of the utter hopelessness of any coloured copy, more especially when done by a lithographic process for the purposes of book illustration, being more than a very distant resemblance of the real object. Messrs. Kirchhoff and Bunsen's description and illustrations are excellent as far as they go,

but they by no means exhaust the subject. An attentive observer will easily discover lines and other phenomena of which they have made no mention, and which ought certainly to be included in any drawings which profess to represent these spectra with any degree of accuracy. I am at present engaged in preparing such drawings, and will publish them as soon as completed.

*Researches on the Platinum Metals,*¹ by WOLCOTT GIBBS, M.D., Professor of Chemistry and Physics in the Free Academy, New York.

§ 1.

THE material which formed the subject of the present investigation was chiefly obtained from the United States Assay Office and from the Philadelphia Mint, and I am indebted for it to the kindness of Dr. Torrey and of Professor Bache. Messrs. Cornelius, of Philadelphia, have also liberally presented me with about 600 grammes of the Siberian osmiridium,—a supply which has been of great assistance, and for which I desire to express my thanks. The samples obtained from the Mint at different times, and which had been subjected to different preliminary processes, varied greatly in appearance. In some cases the ore was in distinct scales, rather whiter than the Siberian osmiridium. In a sample of this kind Dr. Genth detected distinct crystals, belonging to the rhombohedral system. Other specimens resembled a fine, gray, metallic sand, while others again—obtained from the sweepings of the Assay Office—presented a heavy, gray powder. Nearly all the ores contained more or less iron, mechanically mixed, which could be extracted by the magnet or dissolved out by acids. Nitro-muriatic acid in particular acted powerfully upon many specimens, dissolving portions of the platinum metals as well as iron, and even so far attacking the osmiridium itself as to occasion a distinct evolution of free osmic acid. In California, the ore is, I believe, almost always associated with gold, from which it is, of course, impossible to separate it by washing. At the Assay Office, in this city, the gold is melted with twice its weight of silver, and the osmiridium allowed to settle. When the gold alloy is poured off, there remains a mass containing nearly all the osmiridium mixed with gold. This mass is fused repeatedly with silver, and the last traces of silver and gold are finally removed by treatment with nitric and nitro-muriatic acid and washing. The osmiridium is sold to the manufacturers of gold pens, who extract from it the excessively hard particles, which serve for the points of pens; the rest is returned to the Assay Office. The amount of osmiridium obtained in this way does not exceed a few ounces in every million of dollars, and many samples of gold are entirely free from it. It cannot, however, be doubted, that large quantities of the ore will be obtained whenever important practical application of the metals contained in it shall create a demand.²

¹ From the *American Journal of Science and Art*, No. 91.

² Dr. Torrey has kindly furnished me with the following notes on the Californian osmiridium:—"For the first year or two after the establishment of the United States Assay Office, the proportion of osmiridium in the Californian gold did not exceed half-an-ounce to the million of dollars. Afterwards, the proportion rapidly increased, till the average was seven or eight ounces to the million of gold. Then, for a year or more, the quantity diminished, but for the last year it has been as large as ever. These differences depend upon the variable composition of the native gold and the constant discovery of new diggings. The grains of osmiridium suitable for pens are roundish and solid, not liable to exfoliate when struck or heated. They seem to have a different composition from the compressed and tabular crystals. The proportion of them is usually not more than a

The density of the osmiridium obtained from California varies greatly in different samples; in one specimen, composed of large, dull, white plates, but slightly acted on by nitro-muriatic acid, the specific gravity, as determined by Rose's method, was found to be 19.352. No very definite conclusions can be reached, however, from such determinations, since, as Berzelius has remarked, the separate scales or grains have probably very different compositions. According to G. Rose, the density of the Siberian ore varies from 19.3 to 21.1. Dr. Torrey has found among the scales of the Californian ore some which could be flattened under the hammer; these were probably *platin iridium*. In general, however, the scales are not malleable; some of a lead-gray or bluish tint being with great difficulty cut by the emery-dust employed by the gold-pen makers. In colour the scales vary from nearly silver-white to dark gray.

§ 2.

The re-resolution of the ores of iridium, osmium, &c., and the separation of the different platinum metals from each other, have, as is well known, always been considered as among the most difficult problems with which the chemist has to deal. Though the researches of Wollaston, Berzelius, Wöhler, and other chemists, have thrown much light on the subject, and though Claus, in particular, in his elaborate "*Beiträge zur Chemie der Platin-Metalle*," has done much to free the chemical history of this group from the errors of his predecessors, I yet found that much remained to be done, especially as the Californian ore differs from the Siberian in the greater relative proportion of ruthenium which it contains. This difference alone renders a different treatment of the ores necessary. In the course of the investigation which I have undertaken, I have been able to test, upon considerable quantities of material, nearly all the methods of working the ores of osmium, &c., which have hitherto been employed. As the experience thus obtained has been at great expense of time and labour, I will here give it as concisely as possible, believing that it will be useful to others who may hereafter take up the same subject.

Fremy's³ most recent method consists in roasting the ore in a current of air or oxygen at a strong red heat. Under these circumstances the osmium is in a great measure removed as osmic acid, while the other metals are, more or less, completely oxidised. The mass from which the osmium has been removed is then to be fused with nitre, after which the remaining osmium may be separated by distillation with nitric acid. Fremy gives a method for the separation of the other metals from each other, which, however, cannot yield these in a state of purity. The separation of the osmium by roasting has, undoubtedly, the advantage of giving pure osmic acid in large quantity and at small expense of materials. On the other hand, this process is not applicable to all the varieties of osmiridium, while in those to which it does apply, the roasting does not remove all the osmium, and, consequently, does not obviate the necessity of one or more subsequent fusions with powerful oxidising agents. So far as the iridium, rhodium, and ruthenium are concerned, it is better to oxidise at once, as Claus recommends, by fusion with nitre and caustic potash.

tenth of all the alloy, but it is sometimes as large as one-fifth. The carefully-selected grains used by the gold-pen makers are so minute that from 10,000 to 15,000 of them are contained in a single ounce. The very best are worth at least 250 dollars an ounce, and a cubic inch, which would be equal to about eleven ounces, is worth 2750 dollars."

³ *Comptes-Rendus*, t. xxxvii. p. 1008.

In an experiment which I made to test Fremy's process, and in which I employed California ore, in the form of a fine, gray sand, and heated to full whiteness in a porcelain tube, I obtained, after long heating, but little osmic acid; the tube became clogged and broke, and, after cooling, I found that the ore had actually melted and presented a gray mass, having the shape of the tube in which it was fused. This mass resembled, when broken, a fine-grained cast iron. It was very hard and distinctly crystalline upon those parts of the fracture nearest the surface. As the large scales of osmiridium from California do not melt before the flame of the compound blow-pipe this result was very unexpected; it was, doubtless, owing to the large quantity of iron which the ore contained.

In a memoir published in 1835, Persoz⁴ gave a method of working osmiridium by first converting the metals into sulphides. The ore is to be mixed with carbonate of soda and sulphur and then projected into a strongly-heated earthen crucible. The crucible is then to be heated to whiteness, allowed to cool, and broken. According to Persoz, the contents of the crucible consist of three layers, of which the lowest contains nearly all the sulphides of the platinum metals. The two lower layers are to be treated with water to dissolve the alkaline sulphides, and the remaining mass heated with oxide of mercury, which leaves oxide of iridium, according to Persoz, while oxide of osmium and metallic mercury are expelled.

Weiss and Doebereiner⁵ confirmed the results of Persoz, so far as the conversion of the platinum metals into sulphides was concerned. They recommended the fusion of the sulphides, after removing those of sodium and of iron, by washing with water and chlorhydric acid, with carbonate of potash and saltpetre, so as to oxidise the sulphur and the platinum metals at the same time. By this process the ore is almost completely resolved in two operations.

On repeating these experiments with the California ore, I obtained the same results as to the formation of the metallic sulphides. After digesting the fused matter with chlorhydric acid and washing, there remained a grayish mass of crystalline scales, which resisted even boiling nitro-muriatic acid, no smell of osmic acid being perceptible. These scales were, however, powerfully acted upon by fused caustic potash, to which saltpetre was cautiously added, or by a mixture of carbonate and nitrate of potash; but the process could only be conducted on a small scale in consequence of excessive frothing, which rendered it necessary to use very large vessels. Chlorine gas, at a red heat, exerted no sensible action upon the mixed sulphides.

The difficulties attending the fusion of the mixed sulphides with oxidising agents may, however, be readily overcome by previously reducing them to the metallic state. This is most simply accomplished by a method suggested to me by Dr. Genth, which consists in evaporating the sulphides to dryness with a small excess of strong sulphuric acid and then igniting gently. A gray metallic sponge remains, which contains all the platinum metals with a small quantity of iron. It is easily reduced to a fine powder by rubbing in a mortar, and may then be completely oxidised by Claus' method, presently to be described. Nitro-muriatic acid acts very slightly upon this metallic mixture, and I have not found it possible by this agent to remove an appreciable trace of platinum.

This process, when carefully conducted, gives good results, but is not free from inconvenience. In the first place, the treatment of the mixed sulphides with sulphuric acid must frequently be repeated twice in order to ensure the complete conversion of the sulphides into metals. Again, the action of a mixture of caustic potash and saltpetre, at a high temperature, upon the finely-divided metallic mass is violent and not free from danger when all the materials are heated together. The danger may be avoided by fusing the nitre and potash together, and, after all frothing has ceased, projecting the metallic sponge, little by little, into the crucible, waiting, in each case, until the resulting action has ceased before projecting a fresh portion. I am of opinion that the previous conversion of the platinum metals into sulphides and subsequent reduction to the metallic state is, upon the whole, more inconvenient and requires more time than the direct re-solution of the ore by fusion with an oxidising mixture.

(To be continued.)

On the Estimation of Phosphoric Acid, by F. MAXWELL LITE.

ALLOW me to bring before the readers of the CHEMICAL NEWS a very excellent mode of effecting the determination of phosphoric acid in all compounds, being a modification of an old process. The substance under examination is brought into solution in hydrochloric acid, aqua regia, or nitric acid, as the case may require, and should it contain any organic acids capable of giving a precipitate with salts of lead, these must be first of all destroyed by calcination and subsequent fusion with bisulphate of potassium, or else by boiling with hydrochloric acid, and occasional additions of chlorate of potassium. The solution is now to be diluted and sulphuretted hydrogen passed through it till it smells strongly of that re-agent. By this means any arsenic, antimony, bismuth, &c., are precipitated as sulphides, and iron, if present, is reduced to its minimum of oxidation. It is now filtered and heated, to expel the excess of sulphuretted hydrogen.

Acetate of potassium is then to be added in excess, after which the liquid should smell pretty strongly of acetic acid. The solution may or may not now be clear, as, if iron salts be present, there will always be some slight cloudiness. Acetate of lead is now to be added, till any further addition produces no precipitate. By this means all the phosphates and sulphates, and most of the chlorides, are decomposed with formation of their corresponding lead salts. The precipitate is allowed to digest in the liquid for an hour or so, and then separated and washed on a filter. The filter being then pierced, its contents are to be washed into a beaker glass and there digested with an excess of sulphide of ammonium. By this means the lead salts are in their turn decomposed with formation of the corresponding compounds of ammonium. The sulphide of lead formed is to be separated and washed on a filter, and the filtrate and washing-water, which now contain all the phosphoric acid as phosphate of ammonium, are to be added together. From this solution the phosphoric acid may be precipitated as the double phosphate of magnesium and ammonium by adding sulphate of magnesium, together with ammonia and chloride of ammonium if required, and lastly determined as pyrophosphate of magnesium in the usual manner.

All the metallic oxides belonging to the alkaline, or the alkaline-earth groups, are, if present in the sub-

⁴ Ann. de Chimie et de Physique, t. lv. p. 210.

⁵ Annalen der Pharmacie, bd. xiv. p. 35.

stance under examination, found in the first filtrate, together with nickel, cobalt, manganese, or zinc. Iron and uranium are not so certain to pass entirely with the filtrate, much in this case depending upon their state of oxidation. This process is especially suitable to the separation of phosphoric acid from alumina or oxide of chromium.

Begnères-de-Bigorre.

The New Alkaline Metal, Cesium.

BUNSEN has discovered in the waters of several mineral springs, a new alkaline metal, the existence of which was first detected by the spectral method of qualitative analysis. The new alkali exists in these waters, together with potassium, sodium, and lithium, and its presence may be shown by the spectral analysis with the greatest facility, although only a few milligrammes are contained in several kilogrammes of the material. The author gives only a very brief preliminary notice of the new metal, promising a more extended investigation. The chloride may be distinguished from the chlorides of sodium and lithium by the yellow precipitate which it gives with chloride of platinum. It is distinguished from potassium by the solubility of its nitrate in alcohol. The vapours of the compounds of this metal, when heated so as to become luminous, give an extremely characteristic spectrum, which at the same time exhibits the remarkable simplicity of the spectra of the other alkaline metals. Its spectrum consists of only two blue lines—a weaker line, corresponding with the blue strontium line, and another which lies only a little farther toward the blue end of the spectrum, and which vies in intensity and sharpness of definition with the red line of lithium.—*Journal für prakt. Chemie.*

PHARMACY, TOXICOLOGY, &c.

On *Actæa Racemosa* and its Preparations, by HARRY NAPIER DRAPER.

Actæa racemosa, known also as *cimicifuga racemosa*, *cimicifuga serpentaria*, *macrotys racemosa*, and commonly as black snake-root and cohosh, is a ranunculaceous plant, a native of the United States of America. The root, which is the part employed in medicine, is, under the name of *cimicifuga*, officinal in the United States Pharmacopœia. This root, as met with in commerce, consists of an irregularly-shaped caudex, sometimes several inches in length, and varying between one-third and one inch in thickness, from which arise a number of slender radicles. Externally its colour is dark brown, but when cut or broken it is internally of a yellowish white. When fresh it is stated to have a peculiar and rather disagreeable odour; but as this is not apparent in the specimens which have come under my observation, it is most probably lost by keeping. Its taste is bitter and astringent.

No quantitative examination of the root appears to have been made, but the following qualitative analysis is given on the authority of Mr. Tighlman, of Philadelphia:—

Gum . . .	Black colouring matter.
Starch . . .	Green colouring matter.
Sugar . . .	Lignin.
Wax . . .	
Fatty matter . . .	Potassium.
Tannic acid . . .	salts of { Calcium.
Gallic acid . . .	{ Magnesium.
	{ Iron.

Although no active principle analogous to the alkaloids has yet been isolated, it is probable that *cimicifuga* owes its remedial action to some body of this nature; and as, according to Dr. Wood, the root deteriorates by keeping, this active constituent may suffer decomposition.

The therapeutic action and medicinal use of *actæa* do not fall within my province, but as the remedy is a new one amongst us, I may state, that from what is known of its action in health, it is considered to be a sedative, and is found to exercise a specific action on the uterus. In America, to which country its use had hitherto been limited, it has been used in dropsy, hysteria, some forms of phthisis, and very successfully in the treatment of chorea and rheumatism. Dr. Wood speaks very highly of its efficacy in the treatment of periodical convulsions connected with diseases of the uterus; and it is scarcely necessary to say that Dr. Simpson, to whom we owe its introduction to this country, has found it most efficacious in the cure of puerperal hypochondriasis.¹

Actæa is given in substance, as tincture, extract, and decoction. A resin obtained from the dried root, and known under the name of *cimicifugin*, is also sometimes prescribed.

Pulvis actææ.—This is the powder of the root, and is given in any convenient vehicle—dose twenty to sixty grains.

Tinctura actææ, dose ʒss. to ʒj.

Decoctum actææ, dose ʒj. to ʒij.

Extractum actææ fluidum, dose ʒj.

Extractum actææ durum, dose 4 to 8 grains.

Cimicifugin.—This, the resinous portion of the root, is supposed to represent to a certain extent its active principle, and is sometimes given in doses of from one to two grains.

Dublin.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on *Inorganic Chemistry*, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE III.

Hydrogen—Its Properties—Compounds of Oxygen and Hydrogen—Water.

We have to-day to consider something of the history of another of the elementary bodies—a body which, in many respects, contrasts very strongly with the element which we examined on the last occasion. The element which I propose to introduce to you to-day is Hydrogen. It exists in nature, we may say almost exclusively in combination; it is very rarely indeed that we meet with hydrogen in a free or uncombined state. It has, on one or two occasions only, been found in this condition in the gases issuing from volcanoes; so that we may assume generally that hydrogen exists only in the combined form in nature. When we come to understand the properties of this element, we shall see that it could not well be otherwise, owing to the affinity between oxygen and hydrogen being so powerful. Hydrogen also exists almost entirely in one form of combination, namely, in that form which we know as water. Nine tons of water contain one ton of this element, hydrogen, in combination—chemical combination—with oxygen. We meet also with hydrogen, as an essential constituent of animal and vegetable substances. But when we take into account the very small weight of these, compared with the vast weight of the ocean, of

¹ *Medical Times and Gazette*, December 8, 1860, vol. ii. p. 552.

seas, and of rivers, we may say, generally, that hydrogen exists in nature, almost exclusively in combination with oxygen, and in the form of water.

It is, therefore, from this material, water, that we extract hydrogen for our experimental purposes; or if we do not employ the water in its free and uncombined condition, we employ it in combination with other materials for the same purpose. Now, the most simple process by which we shall be able to show the extraction of hydrogen from water will be one of those manifestations of chemical affinity, which we termed "displacement." We will add to the water a substance which has for oxygen a more powerful affinity than hydrogen possesses, and which combining with the oxygen will leave the hydrogen free, and as hydrogen exists in its uncombined state as a permanent gas, this element, as soon as we free it from its combination with oxygen, assumes a gaseous state. Here I have a quantity of water in this vessel, and here is a glass tube filled with the same liquid. Into this tube I will introduce a fragment of sodium, which is one of the metallic substances upon our list—one of our positive elements belonging to the metals. Its affinity for the oxygen is so great that it tears that element at once away from the hydrogen when it comes in contact with water; and it requires some little care to introduce the sodium into the tube in which we wish to collect our gas hydrogen as it is evolved, without letting the decomposition begin and become too violent for control before the sodium gets into the tube. I, therefore, wrap up the sodium in this blotting paper. In this way I hope to get it introduced under the rim of the tube, when it will rapidly ascend into the upper part of the tube and there the hydrogen gas will collect. We will take next a metal which effects the same purpose, but, at the same time, ignites the hydrogen that is produced. This body is potassium; and we will guard ourselves from any risk of explosion by protecting the surface of the liquid by means of a thick glass plate, because, with this metal, the risk of an explosive action at the end of the process is far greater than with sodium [the potassium was then introduced into the water]. You see the metal disengages the hydrogen, which burns with a beautiful violet flame. In both of these cases, we have exactly the same chemical action. We have first of all a compound of hydrogen and oxygen, which we represent in this way—[illustrating the elements by means of the cubical blocks, as in the last Lecture]—one combining proportion of oxygen, and one combining proportion of hydrogen, and now we bring that into contact with this metal—sodium or potassium, whichever it may be. Let us take a single instance as representative of both. This sodium takes away the oxygen, producing this body, the oxide of sodium, and leaving hydrogen free, which is at once disengaged in the gaseous form. We can effect this separation by the affinity of metals possessing a far less energetic action upon the water than those we have just employed. Iron, for instance, and zinc, both possess the property of decomposing water, but not at ordinary temperatures. It is necessary for our purpose to heat iron to redness in order to effect a separation of the oxygen and hydrogen contained in the water; and we, therefore, pass the water in the form of steam over this metal. Here is a furnace through which an iron tube passes, which iron tube is filled with iron borings or turnings, so that we present a considerable surface of the metal to the action of the steam. At one extremity of this tube we have a little glass retort, containing a small quantity of water, which is made to boil by the spirit lamp. The water is converted into steam, the oxygen going to the iron, as it did in the former cases to the sodium and potassium, and the hydrogen, being eliminated as gas, passing away there, where we can collect it. Here you see it comes in bubbles, and when the operation has gone on a few moments longer, the evolution of the gas will, in all probability, be more rapid.

You see it is already coming very freely, and I dare say we shall succeed in collecting this vessel full of it in a short time. In fact this process, when in active operation, gives us large quantities. None of these processes, however, are very convenient for the preparation of this gas. We have here a much more convenient mode of preparing hydrogen, in considerable quantities for experimental purposes, and this is a form of apparatus which is very convenient for preparing many of these gaseous substances in a regular stream. We are now going to use a compound of water for the reaction in the place of water alone, and we are going to act upon that compound at ordinary temperatures without the assistance of heat. We are going to employ zinc and a combination of sulphuric acid with water, and this is the mode in which we shall disunite the elements forming this compound which we are employing for the extraction of the hydrogen. [The Lecturer illustrated the action by means of the cubes representing the different atoms.] You see it consists of one equivalent of sulphur, four of oxygen, and one of hydrogen. It is a compound, therefore, of hydrogen and oxygen, in the form of water, and what we term sulphuric anhydride, of which we shall know more presently. The action of the zinc is very simple. The zinc takes the place of the hydrogen in this compound, and in place of what we might term sulphate of hydrogen, or sulphate of water, we have now sulphate of zinc, or sulphate of oxide of zinc, as it might also be termed. You see we simply put zinc into this compound in the place of hydrogen. This apparatus [the one in which the hydrogen was generating by this last process] consists of three globes. The two lower globes are connected by a neck containing a kind of perforated stage on which the zinc can repose. The upper globe is united with the lowest by a long glass tube, which passes through the central bulb and nearly to the bottom of the lowest bulb, so that if I pour any liquid into the upper funnel it will descend gradually into the lowest globe and displace any gas which may be there, and then ascend into the centre globe, expelling a stream of gas from the exit tube. If we want to collect the gas, all we have to do is to put this flexible tube [at the end of the glass delivering-tube of the apparatus] underneath this gas jar, and turn on the stop-cock. As soon as the liquid, which consists of dilute sulphuric acid, as I have before said, comes in contact with the zinc, an active decomposition takes place, and the hydrogen continues to be disengaged in a constant stream so long as contact between the liquid and the zinc is kept up. When, however, a sufficient quantity has been collected, we have only to turn this stop-cock and prevent the exit of the gas. Then the pressure of the gas in the vessel forces the liquid into the upper globe. The action goes on for a short time even after the liquid has apparently actually left contact with the zinc, and some of the liquor in the lower globe is expelled into the upper. This process is that which we almost invariably employ for the preparation of hydrogen gas.

Hydrogen possesses the following properties:—It is the lightest material with which we are acquainted; it is only, bulk by bulk, one-sixteenth the weight of oxygen, and only one-fourteenth the weight of an equal bulk of atmospheric air. One of the most common modes of showing the levity of this gas is to inflate some light envelope or balloon with it. We have here a store of this gas ready prepared, and I will inflate this balloon from it. By this arrangement I get a stream of hydrogen issuing from this jet, and if I now tightly clasp the neck of the balloon round it, I shall very shortly inflate it. [The Lecturer proceeds to fill a small collodion balloon with hydrogen.] It is evident already our balloon has some ascending power, although it is still far from being filled. You see how impatient it is to ascend. We will allow it to get quite full. Now, see how it will dart upwards with all this length of twine tied to it; you thus perceive that

the difference between the weight of equal bulks of this material, hydrogen, and of atmospheric air is very considerable. Hydrogen is very slightly soluble in water; far less so, even, than oxygen. Scarcely two volumes of hydrogen dissolve in one hundred volumes of water, we can, therefore, collect it over water without any appreciable loss.

One of the most striking properties of hydrogen is its power of burning, in contact with atmospheric oxygen or free oxygen in general. It is a combustible gas. If I take one of these jars in which we have some of it collected, and introduce into it a lighted taper, you see the gas takes fire, but the taper will not burn in it; it is extinguished on getting into the hydrogen, but it is re-lighted on passing through the flame below. The gas itself burns with a lambent flame, producing hardly any luminous effect. Now that is the way [referring to the slow combustion of the gas at the mouth of an inverted jar] in which the hydrogen burns, when it comes very slowly in contact with the air. If we take another jar, and hold it with the mouth upwards, so that the gas will ascend quickly, you see it burns almost immediately with a slightly explosive flash. From a jet, also, this gas burns with a pale bluish flame, possessing, as you see, very little illuminating power. In fact, the greater part of the yellowish light in that flame is due to the presence of sodium in the air from the experiment just made. If we mix some of this hydrogen with a sufficient quantity of oxygen, and then ignite the mixture, the combination takes place almost instantaneously. It occupies a time far too short to be measured. However, before showing you the effect of this mixture of the two gases, let me just direct your attention to a peculiarity of flame which is not possessed by the hydrogen flame alone, although it is perhaps more readily produced by this flame than any other—the power of producing a kind of musical note when it is burnt in the interior of a glass tube. [A tube was held over a small jet of hydrogen and a musical sound was immediately produced.] You perceive we have a tolerably clear musical note here produced, the pitch of which depends upon the length and diameter of the tube, which might be varied. The effect is produced by the flame itself being thrown into a kind of vibration. It is, in fact, alternately ignited above and below, and this action is kept up so long as the flame burns in the interior of the tube in this flickering manner, that is, with an alternate extinction and re-ignition. I will now produce an effect of the same kind by other means which will show that it is not peculiar to hydrogen alone, but is also produced by common coal-gas. [A tin tube about seven feet long and three inches in diameter was held over a flame of coal-gas issuing from a large argand burner.] This is a note of very much greater depth than we had in the former case, but still the effect is similar in both instances.

Now, as I before said, if we mix together these two elements, oxygen and hydrogen, and then ignite them, combination takes place almost instantaneously with the production of a loudly explosive flash. We may show this in a way perfectly free from any danger, by first mixing our gases in that gas-holder, the construction of which has already been explained, and then allowing them to escape into a strong solution of soap, by means of which we shall produce some soap-bubbles. [Some soap-bubbles were then blown with the mixed gases contained in the gas-holder.] I do not know whether we have the gases in these bubbles, or simply the air that was in the pipe. We will try them. [A light was applied to the bubbles, and they exploded with a loud report.] You see we have produced a very explosive effect there. This explosion, although attended by a comparatively loud report is, in this form at least, a very innocuous one. [Explodes a bubble from the palm of his hand.] We may explode them from the hand without any pain, much less danger.

It would be possible to apply this explosive force in a manner similar to gunpowder. Let us try an experiment in this direction. Here is a little paste-board cannon; if you like to call it so, which I will place over this mixture. [A small paste-board cylinder about five inches in diameter and twelve inches long was placed perpendicularly upon the table, over the vessel containing soap-bubbles.] This shall be our harmless cannon-ball [placing an india-rubber air-ball on the upper end of the cylinder. A light was then applied to the "touch-hole" and the ball was ejected almost to the top of the theatre]. We have a considerable projectile power, although this is far less expansive than gunpowder. In the explosion of a mixture of oxygen and hydrogen, the maximum expansion is ten times, and the gases subsequently contract to two-thirds of their volume before explosion. In the explosion of gunpowder the expansion is four thousand times; hence, you see the difference in the effects which they produce. This explosion of these mixed gases is one quite similar in its effects to an explosion of fire-damp in mines, or an explosion of coal-gas in houses, which sometimes, though rarely, takes place. In the case of fire-damp in coal-mines, though sometimes these explosions are very destructive, yet they cause those disastrous consequences to a great extent by the after effects. A vitiation of the air takes place and produces more fatal effects than the explosion itself. To this I shall have to refer in another Lecture.

We have described hydrogen as a combustible gas, and oxygen as a supporter of combustion. The positive elements are commonly termed *combustibles*, and the negative *supporters of combustion*; but these terms are, however, merely conventional. We might just as well say that oxygen is a combustible gas, although it never burned, but supported combustion in our experiments; while the hydrogen did not support the combustion of the taper, but burnt itself. These terms, however, are, as I have said, purely conventional. The phenomenon of combustion takes place merely at the limiting surfaces of the two gases. When we burn them the phenomenon takes place at the line of division between the two substances, which unite chemically. If our atmosphere had contained coal-gas instead of oxygen we might then have conducted oxygen gas through pipes to burn it for light and heat, just as we now conduct coal-gas through pipes and burn it in the oxygen of the air. That the atmospheric air is really capable of burning when in an atmosphere of coal-gas, as coal-gas is capable of burning in an atmosphere of air, I will endeavour to show you by a very simple experiment. Here is a gas jar which I will fill with coal gas from the gas-pipe. We will inflame it there as it comes in contact with the air to show that the jar is full of coal-gas; and when the jar has thus been filled, I will introduce a current of air from this tube which is connected there with an air jar filled with atmospheric air—filled with the air of the room, and which will be compressed in this tank of water so as to give me a jet of atmospheric air passing into this jar containing the coal-gas. Now we have a quantity of coal-gas passing out there [raising the jar and igniting the gas]. There is the coal-gas burning underneath our jar. [The jar was replaced in its original position.] Now I will pass in the air through that tube in the centre of the jar. The air will take fire. [A light was applied to the end of the tube, and the air ignited.] There it is; there is the air burning in the atmosphere of coal-gas. There is the jet of atmospheric air passing through this tube and burning in the atmosphere of coal-gas which has been supplied to the jar through the vulcanized india-rubber tube; so you see, these terms "combustible" and "supporter of combustion" are entirely conventional. The burning takes place at the line of junction between the two bodies. Whether the oxygen be inside or outside does not matter as regards the operation of combustion.

The amount of heat generated in this process of combustion is very considerable. It is different in different materials, which are submitted to the operation of combustion, and there are two points of view from which the heat of combustion may be regarded. We may, in the first place, regard it as to the total amount of heat; and secondly, as to the temperature produced, or the intensity of that heat, I have marked here, in the memoranda, "The total quantity of heat emitted by a combustible body, is termed its *absolute thermal effect*," whilst the intensity of the heat, or the temperature produced by a combustible, is termed its *pyrometric thermal effect*. The absolute thermal effect of different combustibles used as fuels is a matter which, in practice, is of considerable importance. For instance, in sea-going vessels the amount of heat which can be obtained from various descriptions of fuel is a very important consideration, because upon that depends the amount of power which can be obtained, and the number of times it will be necessary for the stock of fuel to be replenished. Here is an apparatus, which was invented by Mr. Lewis Thompson, for the determination of the absolute thermal effect of different combustibles. This larger vessel is filled with water. If I can burn any fuel in the middle of that water in such a manner that none, or very little, of the heat can escape, and if I then ascertain the rise in the temperature of that water, I shall be able to tell the effect produced upon any given quantity of water, and I shall be able to deduce from that the quantity of steam which the fuel can generate. If we take the temperature of the water to begin with, and then burn our sample of fuel in it, again taking its temperature at the termination of the combustion, we shall then have our data. The temperature of this water is now 51°F . A mixture is formed, consisting of thirty grains of the combustible, mixed with about thirteen times its weight of chlorate of potash, that being also mixed with one-third of its weight of nitrate of potash. The object of these salts is to supply oxygen to the combustible while under the water. Here is the arrangement for burning it below the surface of the water. We have first a little cotton-wick which is dipped in nitre, and which will serve as a slow-match and burn down to the mixture in the copper tube and ignite it. This is placed on a copper base, and over it is inverted a copper bell-jar, perforated with a number of small apertures, through which the gases produced will pass through the water, and there give up their heat. The substance is enclosed in the bell-jar, and the whole of the apparatus lowered down into the vessel containing the water. We will now ignite the wick. [The wick burnt down and ignited the mixture before the apparatus was lowered into the water.] We have got our effect rather quicker than we wanted; however, it will be quite sufficient for our purpose. You see the ignition, which ought to have taken place when we lowered it to the bottom, occurred too soon. The combustion is now going on at the bottom of the water; and the gases have to pass through the water, communicating their temperature to it. At the conclusion of the operation the water is agitated to get the mean temperature; and things are so arranged that each degree through which the temperature rises corresponds to one pound of water which would be converted into steam by the burning of one pound of the fuel. If we found that the water had risen ten degrees, we should know that this sample of coal would be capable of converting ten times its weight of water into steam; and so by making the experiment with different fuels, you get in a very short time their relative value for steam or other such purposes. The absolute thermal effects of different fuels—different combustibles—are represented in this table, and you will there see the great pre-eminence of hydrogen in this respect. Taking the absolute thermal effect of carbon as unity, the same weight of hydrogen produces in its combustion nearly $4\frac{1}{2}$ times the heat of

the carbon; and olefiant gas $1\frac{1}{2}$ times, and so on. Here is the Table:—

Absolute Thermal Effect of Combustibles.

Hydrogen	4.46	Alcohol	0.86
Olefiant gas	1.52	Good coal	0.77
Olive-oil	1.22	Dry wood	0.46
Ether	1.02	Air-dry wood	0.45
Carbon	1.00	Peat	0.36
Charcoal	0.96		

The absolute thermal effect, ascertained by experiments of this kind, of different kinds of fuel in ordinary use, especially of the two kinds of fuel generally used for heating purposes in contradistinction to gas, is shown in this table.

Quantity and cost of gaseous and solid fuel required to evaporate 1000 lbs. of water from 212° :—

1833 cubic feet of coal-gas cost, at 4s. per 1000 cubic feet,	7 4	s. d.
2874 " " water-gas " 2s. " "	5 9	
125 lbs. coals " 20s. per ton " "	1 1	
112 lbs. coal " " " " "	1 0	

1833 cubic feet of coal-gas at 4s. per thousand, are required to evaporate 1000 lbs. of water from 212° . To do this amount of work, therefore, you are required to employ a quantity of gas which costs 7s. 4d. If, in the place of coal-gas you use gas which is produced from water by charcoal,—a mixture of hydrogen and carbonic oxide, chiefly,—2874 cubic feet are necessary, which will cost the sum of 5s. 9d. for that amount of heating power. 125 lbs. of coke will do precisely the same amount of work, will heat exactly the same quantity of water to the same temperature for 1s. 1d. Or 112 lbs. of coal will do the same for 1s. I have put this diagram before you to show at a glance the great difference there is between these kinds of fuel. You see that coal and coke produce a much greater amount of heat at a less cost than gas, although gas has been proposed for heating purposes; and no doubt there are great facilities for the use of gas which may contribute to some extent to counterbalance the great discrepancy in its cost.

The light of combustion is a matter to a great extent distinct from the heat that is given off from the burning substance. It depends upon conditions which I shall not in the present lecture have time to enter fully into; but two of those conditions are, first, the presence of solid matter in the flame during combustion; and secondly, the intensity of the temperature of that flame. The more solid matter there is in the flame, within certain limits, the greater will be the light emitted from that flame supposing its temperature to be constant; but having the same amount of solid matter the light will vary with the intensity of the heat. If you intensify the heat you will intensify the light. Let me endeavour to illustrate that by our jet of hydrogen here. This jet of hydrogen if it be made to impinge on a small cylinder of lime, will ignite that to a certain temperature, and we shall get a little more light from the combustion of the hydrogen than we do without the lime, especially if we were to wait for some little time until the lime had become as strongly ignited as this jet is capable of igniting it. But if I increase the pyrometric thermal effect of the flame, and make it burn at the expense of pure oxygen instead of air, I shall greatly increase the intensity of the light. The pyrometric thermal effect of hydrogen in air is only 1611°C .; but if it burns in oxygen it produces a temperature of 3148°C ., so that I nearly double the temperature by employing oxygen. [A piece of lime was placed in the hydrogen flame.] You see some of the lime has become ignited so as to increase the luminosity, but the luminosity is very small. Now let me introduce some oxygen and make the hydrogen burn at the expense of oxygen instead of the air, and I think you will soon see what the increase in the pyrometric thermal effect—in the temperature of that flame—effects as regards luminosity:

You see what a brilliant light we have emitted from this solid, which is thus rendered incandescent in the interior of the flame. If we again make the combustion dependent on air, the temperature immediately falls and the light ceases.

In these cases of the combustion of hydrogen, whether it took place in atmospheric air or in oxygen, we had one uniform product, and that is what we term the protoxide of hydrogen, a compound of one equivalent of each element, and which we all know as water. Here is a jet of hydrogen which has been burning from the beginning of the lecture, and from which we have been producing a liquid, which if tested would be found to be pure water. It is now trickling down. It is the product of this hydrogen-flame which is burning at the other end of the condenser; if we, on the other hand, submit water to the action of a force which is capable of separating its elements and presenting them both in their free state, we find it also to be composed of these two elements in the proportion of one volume of oxygen to two volumes of hydrogen. Here, for instance, is an apparatus by which I hope to be able to illustrate this. Here is a voltaic battery of moderate power, from which a current is passing into this water which is acidulated with sulphuric acid. There are two tubes standing over the ends of the wires. Here you see we are collecting our gases which we will presently examine. This decomposition may, I think, be seen more clearly at a distance if I throw a magnified representation of it on the screen. Let us have the room darkened. I have here a much smaller decomposing vessel than the one I have just been employing, but exactly the same materials, namely, two platinum poles, a vessel to contain the acidulated water, and the means of making the communication between the two poles of the battery. [The room was darkened, and the gas-lights turned down. An image of the decomposing apparatus was then projected on the screen.] There are our two poles, which I dare say we shall get a little clearer by bringing them better into focus. Now I will make the connection with the battery, and you see we have at once a rapid disengagement of the two gases. [The bubbles of the gases were distinctly seen proceeding from the poles.] You will notice the much larger amount of gas which is being evolved from one pole than there is being evolved from the other. You have seen there upon the screen the mode in which the gases are torn asunder, and we have them here collected in jars by a larger form of apparatus. We will put the battery again into connection with this larger apparatus, so as to fill the jars with the gases, and we will test their quality if we have sufficient time left for it. You will now understand why it is that I have arranged these elements under the heads of positive and negative. If we carefully watch this experiment we notice that the hydrogen makes its escape from the negative pole of that voltaic arrangement, and that the oxygen is set free from the positive pole; and hence it is said that the hydrogen is a *positive*, or *electro-positive*, element, because it is attracted by the negative pole, whilst the oxygen is said to be a *negative*, or *electro-negative*, body, because it is attracted by the positive pole of the battery.

Water thus produced possesses a number of very remarkable properties, only one of which, however, I shall be able to show you in the present Lecture, namely, its colour. Water, seen through a glass full of the liquid, appears to be perfectly colourless; but if we look at it through a stratum of fifteen feet, which we shall be able to do by means of this arrangement contrived by Dr. Tyndall, I hope to prove to you that it is by no means the colourless liquid which we sometimes imagine it to be. [The apparatus consisted of a tin tube fifteen feet long and about three inches in diameter, placed horizontally on a stand, and half filled with water. The tube was closed by plate glass at each end, and a beam of electric light

was thrown through it from the other end. By this means an image of the contents of the tube was projected on a white screen. Dr. Tyndall assisted in the adjustment of the apparatus.] Now, you see, this tube is about half filled with water. The image there upon the screen is inverted by the lens, so that you see the upper air-space of this tube in the lower part of that image, and that is colourless, while the rays which pass through the stratum of water are of a greenish-blue colour. The colour varies from a pure green up to a blue, according to the purity, or otherwise, of the water. Thus it is evident that the colour of water is very appreciable, for we have here a stratum of only fifteen feet in thickness, which exhibits a very considerable amount of colour. There is, therefore, no difficulty in comprehending the fact that in looking through a deeper stratum such as we see in the Swiss lakes, and in the waters which we have around our own shores, that this colour of water makes itself very perceptible.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, February 22, 1861.

On Platinum, by M. FARADAY, D.C.L., F.R.S., Fullerman Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.

WHETHER I was to have the honour of appearing before you this evening or not seemed to be doubtful upon one or two points. One of these I will mention immediately; the other may or may not appear during the course of the hour that follows. The first point is this. When I was tempted to propose this subject for your attention this evening, it was founded upon a promise, and a full intent of performing that promise, on the part of my friend Deville, of Paris, to come here to show before you a phenomenon in metallurgic chemistry not common. In that I have been disappointed. His intention was to have fused here some thirty or forty pounds of platinum, and so to have made manifest through my mouth and my statement the principles of a new process in metallurgy in relation to this beautiful, magnificent, and valuable metal; but circumstances over which neither he nor I nor others concerned had sufficient control, have prevented the fulfilment of that intention, and the period at which I learned the fact was so recent, that I could hardly leave my place here to be filled by another, or permit you, who in your kindness have come to hear what might be said, unreceived in the best manner possible to me under the circumstances. I, therefore, propose to state as well as I can what the principles are on which M. Deville proceeds, by means of drawings, and some subordinate or inferior experiments.

The metal platinum, of which you see some very fine specimens on the table, has been known to us about a hundred years. It has been wrought in a beautiful way in this country, in France, and elsewhere, and supplied to the consumer in ingots of this kind, or in plates, such as we have here, or in masses, that by their very fall upon the table indicate the great weight of the substance, which is, indeed, at the head of all substances in that respect. This substance has been given to us hitherto mainly through the philosophy of Dr. Wollaston, whom many of us know, and it is obtained in great purity and beauty. It is a very remarkable metal in many points, besides its known special uses. It comes to us in grains. Here is a very fine specimen of native platinum in grains. There is here also a nugget or ingot, and here are some small pieces gathered out of certain alluvial soils in Brazil, Mexico, California, and the Uralian districts of Russia.

It is strange that this metal is almost always found

associated with some four or five other metals most curious in their qualities and characteristics. They are called platiniferous metals; and they have not only the relation of being always found associated in this manner, but they have other relations of a curious nature which I shall point out to you by a reference to one of the tables behind me. This substance is always native, it is always in the metallic state, and the metals with which it is found connected, and which are rarely found elsewhere, are palladium, rhodium, iridium, osmium, and ruthenium. We have the names in one of the tables arranged in two columns, representing, as you see, two groups; platinum, iridium, and osmium, constituting one group, and ruthenium, rhodium, and palladium the other. Three of these have the chemical equivalent of 98, and the others a chemical equivalent of about half that number. Then the metals of one group have an extreme specific gravity, platinum being, in fact, the lightest of the three, or, as light as the lightest. Osmium has a specific gravity of 21.4, and is the heaviest body in nature; platinum is 21.5, and iridium the same; the specific gravity of the other three being only about half that, namely, 11.3, 12.1, and 11.8. Then there is this curious relation, that palladium and iridium are very much alike, so that you would scarcely know one from the other, though one has only half the weight of the other, and only half the equivalent power. So with iridium and rhodium, and osmium and ruthenium, which are so closely allied that they make pairs being separated each from its own group. Then these metals are the most infusible that we possess. Osmium is the most difficult to fuse; indeed, I believe it never has been fused, while every other metal has; ruthenium comes next, iridium next, rhodium next, platinum next (so that it ranks here as a pretty fusible metal, and yet we have been long accustomed to speak of the infusibility of platinum,) and next comes palladium, which is the most fusible metal of them. It is a curious thing to see this fine association of physical properties coming out in metals which are grouped together somehow or other in nature, but, no doubt, by causes which are related to analogous properties in their situation on the surface of the earth, for it is in alluvial soils that these things are found. Hence arises that curious difficulty which occurred lately about the specific gravity, which I was one to complain of, in Mr. De la Rue's pocket-books; but he was right in putting another body above platinum, which is not the heaviest body in nature.

Now, with regard to this substance, let me tell you briefly how we get it. The process used to be this. The ore which I showed you just now was taken, and digested in nitro-muriatic acid, and then converted into a solution which caused the leaving behind of certain bodies that I have upon the table. The platinum being dissolved with care in acids of a certain strength, to the solution the muriate of ammonia was added, as I am about to add it here. A yellow precipitate was then thrown down, as you perceive is the case now; and this, carefully washed and cleansed, gave us that body [pointing to a specimen of the chloride of platinum and ammonium], the other elements, or nearly all, being ejected. This substance being heated, gave us what we call platinum sponge, or platinum in the metallic state, so finely divided as to form a kind of heavy mass or sponge, which, at the time that Dr. Wollaston first sent it forth, was not fusible for the market or in the manufacturers' workshops, inasmuch as the temperature required was so high, and there were no furnaces that could bring the mass into a globule, and cause the parts to adhere together. Most of our metals that we obtain from nature, and work in our shops, are brought at last into a mass by fusion. I am not aware that there is in the arts or sciences any other than iron which is not so. Soft iron we do not bring together by fusion, but by a process which is analogous to the one that was followed in the case of platinum, namely, welding; for these divided grains of spongy platinum having been well washed and

sunk in water for the purpose of excluding air, and pressed together, and heated, and hammered, and pressed again, until they came into a pretty close, dense, compact mass, did so cohere that when the mass was put into the furnace of charcoal, and raised to a high temperature, the particles, at first infinitely divided,—for they were chemically divided,—adhered the one to the other, each to all the rest, until they made that kind of substance which you see here which will bear rolling and expansion of every kind. No other process than that has hitherto been adopted for the purpose of obtaining this substance from the particles by solution, precipitation, ignition, and welding. It certainly is a very fine thing to see that we may so fully depend upon the properties of the various substances we have to deal with that we can by carrying out our processes obtain a material like this, allowing of division, and extension under a rolling mill, a material of the finest possible kind; the parts being held together, not with interstices, not with porosity, but so continuous that no fluids can pass between them; and, as Dr. Wollaston beautifully showed, a globule of platinum fused by the voltaic battery and the oxy-hydrogen blow-pipe, when drawn into a wire, was not sounder or stronger than this wire made by the curious coalescence of the particles by the sticking power that they had at high temperatures. This is the process adopted by Messrs. Johnson and Matthey, to whose great kindness I am indebted for these ingots and for the valuable assistance I have received in the illustrations.

The treatment, however, that I have to bring before you is of another kind, and it is in the hope that we shall be able before long to have such a thing as the manufacture of platinum of this kind that I am encouraged to come before you, and tell you how far Deville has gone in the matter, and to give you illustrations of the principles on which he proceeds. I think it is but fair that you should see an experiment showing you the way in which we get the adhesion of platinum. Probably you all know of the welding of iron: you go into the smith's shop, and you see him put the handle of a poker on to the stem, and by a little management and the application of heat he makes them one. You have no doubt seen him put the iron into the fire and sprinkle a little sand upon it. He does not know the philosophy he calls into play when he sprinkles a little sand over the oxide of iron, but he has a fine philosophy there, or practises it when he gets his welding. I can show you here this beautiful circumstance of the sticking together of the particles up to the fullest possible intensity of their combination. If you were to go into the workshops of Mr. Matthey, and see them hammering and welding away, you would see the value of the experiment I will show you. I have here some platinum wire. This is a metal which resists the action of acids, resists oxidation by heat, and change of any sort, and which, therefore, I may heat in the atmosphere without any flux. I bend the wire so as to make the ends cross: these I make hot by means of the blow-pipe, and then by giving them a tap with a hammer I shall make them into one piece. Now that the pieces are united I shall have great difficulty in pulling them apart though they are joined only at the point where the two cylindrical surfaces came together. Now that I have succeeded in pulling the wire apart the division is not at the point of welding, but where the force of the pincers has cut it, so that the junction we have effected is a complete one. This, then, is the principle of the manufacture and production of platinum in the old way.

The treatment which Deville proposes to carry out, and which he has carried out to a rather large extent in reference to the Russian supply of platinum, is one altogether by heat, having little or no reference to the use of acids. That you may know what the problem is, look at this table, which gives you the composition of such a piece of platinum ore as I showed you just now. Wherever it

comes from the composition is as complicated, though the proportions vary.

Platinum	76.4
Iridium	4.3
Rhodium	0.3
Palladium	1.4
Gold	0.4
Copper	4.1
Iron	11.7
Osmide of iridium	0.5
Sand	1.4

100.9

This refers to the Uralian ore. In that state of combination as shown in the table the iridium and osmium are found combined in crystals sometimes to the amount of 0.5 per cent., and sometimes 3 or 4 per cent. Now this Deville proposes to deal with in the dry way, in the place of dealing with it by any acid.

I have here another kind of platinum; and I show it to you for this reason. The Russian Government having large stores of platinum in their dominions, have obtained it in a metallic state and worked it into coin. The coin I have in my hand is a 12 silver rouble piece. The rouble is worth 3s., and this coin is, therefore, of the value of 36s. The smaller coin is worth half that sum; and the other, half of that. The metal, however, is unfit for coinage. When you have the two metals, gold and silver, used for coinage, you have a little confusion in the value of the two in the market; but when you have three precious metals (and you may call platinum a precious metal) worked into coin, they will be sure to run counter to one another. Indeed, the case did happen that the price of platinum coin fixed by the Government was such that it was worth while to purchase platinum in other countries and make coin of it, and then take it into the country and circulate it. The result was that the Russian Government stopped the issue. The composition of this coin is, platinum, 97.0; iridium, 1.2; rhodium, 0.5; palladium, 0.25; a little copper, and a little iron. It is, in fact, bad platinum: it scales, and it has an unfitness for commercial use in the laboratory, which the other well purified platinum has not. It wants working over again.

Now, Deville's process depends upon three points,—upon intense heat, blow-pipe action, and the volatility of certain metals. We know that there are plenty of metals that are volatile, but this, I think, is the first time that it has been proposed to use the volatility of certain metals, such as gold and palladium, for the purpose of driving them off and leaving something else behind. He counts largely upon the volatility of metals which we have not been in the habit of considering volatile, but which we have rather looked upon as fixed; and I must endeavour to illustrate these three points by a few experiments. Perhaps I can best show you what is required in the process of heating platinum by using that source of heat which we have here, and which seems to us almost illimitable,—namely, the voltaic battery; for it is only in consequence of the heat that the voltaic battery affects the platinum. By applying the two extremities of the battery to this piece of platinum wire, you will see what result we shall obtain. You perceive that we can take about this heating agent wherever we like, and deal with it as we please, limiting it in any way. I am obliged to deal carefully with it; but even that circumstance will have an interest for you in watching the experiment. Contact is now made. The electric current, when compressed into thin conducting-wires, offering resistance, evolves heat to a large extent, and this is the power by which we work. You see the intense glow immediately imparted to the wire, and if I applied the heat continuously the effect of the continued current would be to melt the wire. As soon as the contact is broken the wire resumes its former appearance; and now that we make contact again you perceive

the glow as before. [The experiment was repeated several times in rapid succession.] You can see a line of light, though you can scarcely perceive the wire; and now that it has broken with the great heat, if you examine it you will perceive that it is indeed a set of irregularities from end to end,—a set of little spheres, which are strung upon the axis of the platinum running through it. It is that wire which Mr. Grove described as being produced at the moment when fusion of the whole mass is going on. In the same manner, if I take a tolerably thick piece of platinum, and subject it to the heat that can be produced by this battery, you will see the brilliancy of the effect produced. I shall put on a pair of spectacles for the experiment, as there is an injurious effect of the voltaic spark upon the eyes if the action is continued, and it is neither policy nor bravery to subject any organ to unnecessary danger, and I want, at all events, to keep the full use of my eyes to the end of the Lecture.

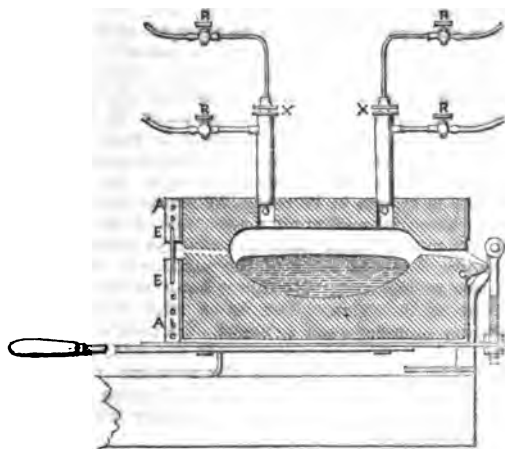
You now see the action of the heat upon the piece of platinum—heat so great as to break in pieces the plate on which the drops of metal fall. You perceive, then, that we have sufficiently powerful sources of heat in nature to deal with platinum. I have here an apparatus by which the same thing can be shown. Here is a piece of platinum, which is put into a crucible of carbon made at the end of one pole of the battery, and you will see the brilliant light that will be produced. There is our furnace, and the platinum is rapidly getting heated; and now you perceive that it is melted, and throwing off little particles. What a magnificent philosophical instrument this is! When you look at the result, which is lying upon the charcoal, you will see a beautifully fused piece of platinum. It is now a fiery globule, with a surface so bright, and smooth, and reflecting, that I cannot tell whether it is transparent, or opaque, or what. This, then, will give you an idea of what has to be done by any process that pretends to deal with thirty, or forty, or fifty pounds of platinum at once.

Let me now tell you briefly what Deville proposes to do. First of all he takes this ore with its impurities and mixes it (as he finds it essential and best) with its own weight of sulphuret of lead—lead combined with sulphur. Both the lead and the sulphur are wanted; for the iron that is there present, as you see by the table, is one of the most annoying substances in the treatment that you can imagine, because it is not volatile; and while the iron remains adhering to the platinum, the platinum will not flow readily. It cannot be sent away by a high temperature—sent into the atmosphere so as to leave the platinum behind. Well, then, a hundred parts of ore and a hundred parts of sulphuret of lead, with about fifty parts of metallic lead, being all mingled together in a crucible, the sulphur of the sulphuret takes the iron, the copper, and some of the other metals and impurities, and combines with them to form a slag; and as it goes on boiling and oxidizing, it carries off the iron, and so a great cleansing takes place.

Now, you ought to know that these metals, such as platinum, iridium, and palladium, have a strong affinity for such metals as lead and tin, and upon this a great deal depends. Very much depends upon the ore throwing out its impurities of iron and so forth by being taken up with the lead present in it. That you may have a notion of the great power that platinum has of combining with other metals, I will refer you to a little of the chemist's experience—his bad experience. He knows very well that if he takes a piece of platinum foil and heats a piece of lead upon it, or if he takes a piece of platinum foil, such as we have here, and heats things upon it that have lead in them, his platinum is destroyed. I have here a piece of platinum, and if I apply the heat of the spirit lamp to it, in consequence of the presence of this little piece of lead which I will place on it, I shall make a hole in the metal. The heat of the lamp itself would do no harm to the platinum, nor would

other chemical means; but because there is a little lead present, and there is an affinity between the two substances, the bodies come together at once. You see the hole I have made. It is large enough to put your finger in, though the platinum itself was, as you saw, almost infusible, except by the voltaic battery. For the purpose of showing this fact in a more striking manner, I have taken pieces of platinum-foil, tin foil, and lead foil, and mingled them together; and if I apply the blow pipe to them you will have, in fact, a repetition on a larger scale of the experiment you saw just now when the lead and platinum came together, and one spoiled the other. When the metals are laid one upon the other and folded together and heat applied, you will not only see that the platinum runs to waste, but that at the time when the platinum and lead are combined there is ignition produced, there is a power of sustaining combustion. I have taken a large piece, that you may see the phenomenon on a large scale. You saw the ignition and the explosion which followed, of which we have here the results—the consequence of the chemical affinity between the platinum and the metals combined with it, which is the thing upon which Deville founds his first result.

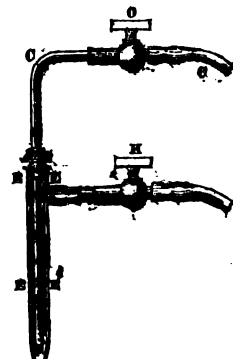
When he has melted these substances and stirred them up well, and so obtained a complete mixture, he throws in air upon the surface to burn off all the sulphur from the remaining sulphuret of lead; and at last he gets an ingot of lead with platinum—much lead comparatively, and little platinum. He gets that in the crucible with a lot of scorise and other things, which he treats afterwards. It is that platiniferous lead which we have to deal with in our future process. Now let me tell you what he does with it. His first object is to get rid of the lead. He has thrown out all the iron, and a number of other things, and he has got this kind of compound indicated in the table. He may get it as high as 78 per cent. of platinum, and 22 of lead; or 5 or 10 or 15 of platinum, and 95, or 90 or 85 of lead (which he calls weak platinum), and he then places it in the kind of vessel that you see before you. Suppose



we had the mixture here; we should have to make it hot, and then throw in air upon the surface. The combustible metal, that is, the lead, and the part that will oxidise, are thoroughly oxidised; the litharge would flow out in a fused state into a vessel placed to receive it, and the platinum remains behind.

Here is the process which Deville adopts for the purpose of casting off the lead after he has got out the platinum from the ore. (Having made use of your friend, you get rid of him as quickly as you can.) He gets his heat by applying the combination of oxygen and hydrogen, or of carburetted fuel for the purpose of producing a fire. I

have here a source of coal-gas; there I have a source of hydrogen; and here I have a source of oxygen. I have here also one of the blow-pipes used by Deville in his



process for working platinum in the way I have spoken of. There are two pipes, and one of them goes to the source of coal-gas, and the other to the supply of oxygen. By uniting these we obtain a flame of such a heat as to melt platinum. You will perhaps hardly imagine what the heat is unless you have some proof of it; but you will soon see that I have actually the power of melting platinum. Here is a piece of platinum-foil running like wax under the flame which I am bringing to bear against it. The question, however, is whether we shall get heat enough to melt, not this small quantity, but large masses, many pounds of the metal. Having obtained heat like this, the next consideration is, what vessel is he to employ which could retain the platinum when so heated, or bear the effects of the flame? Such vessels are happily well supplied at Paris, and are formed of a substance which surrounds Paris; it is a kind of chalk (called, I believe, by geologists *calcaire grossière*), and it has the property of enduring an extreme degree of heat. I am now going to get the highest heat that we can obtain. First, I show you the combustion of hydrogen by itself. I have not a large supply, because the coal-gas is sufficient for most of our purposes. If I put a piece of this chalk into the gas, you see we get a pretty hot flame which would burn one's fingers a good deal. But now let me take a piece of lime and subject it to the joint action of oxygen and hydrogen. I do this for the purpose of showing you the value of lime as a material for the furnaces and chambers that are to contain the substances to be operated on, and that are consequently to sustain the action of this extreme heat. Here we have the hydrogen and the oxygen, which will give the most intense heat that can be obtained by chemical action; and, if I put a piece of lime into the flame we get what is called the lime-light. Now with all the beauty and intensity of action which you perceive, there is no sensible deterioration of the lime except by the mechanical force of the current of gases running from the jet against the lime, sweeping away such particles as are not strongly aggregated. "Vapour of lime" some call it, and it may be so, but there is no other change of the lime than that under the action of heat of this highly-exalted chemical condition, though almost any other substance would melt at once.

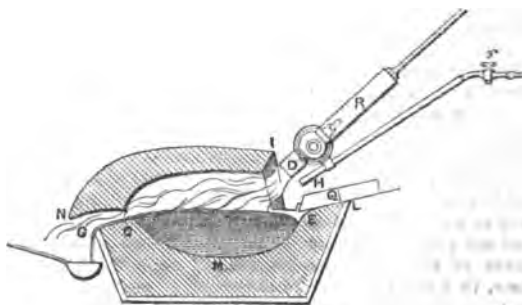
Then as to the way in which the heat is applied to the substance. It is all very well for me to take a piece of antimony, and fuse it in the flame of a blow-pipe. But if I tried this large piece in the ordinary lamp flame, I should do nothing; if I tried a smaller piece I should do little or nothing; and if I tried a still smaller piece I should do little or nothing; yet I have a condition which will represent what Deville carries to the highest possible extent, and which we all carry to the highest extent, in the use of the blow-pipe. Suppose I take this piece of antimony: I

shall not be able to melt it in that flame of the candle by merely holding it there; yet, by taking pains we can even melt a piece of platinum there. This is a preparation which I made for the purpose of proving the fusibility of platinum in a common candle. There is a piece of wire drawn by that ingenious process of Dr. Wollaston's, not more than the three-thousandth part of an inch in diameter. He put the wire into the middle of a cylinder of silver, and drew both together until the whole compound was exceedingly thin; and then he dissolved away the silver by nitric acid. (This is some that he gave me himself.) There was left in the centre a substance which I can scarcely see with an eye-glass, but which I know is there, and which I can make visible, as you see, by putting it into the candle where the heat makes it glow like a spark. I have again and again tried this experiment up-stairs in my own room, and have easily fused this platinum wire by a common candle. You see we have, therefore heat enough in the candle as in the voltaic battery or in the highly exalted combustion of the blow-pipe, but we do not supply a continuous source of heat. In the very act of its becoming ignited the heat radiates so fast that you cannot accumulate enough to cause the fusion of the wire except under the most careful arrangement. Thus I cannot melt that piece of antimony by simply putting it into the candle; but if I put it upon charcoal and drive the fiery current against it, there will be heat enough to melt it. The beauty of the blow-pipe is that it sends hot air (making hot air by the combustion of the flame) against the thing to be heated. I have only to hold the antimony in the course of that current, and particle by particle of the current impinges upon the antimony, and so we get it melted. You now see it red hot, and I have no doubt it will continue to burn if I withdraw it from the flame and continue to force the air on it. Now you see it burning without any heat but that of its own combustion, which I am keeping up by sending the air against it. It would go out in a moment if I took away the current of air from it, but there it is burning, and the more air I give it by this or any other action the better it is. So then we have here not merely a mighty source of heat, but a means of driving the heat forcibly against substances.

Let me show you another experiment with a piece of iron: it will serve two purposes,—showing you what the blow-pipe does as a source of heat, and what it does by sending that heat where it is wanted. I have taken iron in contrast with silver or other metals, that you may see the difference of action, and so be more interested in the experiment. Here is our fuel, the coal gas; and here our oxygen. Having thus my power of heat, I apply it to the iron, which, as you see, soon gets red hot. It is now flowing about like a globule of melted mercury. But observe, I cannot raise any vapour: it is now covered with a coat of melted oxide, and unless I have a great power in my blow-pipe, it is hardly possible to break through it. Now, then, you see these beautiful sparks: you have not only a beautiful kind of combustion, but you see the iron is now driven off, not producing smoke, but burning in a fixed condition. How different this is from the action of some other metals,—that piece of antimony, for instance, which we saw just now throwing off abundance of fumes. We can of course burn away this iron by giving plenty of air to it; but with the bodies which Deville wants to expose to this intense heat he has not that means; the gas itself must have power enough to drive off the slag which forms on the surface of the metal, and power to impinge upon the platinum so as to get the full contact that he wants for the fusion to take place. We see here, then, the means to which he resorts: oxygen, and either coal gas, or water gas, or pure hydrogen, for producing heat, and the blow-pipe for the purpose of impelling the heated current upon the metals.

I have two or three rough drawings here representing

the kind of furnaces which he employs. They are larger, however, than the actual furnaces he uses. Even the furnace in which he carries on that most serious operation of fusing 50 lbs. of platinum at once is not much more than



half the size of the drawing. It is made of a piece of chalk below and a piece of chalk above. You see how beautifully chalk sustains heat without altering in shape; and you may have thought how beautifully it prevents the dissipation of the heat by its very bad conducting powers. While the front part of the chalk which you saw here was so highly ignited, I could at any moment touch the back of it without feeling any annoyance from the heat. So, by having a chamber of chalk of this sort, he is able to get a vessel to contain these metals with scarcely any loss of heat. He puts the blow-pipes through these apertures and sends down these gases upon the metals, which are gradually melted. He then puts in more metal through a hole at the top. The results of the combustion issue out of the aperture which you see represented. If there be strips of platinum he pushes them through the mouth out of which the heated current is coming, and there they get red hot and white hot before they get into the bath of platinum. So he is able to fuse a large body of platinum in this manner. When the platinum is melted he takes off the top and pours out from the bottom piece, like a crucible, and makes his cast. This is the furnace by which he fuses his 40 lbs. or 50 lbs. of platinum at once. The metal is raised to a heat that no eye can bear. There is no light and shadow, no chiaroscuro there; all is the same intensity of glow: you look in and you cannot see where the metal or the chalk is; it is all as one. We have, therefore, a platform with a handle, which turns upon an axis, that coincides with the gutter that is formed for the pouring of the metal; and when all is known to be ready, by means of dark glasses, the workmen take off the top piece and lift up the handle, and the mould being then placed in a proper position, he knows that the issue of the metal will be exactly in the line of the axis. No injury has ever happened from the use of this plan. You know with what care it is necessary to carry such a vessel of mercury as we have here, for fear of turning it over on one side or the other; but if it be a vessel of melted platinum the very greatest care must be used, because the substance is twice as heavy; yet no injury has been done to any of the workmen in this operation.

I have said that Deville depends upon intense heat for carrying off vapour, and this brings me to the point of showing how vapours are carried off. Here is a basin of mercury, which boils easily, as you know, and gives us the opportunity of observing the facts and principles which are to guide us. I have here two poles of the battery, and if I bring them into contact with the mercury see what a development of vapour we have. The mercury is flying off rapidly, and I might, if I pleased, put all the company around me in a bath of mercury-vapour. And so, if we take this piece of lead and treat it in the same way, it will also give off vapour. Observe the fumes that rise from it, and even if it was so far enclosed from the air

that you could not form any litharge you would still have those abundant fumes flying off. I may also take a piece of gold, and show you the same thing. I have here a piece of gold which I put upon a clean surface of Paris limestone. Applying the heat of the blow-pipe to it, you see how the heat drives off the vapour, and if you notice at the end of the Lecture you will observe on the stone a purple patch of condensed gold. Thus, you see, a proof of the volatilisation of gold. It is the same with silver. You will not be startled if I sometimes use one agent and sometimes another to illustrate a particular point: the volatility of gold or silver is the same thing, whether it be effected by the voltaic battery, or by the blow-pipe. [A lump of silver was placed in a charcoal crucible between the poles of a voltaic battery.] Now, look at the fumes of silver, and observe the peculiar and beautiful green colour which they produce. We shall now show you this same process of boiling the silver, cast on a screen from the electric lamp which you have before you, and while Dr. Tyndall is kindly getting the lamp ready for this purpose, let me tell you that Deville proposes to throw out all these extraneous things that I have spoken of, except two, namely, iridium and rhodium. It so happens, as he says, that iridium and rhodium do make the metal more capable of resisting the attacks of acids than platinum itself. Alloys are compounded up to 25 per cent. of rhodium and iridium, by which the chemical inaction of the platinum is increased, and also its malleability and other physical properties. [The image of the voltaic discharge through vapour of silver was now thrown upon the screen.] What you have now on the screen is an inverted image of what you saw when we heated the silver before. The fine stream that you see around the silver is the discharge of the electric force that takes place, giving you that glorious green light which you see in the ray, and if Dr. Tyndall will open the top of the lamp you will see the quantity of fumes that will come out of the aperture, showing you at once the volatility of silver.

I have now finished this imperfect account. It is but an apology for not having brought the process itself before you. I have done the best I could under the circumstances, and I know your kindness well, for if I were not aware that I might trust to it, I would not appear here so often as I have done. The gradual loss of memory and of my other faculties is making itself painfully evident to me, and requires, every time I appear before you, the continued remembrance of your kindness to enable me to get through my task. If I should happen to go on too long, or if I should fail in doing what you might desire, remember it is yourselves who are chargeable by wishing me to remain. I have desired to retire, as I think every man ought to do before his faculties become impaired; but I must confess that the affection I have for this place and for those who frequent this place, is such that I hardly know when the proper time has arrived.

CHEMICAL SOCIETY OF DUBLIN.

At the last meeting of this Society the following paper was read "*On the Non-Existence of a Bicarbonate of Lime in Carbonated Waters*," by EMERSON J. REYNOLDS, Assistant Chemist to the Society.

In the treatises on chemistry it is generally asserted that bicarbonate of lime is formed when the carbonate of that base is dissolved in water containing carbonic acid, but its existence has not been proved, neither has any one determined the actual amount of gas required to dissolve a given weight of the pure carbonate of lime. With the view of proving whether a bicarbonate of lime is really formed under the circumstances mentioned, the experiments described as follows were undertaken:—Carbonic acid

gas, properly dried was conducted into the first, or larger flask, in the alkali-metrical arrangement of Fresenius and Will, which contained 1000 grains of pure lime-water, the strength of which had been previously accurately determined; this flask being connected in the usual manner with the smaller ones, containing strong sulphuric acid, through which the excess of gas had to bubble, and consequently any moisture that was carried over was retained, and in this way loss was prevented. The two flasks were carefully weighed before commencing the experiment. When carbonic acid gas first passes through lime-water carbonate of lime is formed, and by continuing the slow stream of gas, and frequently agitating the contents of the flask, re-solution is effected; then, after drawing a stream of dry air through the apparatus in the usual way, and re-weighing, the increase in weight of the amount of gas absorbed by the lime-water, for the purpose of forming the neutral carbonate of lime and of dissolving it again, will be ascertained.

The next stage of the experiment consists in manipulating, as in ordinary alkali-metrical determinations; again drawing a stream of dry air through the apparatus, and weighing; the loss will give the amount of carbonic acid used in the first instance, and act as a check on the former results.

Having described the method of conducting the experiment, I will now speak of the results which I have obtained. Before doing so, however, let me draw your attention to the advantage of using lime-water instead of the anhydrous carbonate of lime. In the first place, it is much easier to dissolve a moist precipitate than a dry powder in any menstruum. And, secondly, the quantity of water employed, together with the time which should elapse before complete solution could be effected, would be more than sufficient to dissolve a quantity of gas, which might lead to the supposition that a bicarbonate of lime was formed—not that I mean to say that the amount of lime-water I use is not quite enough to lead to the same idea, but having the carbonate of lime in this easily soluble condition, and in consequence of the shaking, the water has only time to dissolve just sufficient gas to cause complete solution to take place, and no more; at least such are the conclusions my experiments seem to warrant. Like most other cases of solution it is much facilitated by agitation.

I give the following results of four experiments which I have selected from several others, as showing the greatest difference:—

Experiment	Increase in weight on re-dissolving precipitate.	Amount of CO ₂ required to dissolve 2.5 gra. CaO, CO ₂ .	Degree of agitation and time.
No.			
1	1.390	.290	Continuous, 4 m.
2	1.400	.300	Nearly the same.
3	1.530	.430	Less, 8 min.
4	2.300	1.200	None, 18 min.

The quantity of lime-water used in each experiment was 1000 grains, and the quantity of actual lime present was 1400 grains, the same being used in all.

Now, 1400 grains of lime would require to form with it a neutral carbonate, 1.1 grains of carbonic acid, so that having found what the increase in weight is in each experiment, the quantity 1.1 must be deducted; the remainder will be the amount of gas required to effect the solution of 2.5 grains of carbonate of lime. It will be easily seen from the above table that the amount of gas absorbed varies according to the degree of agitation. In order to explain what I mean, it will be necessary to go back for a moment to the table given above; it will be seen from this that the time employed in performing each experiment is greater as the agitation is less, and *vice versa*; for in experiment No. 1 the time occupied in dis-

solving the precipitate was only four minutes, whereas in experiment No. 4 it required nearly eighteen minutes to effect the same purpose, but in the latter case no agitation was used, further than was caused by the slow passage of bubbles of gas.

There is another experiment which I tried, namely, the action of a solution of litmus on water impregnated with gas, and on water containing carbonate of lime held in solution by exactly the same quantity of gas as was contained in the other. This appears to be a very easy experiment, but it is in reality very difficult to obtain accurate results with it, in consequence of the fact, that the litmus does not change at once from a red to its natural purplish colour, but goes through intermediate gradations of shade which requires considerable practice to enable the operator to recognise.

I find that the best way to proceed is the following: Take both solutions and place them opposite to a sheet of white paper; then keeping the eye a little above the level of the liquid, add the standard solution of litmus, until, on shaking, a purple tint is noticed; then treat the second solution in the same way, until the same colour is produced, each time, of course, reading off the quantity of litmus solution used. After frequent practice in this way, the eye becomes accustomed to a change of colour.

Now, it is evident, that if all the carbonic acid present were in combination with the lime that it would affect the colour of the test much less than if the carbonate of lime had mechanically dissolved by a portion of this acid. I tried this in the following way: Having charged in a gasogene pure distilled water with carbonic acid gas, I took some of this solution and left it exposed to the air for three or four hours, in order to allow the excess of gas to escape; 1000 grains of this water were placed in a bottle and three grains of pure carbonate of lime were dissolved in it, the solution of the salt being aided by a violent agitation of the fluid. A like quantity was put into another bottle by itself. On adding, from a burette, the infusion of litmus, eighty-four measures were used before the purple tint was observed in the bottle containing the carbonate of lime; now the test was added to the bottle which had the aqueous solution of carbonic acid in it, when eighty-five measures were used. I repeated this experiment upwards of thirty times, with nearly the same results. Three times the quantities were just the same. From the results of these experiments, I think I may conclude that, under the circumstances mentioned a bicarbonate of lime is *not* formed, but that what is termed so is merely a solution of CaO CO_2 in CO_2 . Indeed, there are many cases in which the presence of an acid favours the solution of a substance insoluble, or nearly so, in distilled water. There is no instance in chemistry of a bicarbonate being so loosely constituted as to lose one equivalent of its acid by mere exposure to the air; even magnesia, which is known to exist as a bicarbonate, both in solution and in the solid state, loses only one-fourth of its carbonic acid, and is deposited in the state of sesquicarbonate, as has recently been shown.

CORRESPONDENCE.

Copper in Bread and Flour.

To the Editor of the CHEMICAL NEWS.

SIR,—It is contrary to the usual practice for a scientific man to notice anonymous correspondents, because truthful statements need no fictitious names; but as "Verax" insinuates improper motives, and exaggerated and inaccurate statements against parties, more particularly myself, in regard to my article on Copper in Bread, &c., I beg to observe that "Verax" might with equal propriety doubt or deny the statement of Professor Voelcker, in his paper

read before the last British Association meeting, wherein he noticed the detection of copper and zinc in Gloucester and other kinds of cheese. Sulphate of copper, to my knowledge, being used to impart a factitious appearance of age to new cheese. It cannot be that a person like Dr. Voelcker, so familiar as he is with chemistry, is mistaken in the production of copper, any more than myself in my experiments on bread and flour, as will appear from the mode of procedure, which is simple and intelligible enough. The bread was incinerated in an earthenware crucible, and kept at a red heat for several hours, the black residue was finely powdered, digested at a boiling heat in distilled water, acidulated with pure nitro hydrochloric acid, and passed through filtering-paper (which was ascertained to be perfectly pure and free from copper), when a golden yellow-coloured solution, with a greenish tinge, was obtained, which, on being supersaturated with ammonia, developed the usual characteristic blue colour of copper, and on standing a precipitate of peroxide of iron was formed. The same result was obtained in operating upon flour which came from the same person who supplied the bread. These are the facts of the case, and may be taken for what they are worth. I never sought them, for they came to me in the ordinary course of things, nor is it my business to prosecute or publish the names of parties.

I am aware, as observed by "A. P.," in his letter to your Journal of the same date, that it is usual to steep wheat in solutions of sulphate of copper previous to sowing, but I am not aware that copper ever grows up with the plant as to obtain in the grain, and consequently in the flour, more particularly as the sulphate was detected in the flour by the microscope, which leaves no doubt as to how it came there, or the purpose for which it was used—viz., to improve bad flour.—I am, &c.

Cheltenham.

JOHN HORSLEY.

The New Palace of Westminster.

To the Editor of the CHEMICAL NEWS.

SIR,—In your notice of M. Szerelmey's pamphlet in the CHEMICAL NEWS of February 23, I think that the decay of the stone of the Westminster Palace has been a little exaggerated. The week before last I had the pleasure of going round the Houses of Parliament with some scientific friends, and I, for one, was surprised to see the very slight indications of decay—the size of the building being taken into consideration.

On the river front, which is supposed to be in the worst condition, there are very few stones more than 12 or 15 feet from the ground that are in a decaying state, but below this a considerable number are unfortunately crumbling away. It is remarked that on the river front there are very few places where any of the highly-wrought stone is in the least touched. Can this be accounted for in any other way than by supposing that the sculptor rejected for his work any stone that was not of the very best quality? In Old Palace Yard, some of the stones have given way; but in that immense structure, the Victoria Tower, there is an extremely small number decayed. This may certainly be due to the fact that it was the last portion erected.

It has frequently been stated that the atmosphere of London has not a very beneficial effect upon stone. Perhaps not; but if it were the atmosphere, charged with sulphuric and carbonic acids, which really destroyed the stone, is it not reasonable to expect that the whole of the building would have been equally attacked? It has been affirmed, on good authority, that if the rain-water containing sulphuric and carbonic acids, once gets into the stone, and it begins to decay, there is no stopping it, even if the access of more water be prevented by a preservative. I confess I do not understand this,—for if the sulphuric acid come in contact with the stone, which is the secondary

carbonate of calcium and magnesium, it is only rational to suppose that a corresponding sulphate would be produced, which could scarcely have any deleterious effect on the unaltered stone. On the contrary, it is said that at the upper part of the building there are several stones that were put up in good condition and which commenced to decay, but after a time this decay stopped of its own accord, and the stone is now in a perfectly sound state: the same thing may perhaps occur with that which is so bad in other parts.

After all, I think we may come to the conclusion, that considering the immense size of the edifice, it is not much, if at all, worse than other buildings. The Museum of Practical Geology is often pointed out as an example of the manner in which dolomite ought to stand; but it is also true, that even there there are one or two stones decaying. I could mention a church in Kent, in which there is a doorway on the south side which, I believe, was erected toward the end of the Perpendicular Period of architecture, but in which scarcely any of the mouldings can be traced; and almost all old buildings are in the same state. There are a few stones even in Henry VII.'s Chapel at Westminster which are decaying already, although that has not been restored very many years. It has been computed by some eminent architects, that not more than one stone in 500 in the immense Palace of Westminster is affected, and that those that are so may be easily replaced.

One word with regard to the preservatives. That of the Water Glass Company alters the appearance of the stone least, but does not seem to have arrested the decay. Mr. Ransome's process appears to stop the decay, but leaves in some places a slight whitish appearance that at a short distance looks like mildew. M. Szerelmey's preservative is very much like paint, and of course protects the stone, but at the total sacrifice of its appearance. Mr. Daine's process appears to come under the category of the "greasy" and "dirty" processes, as the oil before it is quite dry becomes covered with some of the carbonaceous matters of the London atmosphere: it appears to protect the stone as it renders it impervious to water.

However, let us comfort ourselves with the hope that this magnificent building may remain as a monument of the skill of one of our most distinguished modern architects, and that the "cruel fate" that has been predicted for it may be averted.—I am, &c. HERBERT McLEOD.

Chemical Notices from Foreign Sources.

ORGANIC CHEMISTRY.

On Cyclamin and the Tuber of *Cyclamen Europæum*, L.—Dr. T. W. C. Martius has published, in *Buchner's N. Repert.*, viii. 388—395, an account of his experience in preparing cyclamin. The author recommends the following method:—Six pounds of the dried tubers, collected in the fall, are reduced to a coarse powder, mixed with two pounds of animal charcoal, digested for twenty-four hours in a still with six gallons of alcohol of at least .825 specific gravity, then boiled, the distilled alcohol being returned into the still, and expressed while yet warm; the residue is treated in the same manner with four and a-half gallons alcohol, and the mixed tinctures, after having been filtered, are distilled until one-fourth is left behind, which residue is set aside for six or eight weeks. The precipitate is then washed upon a filter with alcohol until it passes colourless. The filtrate and the washings are mixed and evaporated to three or four quarts. In a few weeks more cyclamin will have separated; after washing, it is added to the portion first obtained. The whole is then mixed with six ounces of animal charcoal, and treated with four pints of boiling alcohol. On standing over night, the filtrate deposits most of the cyclamin; the liquid portion is distilled or evaporated to

six or eight ounces, and will separate some more cyclamin after several days.

Cyclamin is a white, granular powder, uncrystallisable, soluble in alcohol and water. The aqueous solution is fluorescent, and coagulates on boiling, but the coagulum re-dissolves in the liquid; insoluble in ether, chloroform, glycerine, and caustic alkalies. The solutions are neutral to test-paper. They are precipitated white by nitrate of silver, acetate and subacetate of lead, and bluish-white by sulphate of copper. When boiled with diluted acids, it yields glucose in solution and a white granular precipitate, *cyclamiretin*, insoluble in water and ether, but soluble in alcohol.

According to Dr. Klinger's analysis, cyclamin is composed of $C_{10}H_{21}O_{20}$; *cyclamiretin* = $C_{22}H_{16}O_{12}$.

In the *Zeitschrift der Aerzte zu Wien*, Professor Dr. G. Schroff has published the results of his experiments with cyclamin and the root of *Cyclamen Europæum*. The following is selected from an abstract of that paper in *Buchner's N. Repert.*, viii. 452—459:—

Cyclamin is amorphous, white, of a disagreeable, acrid taste, persistent in the throat. Its aqueous solution foams like soap-water. It coagulates between 60° and 75° C. (140° and 167° F.)

From their experiments, De Luca and Bernard came to the conclusion that cyclamin had properties similar to curara. Pelikan thinks it an acrid poison. The following are the most important of Professor Schroff's results:—

Cyclamin is a poison; it is the most powerful, but not the only active constituent of the root. The well-dried root may be preserved for several years without losing its efficacy. It is without any influence on the skin. It produces burning, nausea, and vomiting, but no inflammation of the mucous membranes of the mouth and stomach. Introduced into the respiratory organs, it absorbs hæmatin from the blood, and produces intense inflammations; also hyperinosis. It causes intense inflammation in the rectum, the peritoneum, and when introduced below the skin. It exerts only a passing influence on the cerebro-spinal system, when introduced directly into the circulation. Its action is directed towards the salivary glands. After being absorbed, it probably alters the composition of the blood; it is uncertain yet in what state and by what organs it is excreted. In its properties it is similar and stronger than smilacin, senegin, and saponin; gastritis excepted, it also resembles emetis, colocynthin, elaterin, and colchicia, but is weaker. It differs essentially from the narcotic and acrid-narcotic poisons.

The juice of the root of *Cyclamen Europæum* has only the properties of cyclamin. The quality of the action of the fresh root is the same as of the dried tuber, but in quantity it is less, being one-third of the latter. The alcoholic extract has the same qualitative properties, but is double the strength of the dried root. The aqueous extract is almost inert; a boiling temperature injures the activity. Besides the symptoms produced by cyclamin, the root and alcoholic extract cause gastritis.

Cyclamen is a fish-poison; its juice, coming into contact with their respiratory organs, forces the fishes to seek the surface of the water, and ultimately kills them.

LABORATORY MEMORANDA.

Concentration of Alcohol in Bladders.—It is generally admitted that an ordinary bladder is permeable to the vapour of water and impermeable to that of alcohol. That the latter cannot, however, be concentrated in this manner appears to be shown by the following laboratory memoranda:—March 14, 1860.—12½ of spirit of wine, specific gravity .830, were placed in a clean bullock's gut bladder, which was then securely tied and hung up in the laboratory. Nov. 23, 1860.—The whole had evaporated.

103 of spirit of wine, specific gravity .840, were exposed in the same way. Dec. 17, 1860.—The specific gravity was .870. Jan. 22, 1861.—The specific gravity was .873. It then measured 413, and left, on evaporation, a slight residue of animal matter extracted from the bladder, appreciable to the taste, but not sufficient to account for the increased specific gravity. It is submitted, then, that alcohol cannot, as has been assumed, be thus prepared in the pure state, for the spirits of wine permeates the bladder and evaporates, becomes weaker and not stronger, and takes up a little extractive matter.—EDWARD C. C. STANFORD, F.C.S.

MISCELLANEOUS.

Chemical Society.—At the next meeting of this Society, on March 7, Professor Field will read a paper "On some New Minerals from Chili."

Royal Institution.—The following is the arrangement of Lectures for the ensuing week:—Tuesday, March 5, at 3 o'clock, Professor Owen "On Fishes." Thursday, March 7, at 3 o'clock, Professor Tyndall "On Electricity." Friday, March 8, at 8 o'clock, Dr. E. Frankland "On Inorganic Chemistry." Saturday, March 9, at 3 o'clock, Dr. E. Frankland "On Inorganic Chemistry."

Interpretation of the Adulteration Act.—According to the doctrine enounced in the Sheriffs' Court of London, impure articles are not, legally speaking, adulterated, unless they contain foreign ingredients injurious to health,—a bad precedent certainly, inasmuch as "a fraudulent act" is overlooked. This theory was laid down in a case where compensation was sought from a tradesman who sold as mustard a compound formed of that article combined with flour and turmeric. The question is, whether, by prohibiting adulteration, the Legislature has forbidden the sale of impure articles. Referring to the Act, we find in the preamble the word "fraud" specified as foremost; but in the first clause there are two points for consideration, viz.: "Every person who shall sell any article of food or drink with which, to the knowledge of such person, any ingredient or material injurious to the health of persons eating or drinking such article has been mixed (and every person who shall sell as pure or unadulterated any article of food or drink which is adulterated or not pure), shall for every offence," &c. The final clause, however, of the Act appears plainly to imply that the practice of adulteration, by dilution or otherwise, with the most harmless substances, was supposed to be prohibited, for it contains the words—"This Act shall not be construed so as to affect the ordinary reduction of the strength of foreign, British, or Colonial spirits by persons licensed and paying duties under the Excise." It is manifest, therefore, that this proviso would not have been required in an Act under which the custom of reducing mustard by flour was allowable, for the reduction of spirits by an admixture of water is a precisely analogous process; and if that requires a special proviso for its protection, the law must be held to forbid such a practice in general. This is the only interpretation any ordinary person can arrive at. If legal authorities are to decide otherwise, the value of the Act is in a great measure destroyed. Who can then object to watered milk or degenerated butter, if floured mustard is to pass as "lawful merchandise"? Provided that tradesmen only select innocuous things, they may mix, corrupt, and adulterate as freely as they please with perfect impunity (a nice state of things to have in view!). No one asked for an Act to prevent poison being sold as food. What the public wanted was a measure that should compel tradesmen to sell in its genuine state any article that might be asked for; instead of which, we have obtained an Act

which admits of flour being vended as mustard, water as milk, spirits, &c., horse-beans as coffee, and any innocent kind of foliage as tea. The sooner this state of things is remedied the better.—*Birmingham Journal*.

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the *CHEMICAL NEWS*, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 6d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part I. of the present Volume is now ready.

"* In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

"* All Editorial Communications are to be addressed to Mr. CROOKES, and Advertisements and Business Communications to the PUBLISHERS, GRIFITH, BOHN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the *CHEMICAL NEWS*, containing a copious Index, is now ready, price 12s., by post, 12s. 6d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

Mr. Wentworth Scott's letter on Adulteration of Food is in type, but its insertion is unavoidably delayed until next week.

Pennsylvania Oil.—R. S. G. wishes to know where this can be obtained in England.

A Constant Reader.—The specific gravity of the nitric acid used in making nitro-benzol should be 1.5. Usually, however, a mixture of weaker acid, with oil of vitriol, is employed.

A Subscriber.—We believe there is a liquid employed in the arts under the name of "Decoction of Beech," but we are not acquainted with its properties. Cinchonine can be obtained at an operative chemist's.

Red Colour of Gravel.—A correspondent writes, "The bright red colour of gravel is due, I believe, to the presence of iron in a highly oxidised state. This colour, as you are no doubt aware, flies by exposure to the air and elements. Can any of your readers say whether there are any means of restoring the colour by chemical agency, and if so, what should be used, and how applied?"

A B.—We do not know what is the composition of *Florentine Bronze*. A good bronze can be made for copper or brass by taking one drachm of sal ammoniac, fifteen grains of oxalic acid, and a pint of vinegar. After well cleaning the article to be bronzed, warm it gently, and brush it over with the liquid, using only a small quantity at a time. When rubbed dry repeat the application till the desired tint is obtained.

J. A. Davies.—We regret that our correspondent's communications are of too theoretical a nature to warrant our displacing other matter to give them insertion in our columns. We have, therefore, with many thanks, returned them as requested.

Dupré's New Metal.—At the moment of going to press we have received a communication from Mr. C. Greville Williams, stating that he also has observed a blue line, and finding that it is invariably present with lime, has concluded that it is not due to a new metal, but is merely a reaction of calcium.

A Subscriber, F. H.—We cannot at present give the Professor's address. Any large philosophical instrument maker would be able to give you some information about the apparatus. The charge will be somewhat higher than it is for the usual binding, as only two are to be done. It will, however, not exceed 2s. 6d. each volume if they are sent to our office with necessary instructions. Your plan of evolving the mixed gases by electrical means in a strong copper globe under pressure, and then burning them with suitable arrangements, is open to the objection of expense. It would cost ten times as much to produce H and O in that way as to prepare them by chemical means. In other respects the apparatus you describe would be good, provided it were made strong enough to bear the pressure which you speak of (35 atmospheres), and the gases passed through water or mercury safety tubes before being burned. Acidulated water should be used in the generator.

Erratum.—In No. 63, page 105, top line, for *Gashorne* and Co. read *James Thorne* and Co.

THE CHEMICAL NEWS.

VOL. III. No. 66.—March 9, 1861.

THE ADULTERATION OF FOOD AND DRINK.

CHEMISTS have an interest in the Act passed for the Prevention of Adulteration of Food and Drink beyond that of any other portion of the public. They are liable to suffer by the fraudulent treatment of these substances just as much as other people, and it opens a number of appointments in the public service which are almost the only ones for which a chemist is eligible. Hitherto, neither chemists nor the public at large have benefited by the Act, a result we predicted when the Bill was first introduced into the House of Commons. As it appears to be the general impression that the able Member who introduced the Bill proposes to introduce another during the present Session of Parliament, we will take the Act as it stands, and offer a few remarks on the several clauses it comprises, and suggest some alterations which appear to us necessary to render it effective.

Clause I. gives Justices the power of inflicting a penalty not exceeding £5, and the costs attending his conviction, on any person who shall sell any article of food or drink adulterated with a substance injurious to health, or as being genuine when it contains a foreign substance not supposed to be injurious to health—provided the vendor is aware of the adulteration. For a second offence, the Justices may cause the name, address, and occupation of the offender to be published in any way they may consider desirable. This clause, which requires that the vendor shall be aware of the adulteration, appears to us to render the Act nugatory at the very outset. Who is to prove he had such knowledge if he denies that he had? The only person capable of furnishing such proof must be one of his own servants; and even supposing this evidence to be obtained, we doubt very much whether it would ensure a conviction, as Englishmen have, and rightly so, a horror of the system of delation; and this would be delation of the worst kind, short of compelling a woman to betray her husband. The sale of articles already adulterated by the wholesale dealer occurs with sufficient frequency to render it almost impossible for a Justice to convict in the face of the retailer's denial of a guilty knowledge, however strong his impression may be that the guilty party is before him. The remedy for this is obvious: let the vendor be convicted when the evidence is clear enough to establish the charge of adulteration, without entering into the question of his knowledge at all. At the same time, we would not prevent him from obtaining prompt justice whenever it happened that he was innocently condemned; and this is easily provided for by giving him the privilege of suing the wholesale dealer in the County Court for costs and damages. As to the mode of giving publicity to a second offence, we think it would be preferable to relieve the Justices of the odium they would incur by adopting what we conceive would be the only effectual one—viz., by compelling the offender to pay for an insertion of an advertise-

ment of his offence in a newspaper, and to affix a printed copy of it on a conspicuous part of his shop window for a certain number of days,—by making it the legal punishment. Moreover, some such enactment will be found absolutely necessary, otherwise offending parties may complain that the penalties inflicted are of an arbitrary character; that whereas A was sentenced to pay a fine of £5, and to affix a printed account of his offence and conviction in his window for twenty days, and to pay for the insertion of an advertisement in a newspaper, B, for a precisely similar offence, only had to pay the fine and for the advertisement, and escaped the real penalty altogether.

Clauses II and III. give certain bodies power to appoint Analysts possessing competent medical, chemical, and microscopical knowledge in their respective city, districts, counties, or boroughs, with such salary or allowances as they may think fit, subject to the approval of one of Her Majesty's principal Secretaries of State, whose sanction must also be obtained before an Analyst can be removed from his post. Also that the purchaser must prove to the satisfaction of the Justice who hears the case that he told the seller he meant to have the substance analysed, and gave him an opportunity of accompanying him (the purchaser) to see it placed in the hands of an Analyst, so as to secure it from being tampered with.

The grand defect here is that it simply permits vestries, district boards, &c., to appoint Analysts. The Act does not say to vestries, district boards, and so forth, "On and after a certain day you *shall* appoint an Analyst," &c., but you *may* appoint. The result of this mild species of legislation has been precisely what we anticipated when the Bill was first brought forward (see CHEMICAL NEWS, February 18, 1860), and we trust that Mr. Scholefield, or some other independent Member, will take an opportunity of moving the Government for a return of the number of appointments of Public Analysts that have been made. Of course we can only guess at the number of such appointments by what we have seen in different newspapers, but we have a strong impression that if the salvation of the population of the United Kingdom depended on twenty official Analysts being found within its borders, we should all be lost. The reason is clear enough. Vestrymen are, for the most part, tradesmen. Even where the Vestry contains members who are not traders, but men of independent fortune, inspired by a laudable desire to serve their fellow-parishioners in their local Parliament, by regulating the number of their gas lamps, examining the state of their roads, and watching over the salubrity of their domestic arrangements, these, being mostly dependent on the minor class of tradesmen for their re-election, are, naturally, reluctant to give offence to their constituents. If it were made compulsory on such bodies, we believe they would not be very unwilling to make the appointment; but it is, perhaps, rather too much to expect them to do so voluntarily.

As regards the salary to be paid to Analysts. This is a difficult question. If the efficient working of the Act is to be considered before all things, then the best method of accomplishing this will be to give the Analyst the services of a parish official to purchase as many specimens of food and drink as he may direct, and allow him to receive the fines. By this means the Analyst, as every chemist knows perfectly well, will be able to get through a great deal more work; as, instead of turning his attention from an analysis of coffee to one of tea, then to one of sugar, and then back again to coffee, he will deal with a good number of specimens of each of these substances in succession, and will act under the stimulus of greater pay for a greater amount of work. Such a method of remuneration would not be so agreeable to a gentleman as a fixed salary, but as the interests of the public are to be considered before the feelings of its servants, we think it would be the best to adopt, and would be by far the most effectual means of putting an end to adulteration. The municipal authorities ought, however, to guarantee him a minimum yearly salary for his services—that is to say, where the amount derived from fines is less than a sum agreed upon, the deficiency should be made up from the parish funds; because, under this method of working the Act, it is only reasonable to suppose that the practice of adulteration would be greatly checked, and, though he might perform as great a number of analyses as at the beginning, he would find fewer fraudulent specimens, so that his labour would remain the same while his reward would go on diminishing—at least, we confidently hope this would be the case.

It seems rather hard to require a tradesman to leave his shop and accompany a stranger a distance of, perhaps, two or three miles to prevent himself from being victimised; but, as the law stands at present, he must do so, no matter how convinced he may be of the genuineness of the article he has just sold, otherwise he would lay himself open to continual extortion. For example, a man comes into his shop and asks for a quarter of a pound of coffee, and when he has got it he tells him that he intends to have it analysed. The grocer is certain that the coffee does not contain any foreign substance, but the purchaser looks a rather doubtful character and may have some chicanery in his pocket. The Analyst lives two or three miles away; nevertheless, he must put on his coat and go with the disreputable-looking stranger or else sends his shopman. But suppose the case of a shopkeeper in a poor neighbourhood who does not keep a shopman, what then? Must he put up his shutters and leave his business for an hour or two, or suffer the purchaser to add an ingredient to the coffee as he goes on his way to the Analyst, that he may return with the certificate which is to be the instrument of his conviction? The probable result of the announcement of the purchaser's intention would, in many instances, be the offer of a bribe to give up the specimens, which would be precisely what was wanted. We have not heard of a case of this kind, but, if none has happened, every Member of the House of Commons will admit that it is only because the idea has not yet occurred to that numerous class who live by their wits, to profit by the new opening which the Legislature has made for them.

We think it would be only just towards the shopkeeper to go a step further in legislation, and compel the purchaser, not only to give notice that he intended to have the substance analysed, but also to allow the vendor to seal it up, or secure it in any way he may

think best calculated to prevent its being tampered with, before it was placed in the hands of the Analyst.

Want of space prevents us this week from passing in detail through the remaining clauses of the Act; but we intend to recur to the subject in an early number.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Colours of Flames.

MERZ has communicated some investigations on the colouration of the flames of Bunsen's burner and of hydrogen gas produced by the presence of various substances. These observations may be considered as supplementary to those of Bunsen, and, although less certain, are simpler, and more easily applied than the processes of the spectral analysis. The author employs a flame of pure hydrogen, as well as that of Bunsen's burner, and in addition, makes use of blue, violet, red and green glasses. The new substances, which he describes as giving characteristic colours to the flame of Bunsen's burner, are nitric, chromic, and molybdic acids, while phosphoric and sulphuric acids give a peculiar colouration to the dark core of the flame of hydrogen.

The flame of Bunsen's burner gives three sorts of colours—*a*. Border colours. These are of course peculiar only to the most volatile substances. To produce them, the loop of platinum wire is to be held outside of the flame, about one or two millimètres from the lower portion of the outer limit. *b*. Mantle colours—those namely which are seen when the substance is held in the bright blue-coloured mantle which forms the outer portion of the flame. *c*. Flame colours. To produce these, the loop is to be held horizontally, and in the hottest part of the mantle. The hydrogen flame yields another species of colour—viz., the core colours. These are produced only by sulphuric and phosphoric acids, which communicate respectively a blue and green tinge to the cold core of the hydrogen flame.

Nitric and nitrous acids give a bronze-green border colour, usually with an orange-coloured border. The test is to be previously dried in the flame, and dipped into a solution of bisulphate of potash, or into dilute muriatic acid, according as we wish to test for nitric or nitrous acid. The sensibility is about $\frac{1}{10}$ milligramme. Compounds of ammonia and cyanogen give the same bronze-green border, but more faintly.

Phosphoric acid gives a grey-yellow-green border colour, as well as a beautiful green core colour. The dry test is to be dipped into sulphuric acid and held in the flame in the manner above pointed out, in order to show the border colour. The sensitiveness is $\frac{1}{8000}$ milligramme. The green core colour is less sensitive, but indispensable in recognising phosphoric acid in the presence of large quantities of boric acid, and is produced by alternately moistening the test with a solution of fluosilicic acid, and holding it till ignition in the hydrogen flame, until the colour distinctly appears.

Sulphuric acid produces a beautiful blue core colour, being reduced to sulphurous acid. Free sulphuric acid gives the colour when the platinum loop is held in the border of the flame, but a sulphate must be held in the middle of the flame. In the latter case, it is well to dip the test into strong muriatic or fluosilicic acid. The sensibility for sulphuric acid is $\frac{1}{1000}$ milligramme, for a sulphate, $\frac{1}{10000}$ milligramme.

Boric acid gives a beautiful green mantle colour,

which is so intense that the acid may be recognised in the presence of large quantities of phosphoric acid. The sensibility is $\frac{1}{1000}$ milligramme. Borates are to be decomposed with sulphuric acid.

Chromic acid gives a dark brownish-red border colour, and a rose-red mantle colour. The sensibility is $\frac{1}{1000}$ milligramme. The dry test is to be moistened with concentrated sulphuric acid, and held in the border. Oxide of chromium gives no colour, and is to be first oxidised to chromic acid by moistening with a solution of hypochlorite of soda, and drying. Molybdic acid gives a yellowish-green flame colour, like that of baryta, which is however very fleeting, and only occurs on moistening the test with muriatic acid.

Muriatic acid gives a very weak greenish-blue border colour, which lasts for a very short time, and therefore does not deserve attention. The acid is, however, decomposed into free chlorine which may easily be recognised.

Potash gives a grey-blue mantle colour and a rose-violet flame colour. These colours appear reddish-violet through the blue glass, violet through a violet glass, and blue-green through a green glass. Potash is recognised in the presence of lithia, as well as through so thick a layer of blue glass that the lithium red is no longer visible. The test is to be moistened with sulphuric acid, and repeatedly exposed to the flame for a short time. The sensibility is $\frac{1}{1000}$ milligramme, for blue, $\frac{1}{1000}$ milligramme for green glass.

Soda gives an orange-yellow flame colour, which in very large quantities appears pure blue, but in small quantities is invisible through the blue glass; through the green glass, the soda flame appears orange yellow, even with the smallest quantities. This glass is particularly adapted to the recognition of soda in all its compounds. The sensibility is $\frac{1}{1000}$ milligramme. The test is to be moistened with sulphuric acid; dried and held in the hottest point of the flame.

Lithia gives a carmine-red flame colour, which appears violet-red through the blue, carmine-red through the violet, but is invisible through the green glass. The test is to be moistened with sulphuric acid and treated like potash. The sensibility is $\frac{1}{1000}$ milligramme. In the presence of soda, lithia is recognised through the blue glass; in the presence of potash, by the method given by Bunsen. In the case of the alkaline earths, the test is to be moistened with sulphuric acid, carefully dried, and held in the hottest point of the mantle. After all the alkalies have evaporated, the earths may be detected.

Baryta may be recognised by the yellowish-green flame colour which appears blue-green through the green glass. The sensibility for baryta alone is $\frac{1}{1000}$ milligramme. If the green disappears and a red flame colour makes its appearance, the test is to be repeatedly moistened with muriatic acid, and immediately introduced while wet into the hottest part of the flame. If the blue-green colour is no longer seen, we proceed to examine for lime.

Lime is present when the red flame colour, on evaporating the last portion of muriatic acid, appears siskin-green through the green glass. Strontia gives in this case a weak yellow. The sensibility of lime alone is $\frac{1}{1000}$ milligramme, but by employing the green glass, $\frac{1}{1000}$ milligramme.

Strontia may be recognised by the purple or rose colour which is seen through the blue glass when the test, after moistening with muriatic acid, is evaporated to dryness in the flame.

Copper as chloride gives an azure-blue zone, and as nitrate, a pure green flame colour. By the combined

observation of both colours, copper may be distinguished from all other metals which give similar colours. The sensibility of copper alone is $\frac{1}{1000}$ milligramme. The other flame-colouring metals, such as arsenic, antimony, tin, lead, mercury and zinc, exhibit, especially in the form of chlorides, more or less intense bluish or greenish mantle-colours, which, however, cannot be advantageously used as reactions for the metals themselves.—*Journal für prakt. Chemie*, No. xvi. 1860.

Researches on Arsenic, by M. LIPPERT.

THIS inquiry relates to Reinsch's process, which consists, as is well known, in extracting arsenic from its hydrochloric solution, by means of metallic copper. The black deposit which results is usually regarded as pure arsenic. Now, M. Lippert has shown, by painstaking researches, that this deposit contains only 32 per cent. of arsenic, the remainder—that is to say, 68 per cent.—being copper. This is a true alloy, of a definite and constant composition, according to the formula Cu_2As .

When heated in a hydrogen current, this black powder loses a little arsenic, and is transformed into a white, silvery alloy, oxidising slightly in the air, agreeing in its composition, Cu_2As , with *Domeykite*, a mineral found at Copiabo. The great sensitiveness of Reinsch's process arises, then, from the large proportion of copper allied with the deposited arsenic. Once aware of this fact, we can employ this process without inconvenience; for henceforth we shall know that all the arsenic will not be volatilised under the influence of the hydrogen current, and that at least half will remain in the deposit.—*Journal für prakt. Chem.*

On the Action of Oxide of Bismuth on Solutions of Sesquioxides, by M. LEBLAIGUE.

WHEN a solution of a sesquioxide, such as alum, sesquioxide of iron, or chromium, is boiled for a short time—about a quarter of an hour—with excess of oxide of bismuth, anhydrous or hydrated, the following facts are observed:—

The liquid, acid before the reaction, becomes neutral. The sesquioxide is entirely precipitated. The acid with which it was combined forms an insoluble sub-salt, with the oxide of bismuth. The liquid retains no trace of either metal. Thus, when perchloride of iron is used, sensitive tests employed to detect a persalt of iron in a liquid give evidence of no reaction.

This fact, established as respects the salts of sesquioxide of aluminium, iron, and chromium, cannot be shown in the salts of sesquioxide of manganese, on account of the difficulty in preparing them; but there is every reason to believe that the reaction would be the same.

Nothing resembling this reaction takes place when solutions of salts of zinc, protoxide of iron, nickel, cobalt, copper, and lead are operated upon. My researches only justify me in stating that, in operating on rose-coloured salts of manganese, there is sometimes precipitated a brown powder, which apparently consists of sesquioxide of manganese, but the liquid remains rose-coloured. This fact verifies the opinion that the red colour is not due to the presence of a sesquioxide. That in experimenting on a solution of commercial sulphate of copper, there is precipitated a certain quantity of sesquioxide of iron, proceeding from the sulphate of iron, always contained in this sulphate of copper. That when an imperfectly-peroxidised salt of iron is operated upon, the peroxide of iron only is pre-

precipitated, the proto-salt remaining in solution. Oxide of bismuth then allows the separation of sesquioxide and protoxide salts in metallic solutions containing a mixture of the two. On this account, oxide of bismuth is nearly allied to the oxides of copper and lead, which are employed in analyses to effect the separation of iron and manganese.

If we would deduce some considerations from the foregoing facts with reference to chemical statistics, we must remember that, though in the tables of affinities of oxides and acids, as arranged by some authors, they have reserved the last place for bismuth, yet the result of the experiments just recorded show that oxide of bismuth is a more energetic base than is generally supposed, and that it should be placed in the scale of affinity of bases for acids near to copper and lead.—*Journal de Pharmacie et de Chimie.*

Researches on the Platinum Metals, by WOLCOTT GIBBS, M.D., Professor of Chemistry and Physics in the Free Academy, New York.

(Continued from page 131.)

Experiments which I have instituted upon the effects of fusing the ore with steel, phosphorus, arsenic, and sodium, led to no really valuable practical results. It is true that an alloy with steel can be obtained by fusion at a high temperature. Acids slowly dissolve the iron in this alloy, and leave the platinum metals in the form of a black powder which is attacked by nitro-muriatic acid, though not completely dissolved. But the process is tedious, and the results not very satisfactory.¹

Wöhler's method of resolving osmiridium, consists in passing moist chlorine over the ore mixed with common salt and heated to low redness in a glass or porcelain tube. This method is invaluable in analysis, and gives excellent results in working the ore upon a small scale. In all cases, however, several repetitions of the process are necessary for complete resolution or reduction to a soluble form. On the other hand, it can scarcely be doubted that this method could be advantageously employed upon the large scale, if vessels of porcelain of large size and of a proper shape could be obtained. Such vessels might be constructed in the form of long and flattened ellipsoids, furnished at each extremity with wide tubes several inches in length, and would be of great utility in various chemical processes. No process of fusion with oxidising agents compares with Wöhler's method in point of elegance, as no iron or other impurities afterward to be removed, are introduced by the process itself.

Fritzsche and Struve² treat the ore with a mixture of equal parts of hydrate and chlorate of potash, by which a more or less complete oxidation is effected, without any sensible evolution of osmic acid. The temperature required in this process is not high, but large vessels must be employed, as the mixture froths very much at first. The process in question has not appeared to me to possess any sensible advantage over that of Claus, which is moreover less expensive, and can be carried out with smaller vessels.

¹ Since the above was written the elaborate memoir of Deville and Debray on the platinum metals has appeared. (*Ann. de Chimie et de Physique*, 3d, lvi. 385.) I have not however been able to repeat any of the processes for subdividing or oxidizing osmiridium given by these chemists [but consider it probable that the subdivision of the ore accomplished by fusion with zinc may be simpler than that which I have described above.]

² *Pogg. Annalen*, xxxi. 161.

³ *Journal für prakt. Chemie*, xxxvii. p. 483.

Claus's method of resolving the ore consists in fusing for an hour, at a red heat, a mixture of one part of ore with one part of caustic potash and two of saltpetre. The fused mass is to be poured out upon a stone, allowed to cool, broken into small pieces or powdered, and then introduced into a flask, which is to be filled with cold water and allowed to stand for twenty-four hours. The clear deep orange-red solution of osmate and ruthenate of potash is then to be drawn off by means of a siphon, and the black mass remaining again washed in the same manner. The finely-divided oxidised portion of the insoluble matter may now be separated from the unattacked ore by diffusion in water and pouring off, after the subsidence of the heavier ore. The unattacked ore is then to be fused a second time with potash and saltpetre and treated as before. Claus asserts that he has been able in this manner to resolve the Siberian osmiridium completely in two operations.

I have not always been so fortunate with the raw California ore, even when in a finely-divided state. On the contrary, after three or four successive fusions, there usually remained a large quantity of black matter, insoluble in aqua regia. In one operation with 500 grammes of ore, only 200 grammes were rendered soluble by two fusions with potash and saltpetre. I employ at present the method of Claus, with several modifications which I deem essential. The ore, which is usually very impure, is in the first place to be fused with three times its weight of dry carbonate of soda. The fused mass after cooling is to be treated with hot water, to remove all the soluble portions, and then the lighter portions are to be separated by washing from the heavy unattacked ore. In this manner the greater part of the silica and other impurities present may be removed. A previous purification of this kind is not indispensable, and may be omitted altogether when the ore is in plates or large grains, but it is very desirable when the ore is in fine powder, and greatly facilitates the subsequent action of the oxidising mixture. Claus recommends the purification of the ore by boiling with a solution of caustic potash. It is certain that a much larger portion of the ore is resolved by two successive fusions with potash and nitre after previous purification by fusion with soda and washing. By cutting off the top of a mercury bottle a wrought iron crucible is obtained, in which 600 grammes of osmiridium may be fused at one operation with potash and saltpetre as above. There is usually little or no foaming, and if any occur it may easily be checked by stirring with an iron rod. No sensible quantity of osmic acid is given off during the process, which with a little care is entirely free from danger. In this manner, I have worked up 1500 grammes of ore in a few hours in three successive operations.

Claus' method of treating the fused mass to separate ruthenium and osmium is liable to two sources of inconvenience. In the first place, the quantity of water required to dissolve out the soluble portions is very large, and the subsequent treatment of such bulky solutions by distillation with acids, tedious—very large retorts being necessary. In the next place, it is impossible in this way to avoid exposure to the vapour of osmic acid, especially in transferring the solutions from one vessel to another. I therefore prefer the following process, which leaves nothing to be desired in point of safety or convenience. The fused mass is to be broken into pieces with a hammer, and brought into a clean iron pot—a common skillet, with a long handle, answers

⁴ "Beiträge zur Chemie der Platin-metalle." Dorpat, 1854.

this purpose extremely well. Boiling water, containing about one-tenth of its volume of strong alcohol, is then to be added, and the whole is to be boiled over an open fire until the fused mass is completely disintegrated. The osmate of potash is, in this manner, reduced to osmite, KOsO_4 , while the ruthenate of potash is completely decomposed, the ruthenium being precipitated as a black powder—probably a mixture of RuO_2 and Ru_2O_3 , or of the hydrates of these oxides. It is advantageous, after boiling for some time, to pour off the supernatant liquid with the lighter portions of the oxides, and boil a second time with a fresh mixture of alcohol and water. In this manner we obtain a solution of osmite of potash, a large quantity of black oxides, and a heavy black and coarse powder. This last consists chiefly of undecomposed ore, mixed with a small quantity of the oxides of iridium, &c., with scales of oxide of iron from the crucible, and, if the ore has not been previously purified, with the impurities of the ore itself. The greater specific gravity of this residual mass renders it very easy to pour off from it the mixture of black oxides with the solution of osmite of potash and alkaline salts. This solution with the suspended powder is to be poured into a beaker and allowed to settle. The heavy black powder remaining in the iron pot is then to be perfectly dried over the fire, and fused a second time with potash and saltpetre as before. The fused mass is to be treated exactly as after the first fusion. The heavy portions remaining after this operation may be fused a third time with the oxidising mixture. When, however, the ore has been previously purified by fusion with carbonate of soda, or when it was originally in the form of clean scales, the heavy portion remaining after two successive oxidations will be found to consist chiefly of scales of oxide of iron.

The solutions containing osmite of potash and alkaline salts are to be carefully drawn off by a siphon from the black oxides which have settled to the bottom of the containing vessels. The oxides may then be washed with hot water containing a little alcohol, and introduced into a capacious retort. By this process, when carefully executed, no trace of osmic acid escapes—an advantage not to be despised, as the deleterious effects of this body upon the lungs have not been exaggerated, and too much care cannot be taken to avoid inhaling it.

The solution of alkaline salts contains only a portion of the osmium in the ore. The other portion exists in the mixture of oxide, and must be separated by distillation. For this purpose the retort should be provided with a safety-tube, passing through the tubulure, and with a receiver kept cold, and connected by a wide-bent tube with a series of two or three two-necked bottles containing a strong solution of caustic potash with a little alcohol, and also kept cold. All the tubulures and connections must be made perfectly tight. Strong chlorhydric acid is then to be cautiously poured into the retort, through the safety-tube, in small portions at a time. The reaction which ensues is often violent; great heat is evolved, and a portion of the osmic acid distils over immediately, and condenses in the receiver in the form of colourless needles. When a large excess of acid has been added, the action has entirely ceased, and the retort has become cold, heat may be applied by means of a sand bath. The osmic acid gradually distils over, and condenses in the receiver and in the two-necked bottles. Especial care must be taken that the beak of the retort is not too small at the extremity, as it may otherwise become completely stopped up with the condensed osmic acid. The same applies to the

tubes connecting the receivers and two-necked bottles. The distillation should be continued for some time after osmic acid ceases to appear in the neck of the retort; when this has once become hot, the acid condenses, and passes into the receiver in the form of oily drops.

When the distillation is finished, the retort is to be allowed to cool, and then separated from the receiver, which is to be immediately closed with a cork. By gently heating the receiver in a water-bath, the contained osmic acid may be driven over into the two-necked bottles, where it condenses in the alkaline solution, and is reduced by the alcohol to osmite of potash. The solution thus obtained may be added to that obtained directly from the fused mass of ore, and on evaporation in a water-bath and cooling, will yield crystals of osmite of potash, the salt being but slightly soluble in strong saline solutions. The mother liquor from the crystals contains only traces of osmium, and may be thrown away as worthless.

The dissolved portions drawn off from the retort have a very dark brown-red colour. The solution is to be evaporated to dryness, re-dissolved in hot water and again evaporated, after adding a little chlorhydric acid, and this process repeated till no smell of osmic acid can be perceived. A cold and saturated solution of chloride of potassium is then to be added in large excess. This dissolves the chlorides of iron and palladium which may be present, leaving platinum, iridium, rhodium, and ruthenium as double chlorides, insoluble in a strong solution of the alkaline chloride.

The undissolved mass is to be well washed with a saturated solution of chloride of potassium, which, for reasons hereafter to be mentioned, is preferable to sal ammoniac. In this manner nearly the whole of the iron and palladium may be removed, while any insoluble impurities contained in the ore remain with the mixed double chlorides.

(To be continued.)

TECHNICAL CHEMISTRY.

On the Alloys of Copper and Zinc,

by FRANK H. STORER.

(Continued from page 72.)

I have determined the quantity of this impurity in some fifty or sixty instances: it was found to vary from 1.5 per cent. or less to 4 or even 5 per cent., in exceptional cases, of the copper dried at 100° ; ordinarily, it was from 2 to 2.5 per cent. These remarks refer to copper precipitated from sulphuric acid solutions, which is usually in a porous, spongy state. When precipitated from chlorhydric acid solutions it is usually crystalline and compact, and, doubtless, contains less impurity. It is said, however, that a certain amount of loss will invariably occur, even when the best samples of copper precipitated by iron are heated in an atmosphere of hydrogen. Even in copper precipitated by pure zinc I have observed that an appreciable loss occurs,—varying in my experiments from 0.5 to 2 per cent of the dry copper,—when it is ignited in hydrogen gas. It would appear that, while copper may be determined with great accuracy when precipitated by pure zinc in a platinum vessel, as recommended by Fresenius,¹ even when the precipitate is dried only at 100° , for in this case the

¹ "Anleitung zur Quantitativen Chemischen Analyse" (Braunschweig, 1858), s. 247.

copper is deposited in a condition so compact that little or no impurity can adhere to, or oxide form upon it, yet it can hardly be determined with nicety when precipitated in the spongy state, unless it be ignited in a current of hydrogen, or converted into oxide before weighing; it is necessary also to exercise the greatest care that none of the smaller particles of copper are lost during the washing, for many samples of copper, when precipitated as sponge by zinc, are peculiarly liable to this accident.

In regard to the assay which I have attempted to describe, I would remark, that I have used iron instead of zinc,¹ simply because I was, at the time, unable to procure a sufficient quantity of the latter in a state pure enough for analytical purposes. The assay by iron, as I have used it, is clearly not an accurate analytical process, but, on the other hand, it is in several respects preferable, as an assay, to the method by zinc, and will be found, after a little practice, to yield satisfactory results. It is, doubtless, as good a method as any which we possess, wherever absolute accuracy is not required, and where a large number of determinations must be made. Admitting that the results obtained by it may vary among themselves within the limits of one per cent., or, in exceptional cases, even to the extent of one and a-half per cent., I have still deemed it sufficiently accurate for the work to which I have applied it; for it must be remembered that these differences are much smaller than the variations in composition which may occur in different parts of the same cup of alloy, from partial volatilisation of the zinc, or from incomplete mixture of the metals of which it is composed.

I may also remark that, although this assay was not adopted by myself until after thorough trial of several other processes which seemed to be applicable to the special case with which I was occupied, I now consider it a very valuable method.

It deserves mention in this connection, that the assay by iron has been entirely misunderstood by some writers. Thus, Mitchell² directs that the whole of the iron added shall be dissolved in chlorhydric acid, just as if it were zinc, after the copper has been completely precipitated; the absurdity of which procedure is manifest.

In concluding the subject, I may observe that American assayers are accustomed in practice to use a mixture of nitric and chlorhydric acids as a solvent in the first instance of the sample to be assayed, and to expel the nitric acid by evaporating twice to dryness with an excess of chlorhydric acid. The copper being then precipitated from the hot chlorhydric solution. Some of them collect the copper upon a filter, instead of washing by decantation, and rinse it once or twice with very dilute chlorhydric acid before washing with water.

This process has the advantage, that a larger portion of the impurity of the iron is soluble in chlorhydric than in sulphuric acid, hence there is less chance of the copper being contaminated by it; basic iron salts are also less liable to form: the evaporations, which are conducted over the sand-bath, consume somewhat less time than when sulphuric acid is used; and the copper, being precipitated in a crystalline state, is easily washed without loss, and is not liable to be attacked by the acid solution.³

On the other hand, in the process by sulphuric acid,—

which I have used merely in order to remove the lead which is contained in the alloys,—the last traces of copper seem to be somewhat more readily precipitated from the solution, and the accident of the copper adhering too closely to the iron occurs, perhaps, more rarely.

Of the special precautions which must be taken when this assay is applied to the various impure ores of copper, I cannot here speak. Many of them have been already described by Kerl.⁴ I would, however, mention the fact, that, in cases where arsenic is the only impurity, it has been found advantageous in practice to precipitate it upon iron with the copper, and to expel it subsequently when the latter is heated in the atmosphere of hydrogen.

It is a well-known fact, that the combination of copper with zinc is attended with ebullition of considerable violence, so that portions of the melted mass are often thrown to a distance of several feet from the crucible. Yet it does not appear to have been previously noticed by chemists that this action is much more energetic while the first portions of zinc are being added to the copper, and that the loss of zinc by volatilisation is far greater at this time than at any subsequent stage of the operation; indeed, when the alloy has become somewhat rich in zinc, it takes up an additional portion so quietly that scarcely any action is to be observed. The fact is, however, well known to brass-founders, who are in the habit of first melting a quantity of old brass with the copper in their crucibles before adding any zinc as such.

A very violent action will also occur if, after allowing the zinc to melt and remain floating above the copper, as it will do if the mass is not agitated after each addition of zinc, the two metals are suddenly stirred together. This action is apparently analogous to that observed by Levot,⁵ which occurs when layers of melted silver and gold are mixed. Levot ascribes the violent ebullition which ensues when these metals are stirred together to the escape of oxygen which had been absorbed by the molten silver, and which would be expelled as soon as this metal unites with the gold. This hypothesis, however, does not, in my opinion, satisfactorily explain the phenomena which occur in the case of copper and zinc. Another explanation is afforded by the experiments of Person,⁷ who has shown that in the formation of certain alloys a large amount of latent heat is evolved.

The apparent energy with which the combination of copper and zinc occurs is often mentioned in treatises on chemistry, as if it indicated the existence of strong chemical affinity between the two metals. Yet it is easy to separate the zinc completely from any of these alloys by heat. Indeed, Bobierre⁸ has devised a method for their quantitative analysis, by heating the sample of alloy in a porcelain tube, through which a current of hydrogen is made to flow.

In preparing the alloys, it may readily happen, if the mass be not very frequently stirred, that a small portion of the copper, or of an alloy rich in copper, may become chilled and solidify at the bottom of the crucible, while a quantity of easily-fusible alloy, rich in zinc, has formed and remains liquid above it. Accidents of this nature happened to myself very frequently during the earlier part of the research. They occasioned no inconsiderable loss of time, since it was in every instance necessary to prepare a new sample of the alloy on account of the enormous waste of zinc which would occur if one

¹ Method proposed by Vauquelin, *Annales de Chimie* (1798), xxviii. 50.

² "Manual of Pract. Assaying" (London: Baillière, 1854), p. 257.

³ The assay undoubtedly affords more accurate results with chlorhydric acid than when sulphuric acid is used. Mr. Dickinson assures me, that in his hands variations so great as 0.5 per cent. are entirely exceptional, the usual difference between any two assays of a homogeneous sample of ore not being larger than 0.2 to 0.3 per cent.

⁴ *Op. cit.*, s. 219.

⁵ *Ann. Ch. et Phys.* (3), xxxix. 168.

⁷ *Ibid.*, xxiv. 146.

⁸ *Thèses*, p. 57, also, *Compt.-Rend.*, xxxvi. 224.

attempted to re-melt the chilled *culet*; and were especially vexatious, from seeming to indicate the existence of definite alloys, having little or no affinity for each other. Similar accidents sometimes occur in brass foundries, but are evidently less liable to take place here, where the amount of melted metal is large and the heat well regulated, than in the small crucibles and furnaces of the chemical laboratory. It is worthy of remark that many brass-founders refer them to some peculiarity of the particular sample of copper used. If any trouble of this kind is experienced, a portion of common salt is usually thrown into the crucible, it being regarded as a remedy. It is very probable that the presence of some foreign metal may prevent or retard combination; on the other hand, copper which still contains a portion of sulphur is thought to combine with zinc with peculiar facility. In my own experiments, these occurrences appeared to depend entirely upon irregular heating of the furnace, or upon insufficient stirring. D. Forbes⁹ has analysed such specimens which were produced accidentally in the ordinary process of brass-making. They consisted of a white alloy, containing 46.51 per cent. of copper, and of a yellow alloy, containing 56.91 per cent. of copper. From my own experience, I am satisfied that layers containing almost any proportion of the two metals may form.

It is, doubtless, this tendency of the metals to remain unmixed in separate layers, when not subjected to agitation, which has led several chemists to believe that the alloys of copper and zinc are apt to separate, by eliquation, into two portions, respectively rich in copper and in zinc; from which they have inferred, as I have previously remarked, the existence of definite compounds. Although in my own experiments I have been unable to detect any eliquation, I would by no means assert that a certain amount of such separation may not, under some circumstances, occur. The method of analysis which I have adopted is not delicate enough, nor will the very conditions of the case itself admit of an accurate determination of this point. My results are, however, as I think, sufficient to show that any tendency towards eliquation which may exist must be very slight, although they do not prove that a small amount of it may not be produced by gravity, or by some other force acting against the comparatively feeble affinity of the metals for each other. Experiments upon this subject, which have been recorded by previous observers, have evidently been exposed either to the error of insufficient mixing in the first instance, or to the loss of zinc by volatilisation from those portions of the alloy which were in contact with the air, or to both of these causes. Some of them appear to have also been influenced by the presence of foreign metals in the alloy.

(To be continued.)

On the Combustion of Wet Fuel in Furnaces, by
B. SILLIMAN, Jun., Prof. Gen. and App. Chem. in
Yale College.¹⁰

In all ordinary modes of combustion, it is well known that the use of wet fuel is attended with a very great loss of heat, rendered latent in the conversion of water into steam. As the most perfectly air-dried wood still contains about 25 per cent. of water, according to the

experiments of Rumford, the term *wet fuel* might seem appropriate to all fuels but mineral coal and charcoal. But, technically, this term is restricted to substances like peat and those residual products of the arts which, like spent tan, bagasse, and dye stuffs, contain at least one-half and often more than half of their weight of water. Until a recent period, the attempt to consume these products as sources of heat has been attended with uneconomical results, or total failure. It is the object of this paper to describe a mode of combustion in which, by a modification in the form of the furnace, the combustion of wet fuel is not only rendered consistent with the best economical results, but which, as it involves chemical reactions never before, it is believed, successfully applied for such purposes, is deserving of particular notice, from a scientific as well as from a practical point of view.

It is a well-established fact in chemistry, that the affinity of carbon for oxygen, at high temperatures, is so strong, that if oxygen is not present in a free state, any compound containing oxygen which happens to be present is decomposed, in order to satisfy this affinity. This fact is well illustrated in the familiar case of the blast-furnace, where this affinity is employed to deprive the ores of iron of their oxygen in the process of reduction to metallic iron.

In the first stages of combustion, in wet fuels, the chief products given off are steam from the drying of the wet mass, smoke, or volatilised carbon, and oxide of carbon, with, of course, a variable proportion of carbonic acid and carburetted hydrogen. These products, in all ordinary furnaces, pass on together into the stack, carrying with them the heat which they have absorbed and rendered latent. The problem presented is then to recover the heat thus locked up and lost, and, by the furnace now under consideration, this is accomplished by shutting off almost entirely the access of the outer air and causing the wet fuel to supply its own supporter of combustion, drawn from the decomposition of the vapour of water at a high temperature by its reaction with free carbon and the oxide of carbon.

The practical solution of this problem was first successfully accomplished, as appears from a decision of Patent Commissioner Holt, by the late Moses Thompson, in 1854. The controversial questions growing out of this invention are entirely foreign to our present purpose, and in no way affect its practical or scientific value. Suffice it to say, in passing, that we find in this invention, another instance of a truth already so often signalled in the history of inventions, that important results are often obtained, of the highest value in promoting material prosperity and the welfare of society, by those who are guided in their search only by the result in view, and not by any exact knowledge of the scientific principles involved.

Mr. Thompson seems to have been inspired with the conviction that if he could bring the products from the combustion of wet fuel together in a place hot enough for the purpose, and from which the atmospheric air was excluded, they would, as he expresses it in his patent, mutually "consume each other." This notion was realised, and the reaction secured between the elements of water and the carbon of smoke, or the oxide of carbon, in a part of the furnace called by the inventor the "mixing-chamber."

Wherever that place may be situated, or however constructed, the one essential thing about it is, that it should be a very hot place, and one to which the atmospheric air can have no direct access until it has passed

⁹ "Report of Twenty-fourth (Liverpool) Meeting of the British Association for Advancement of Science," 1854, p. 67. See also Liebig and Kopp's *Jahresbericht*.

¹⁰ From the *American Journal of Science*, Vol. XXX. No. 89.

by and through the burning fuel. It is, in fact, a retort, or place for combination and reaction, and may be a distinct chamber or flue, or only a recess or enlargement, greater or less, of the main furnace. Wherever it may be placed, or however built, it must meet the essential conditions of a high temperature, and of atmospheric isolation. In this mixing-chamber, then, the important chemical reaction before insisted on must be set up. The vapour of water is decomposed, furnishing its oxygen to the highly-heated carbon to form carbonic acid, while the oxide of carbon is, in like manner, exalted to the same condition, and any excess of carbon forms with free hydrogen, marsh gas, or light carburetted hydrogen. The vapour of water is thus made to give up, not only its constituent elements, to form new compounds with oxygen, producing in the change great heat, but a great part of the heat absorbed by the water in becoming steam is also liberated in this change of its physical and chemical condition. Moreover as all these products of combustion and of chemical reaction pass together over the bridge-wall of the furnace into a space from which atmospheric air is not excluded, it then and there happens that any free hydrogen, light carburetted hydrogen, or oxide of carbon, which have previously escaped combustion, take fire and burn, yielding up their quota of heat to the general aggregate.

Such is the intensity of heat in that portion of the furnace where these reactions take place that only the most solid structures of refractory fire-bricks will endure it, and the colour seen throughout that portion of the furnace is of the purest white.

In view of the facts already stated, it is easy to understand why it is that when the reactions described are once set up, the admission of a free current of atmospheric air should immediately check the energy of the combustion, and soon result in total suspension of the peculiar energy of this furnace. The air containing only one-fifth part of its bulk of oxygen gas, the active agent in combustion, the access of so large a proportion of cold air,—four-fifths of which are not only indifferent, but positively prejudicial, from the quantity of heat it absorbs,—it happens that the temperature of the mixing-chamber is rapidly reduced below the point at which carbon can decompose vapour of water, and the instant that point is reached the arrival of fresh supplies of steam completes the decline of energy, and the furnace commences forthwith to belch forth from its stack dense volumes of smoke and watery vapour. When in proper action, not a particle of smoke is visible from the stack of a furnace in which wet fuel is burning, and, what is more remarkable, the reactions are so evenly balanced that no wreaths of watery vapour are observed, while in the earlier stages of combustion, before the proper temperature in the mixing-chamber is reached, both these products are seen in great abundance.

Here let the inventor speak for himself in the language of the patent:—

"I build two furnaces side by side, each nearly square in its horizontal section. Towards the top I draw in the wall in such manner as to form a kind of dome with a sufficient opening at the top to feed the bagasse. The outer walls of these furnaces should be from twenty-four to thirty inches thick and built with a special view to rendering them non-conducting, the wall near the top, and the partition between the two furnaces may be thinner. In each furnace chamber there should be a partition of fire brick extending across it from front to back and rising nearly to the top, dividing it into two nearly equal parts. The whole interior of the furnace should be

of fire brick. The main chamber of each furnace should be divided into two parts—upper and lower—by a fire brick grate about one-fifth the height of the furnace above the hearth, the back end of the grate being a little lower than the front. The bottom of the lower chamber may be a grate with an ash pit, but a hearth is much better.

"In each furnace at the front, on each side of the central partition and immediately under the front end of the grate should be doors for feeding wood or other dry fuel, and directly under these doors at the hearth of the lower chamber should be draught openings capable of adjustment to support combustion in the lower chamber.

"Extending across the back of both furnaces, and opening into both by flues is a mixing chamber into which all the gases from both furnaces enter in a highly heated state and mix and consume each other on their way to the boiler and stack. This chamber should be about one-half the capacity of all the fire chambers and it should extend down about as low as the back end of the grate. The flue through which the products of combustion pass out of this chamber and under the boiler should be in section about one square foot to forty cubic feet of mixing chamber.

"The feed openings at the top of the furnaces should be closed by doors which open inwards by the weight of the feed, but are self-closing, and do not yield to pressure from within.

"The sides of the interior of the upper or wet fuel chamber or drying chamber of the furnace, except the front and back, are corrugated up and down, as also the sides of the central walls or partitions, the corrugations extending down to the grate; these corrugations are for the purpose of allowing the heat to radiate upwards from the fire chamber for heating the masonry, and the wet charge, while the gases or vapors driven out of the wet charge by the heat are allowed to descend to the fire chamber or the mixing chamber. If the surfaces of this masonry were smoothed the bagasse would lie against them in such a manner as to obstruct the upward radiation of the heat and downward passage of the vapours.

"These corrugations are unnecessary in burning tan and sawdust.

"The spaces between the grate bars for burning bagasse should be about six inches wide for the finest grinding and twenty inches for the coarsest, and should vary between these widths according to the fineness of grinding, but for sawdust and tan much less, say from one inch to $\frac{3}{4}$ of an inch. The grate should be made of fire brick.

"The operation of my furnace is as follows: A hot fire of dry fuel is kindled in the lower or fire chambers of the furnaces and after it has been continued till the masonry is well heated, the chamber above the grate is fed with the bagasse or other wet fuel. This hot fire in the fire chamber, especially towards the front of it under the principal mass of the wet fuel, must be preserved throughout the operation. The heat from the masonry and the fire chamber will be communicated to the wet fuel which will cause steam and other gases to issue from it and mix with the intensely hot gases of combustion from the fire chamber, and in a short time the mixing chamber will present intense combustion and heat, the dampers of the fire chambers being partially closed. The lower part of the wet charge will by degrees become dry and charred and will fall through the grate prepared as above unto the fire chamber and supply or nearly supply the place of other dry fuel in preserving the fire in this chamber and the wet fuel being from time to time supplied will furnish in a highly heated state aqueous vapours which descending through the corrugations and otherwise into the fire chamber and mixing chamber, will be decomposed, furnishing much oxygen to the fire, and supply the oxygen necessary to combustion

of all the combustible gases issuing from the fire chamber. If by accident the fire in the lower part of the furnace should predominate, the draught should be diminished and more wet fuel added, and, if by accident, the fire in the fire chamber should become too much cooled down the draught should be let on, and any deficiency of dry fuel should be supplied to the fire chamber. Under proper management little or no dry fuel need be fed to the fire chamber after the operation is fairly commenced, the charred matter falling through the open grate will supply its place; and the caloric thus produced by the combustion of wet fuel, will be vastly greater than from the same quantity by measure of the same fuel when dry. In the fire chamber and in the mixing chamber under intense heat the carbonaceous gases will decompose the steam from the wet fuel and effect complete combustion.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, March 1, 1861.

On Bunsen and Kirchhoff's Spectrum Observations, by Professor H. E. Roscoe.

THE subject which I have selected for this evening's discourse—namely, Bunsen and Kirchhoff's Spectrum Observations—may not convey to the minds of those who are unacquainted with the facts which I shall have the honour to bring before you any idea of the vast magnitude and importance of the subject, and the interest which attaches to it. The attention becomes, however, aroused when we learn that these discoveries of Bunsen and Kirchhoff mark a new era in the science of analytical chemistry. By it we are enabled to obtain a knowledge of the composition of the earth's crust with a degree of delicacy and accuracy of which we had before no idea. With this chemical elements supposed to be most rare are found to be more widely distributed; and on the very first practical application of this method of analysis, a new and hitherto undetected alkaline metal has been discovered. The importance of these researches becomes, however, still more strikingly evident when we hear that the conclusions which are drawn therefrom outstep the bounds of our planet, enabling us to determine with all the accuracy and certainty of exact experiment the presence of certain substances well known on this earth—for instance, iron, sodium, potassium—the presence of these bodies in the sun.

These few words will give some slight idea of the magnitude of the subject, and I think I shall not be misunderstood when I say I feel some degree of diffidence in appearing here to-night in the place of those much better known to you, and who are far more qualified than I am to explain these remarkable results of experimental science.

It would be idle in me to attempt any eulogium or panegyric upon such men as Bunsen and Kirchhoff, but it falls upon me to endeavour to lay before you as clearly and precisely as I am able, the results of their discoveries, trusting to you to draw your own conclusions with regard to the value of such results and the character and ability of men able to produce such discoveries.

The colours which certain bodies impart to a flame have long been used as a test of the presence of such bodies. Thus, for instance, soda imparts a yellow colour to the flame, potash a violet colour, lithium a bright crimson, barium a green, and so on. The colours which these substances produce will be seen when I place on these colourless gas flames small quantities of the substances; I put soda on and you have the yellow flame; here

is the purple flame from potash, the red or crimson from lithium, the green from barium, the crimson from strontium, and the yellowish-red flame from lime. The reason why these bodies produce these peculiar colours in flame is that they are volatile—that they volatilise at the temperature of the flame, and that at this high temperature the vapour becomes luminous and gives off this peculiar kind of light. Now, we can obtain the same effect, if we volatilise the salt in any other way. I have here some gun-paper steeped in the solution of these various salts. By lighting it we again get the yellow flame from soda, the purple from potash, and so with the others. It is only because these substances which we place in the flame are volatile at this temperature that we obtain any light at all. If we place a piece of platinum-wire in this flame, the platinum, not volatilizing, gives us no colour, it becomes white-hot itself, but it does not impart to the flame any of the peculiar colours we noticed before, for the reason that platinum is not volatile at that temperature. These facts, I say, have long been known and observed; but it was reserved for Bunsen and Kirchhoff to place these beautiful phenomena in their true light; to apply to them the modern methods of exact research, and thus to open a new and vast field for most important investigation. This they accomplished in a most simple and beautiful manner,—by examining the coloured flames by means of a prism, and thus splitting the vapour of the different metals. If we pass white sunlight through a prism, we see the well-known solar spectrum discovered by Newton. On this diagram we have an illustration of the solar spectrum,—a very rough one, but still showing what we want to see. It is a representation of what we see if we pass a white solar ray through a prism,—we find the spectrum changing in its gradations of colour, from the red, the least refrangible ray, noticing on our way certain dark lines, spaces, or shadows, which exist in the sun-light, and which we always find there,—lines in which we should have a considerable deal to do, until we come to the most refrangible ray, to the violet. Now, if instead of passing white sun-light through the prism, we pass the yellow light which we obtain from the soda flame, in the same manner as we passed the solar light, we get the soda spectrum. In this we find that instead of all the colours of the rainbow, as in the solar spectrum, the soda spectrum consists of only one bright yellow line. This shows us that there is only this particular kind of light present in the soda—that this yellow flame of soda only contains light of one kind. In the same way we find that when we examine the flames produced by the other metals which I have mentioned, not with the naked eye, but with the prism, we obtain spectra from all of them, each giving its peculiar system of lines. I have on a diagram the representation of the spectra produced by these various substances. The arrangement by which these are obtained is a very simple one,—it is obtained by an instrument which I have on the table before me [referring to the instrument]. It consists of a prism encased in a dark box, and on each side of the prism there is placed a telescope. The eyepiece of this first telescope is removed, and in its place is inserted a slit so arranged that it coincides with the focus of the object-glass. The coloured flame is placed in front of the slit, and by looking through the other telescope, and adjusting the prism, we can see the spectrum of the metallic salt placed in the flame. For instance, when I place this soda flame before the slit, and look through this telescope, I see the beautiful yellow line of sodium; there is nothing but a yellow line, all the rest is dark.

Now, in order to begin to illustrate the use of this mode of using flame, I have here a mixture of a salt of potassium and sodium, which contains twenty parts of potassium and one part of sodium; and yet you will see that you

can scarcely recognise the presence of any potash at all in the flame. So intensely yellow is the soda that when we have only one-twentieth part of it, the potash is almost invisible. But if we look through a prism at the flame we then see the spectra of the sodium and the potassium perfectly distinct and separate. The sodium is confined to its own particular sphere,—we see only the one bright yellow line which it gives,—and we see the potassium spectrum as brightly as before. I will throw on the screen a representation of what we really see when we look through the prism. You there see that when the soda line appears the potassium lines remain as distinct as they were before, the soda being confined to its own limits. Now, this holds good with mixtures of all these metallic salts, for in no one case does the spectrum of one interfere with that of the other. These particular lines you see here, which are characteristic of the metal, are brought about by no other substances than we are acquainted with. The presence of that yellow line is an absolutely certain indication of the presence of sodium, the red line of the presence of lithium, and so on. We may, therefore, rely on these appearances as being certain indications of the presence of these substances.

Now, in order to give some opinion of the value of this kind of analysis, I would tell you that the various processes which the chemist must pursue in the ordinary way in order only to detect the presence—not estimate the quantity, but simply to detect the presence—of these five or six alkaline metals when they are mixed together, occupies from two to three hours. And, what is more important, the results by these means are never absolutely certain, especially if there is only a small quantity of a substance present. As a contrast to this I will quote the following. M. Bunsen in his most interesting paper, said:—"A mixture of the chlorides of sodium, potassium, lithium, calcium, strontium, and barium, containing at most one ten-thousandth part of a grain of each of these salts, was brought into the flame, and the spectra produced observed. At first, the bright yellow sodium line Na α appeared with a back ground formed by a nearly continuous pale spectrum. As soon as this line began to fade, the exactly defined bright red line of lithium Li α was seen; and still further removed from the sodium line, the faint red potassium line K α was noticed, whilst the two barium lines Ba α , Ba β , with their peculiar form, became visible in the proper position. As the potassium, sodium, lithium, and barium salts volatilised, their spectra became fainter and fainter, and their peculiar bands one after the other vanished, until, after the lapse of a few minutes, the lines Ca α , Ca β , Sr α , Sr β , Sry, and Sr δ , became gradually visible, and, like a dissolving view, at last attained their characteristic distinctness, colouring and position, and then after some time became pale and disappeared entirely."

Thus we get, with absolute certainty, in half a minute, what, according to the old method, was obtained somewhat doubtfully after three hours' hard work. [The Lecturer proceeded to illustrate the phenomena described by Bunsen, by the aid of the oxyhydrogen lime light, running through the description of the phenomena again.] But this application which I have mentioned is, however, by no means all that this method can accomplish; it cannot only do that speedily and easily, which we find a difficulty with other methods to perform, but it can do for us that which the old method never could. We can detect by this method quantities of the substances the smallness of which is perfectly astounding. Choosing from a number of beautiful examples which Bunsen gives us in his paper, I would refer you to this diagram, on which I have marked some of the most important, to show the delicacy of the spectra analytical method. "Soda: $\frac{1}{5000000}$ th part of a milligramme, or $\frac{1}{100000000}$ th part of a grain." These numbers seem almost absurd. I can scarcely tell you to-night how they were determined, but, in order to show you how delicate the process is, if we burn a piece of sodium weigh-

ing, say the $\frac{1}{10}$ th of a grain on a piece of paper, we allow the vapour to diffuse itself throughout the room. Supposing the whole of it went up in vapour, you would have to see how much of that sodium was present in every cubic foot of the air in this room. Then you must know how much air each of these gas lamps requires to burn a certain quantity of gas in a certain given time—for instance, five minutes; then bring it down to the time required to take an observation, say two seconds, and in that two seconds you will see there must have been a minute fraction of the $\frac{1}{1000000}$ th part of a grain present in that volume of air. In this way, which is a sort of rough description of the method which Bunsen used, he arrived at this astounding fact, that the $\frac{1}{5000000}$ th part of a milligramme, or the $\frac{1}{100000000}$ th part of a grain of soda can be easily detected. Soda is always present in the air; all bodies exposed to the air show the yellow soda line. If a book be dusted near a flame, the soda re-action will be seen. If we leave a piece of platinum wire out in the air for an hour or two, it will no longer impart no colour to the flame as the platinum wire did before, but it will give us the soda flame. If I pass it through my finger you will see the same result,—the yellow flame indicative of the presence of soda, which the platinum wire has taken from my finger.

Of lithia: $\frac{1}{1000000}$ th part of a milligramme, or $\frac{1}{5000000}$ th part of a grain, can be easily detected. Lithia was formerly only known to occur in four minerals. It is now found by spectrum analysis to be one of the most widely distributed elements. It exists in almost all rocks, in 3 cubic inches of sea and river (Thames) water; in the ashes of most plants, in milk, human blood, and muscular tissue. An experiment which Bunsen is very fond of showing is that of holding the end of a cigar, (which our German philosophers generally have at hand,) in the flame. You cannot see the lithium flame produced by it with the naked eye, but it is quite plain when we look at it through a prism.

[Some dusty books were then brought in, and the Lecturer showed very clearly the presence of soda in the dust by knocking them together near the gas-flame.]

On one occasion when Bunsen was examining the spectra of the alkalies obtained from certain of the mineral waters of Germany by means of this apparatus, he observed the occurrence of two bright blue lines, which, when he had examined similar spectra before, by these means, he had not discovered. Hence, he concluded that these bright blue lines must be produced by a new and hitherto undetected alkaline metal. Subsequent research proved the validity of the supposition. The new metal was found and separated from the accompanying alkalies. The analogy between this discovery and a celebrated one in another branch of physical science will be evident to all; as Adams and Leverrier discovered Neptune by perturbations which it produced in the orbit of another planet, so Bunsen discovered cesium by the perturbations which it caused in the spectrum. I have through the kindness of my friend the opportunity of showing a compound of this new metal to you. This was sent to me as the bi-tartrate of cesium by Bunsen, and was all that could be obtained from four tons of mineral water.

I will first show you the flame, which is a very bright violet, which it produces, and then the spectrum. He has also determined its various properties, which, however, I am afraid the time will not permit me properly to describe. It very closely resembles potassium; it produces an insoluble bi-tartrate, and reacts in various other ways like potassium. It is called cesium, from the Latin word "*caesius*," meaning "bluish-gray." I have here a representation of the spectrum of the new metal, which consists of two very bright blue lines, one of which is almost coincident with the strontium blue line. Only on Monday last I received a letter from Bunsen, which contains the following most interesting information. This is an

extract from the note which he sent me, and this I believe is the first announcement which has been made of what I am going to state:—"The substance which I sent you as impure tartrate of caesium contains a second new alkaline metal. I am now engaged in preparing its compounds, and will soon give you a more detailed information concerning it. The spectrum of the new metal consists of two violet lines, situated between Sr δ and K β ." This, of course, is all I can tell you of this new metal, it being all that we know as yet; but this will serve to give you a notion of what we may expect to hear from this new branch of analysis; for if, on the examination of the alkalies of a mineral water, two new alkalies have been discovered, how many more remain to be discovered? This is a matter we must wait to learn.

That this same method of investigation can be extended to all the metallic elements is more than probable. I have before told you that when we bring platinum into flame we obtain no lines in the spectrum, because the platinum is not volatile enough. If we bring it into a flame hot enough, by which we can obtain an incandescent vapour of platinum, we then get a peculiar spectrum. Kirchhoff states that he has assured himself that the rarer metals,—for instance, erbium and terbium,—and these are substances which chemists seldom or never meet with,—can be most quickly and certainly detected by the help of the spectrum analytical method. Experiments are now being carried on, with a view of making this examination practically applicable to all metals, by Bunsen and Kirchhoff. It would therefore be premature to discuss this subject now, but we may soon expect to know more about this most important matter.

(To be concluded in our next.)

ROYAL DUBLIN SOCIETY.

Monday, February 18, 1861.

LORD TALBOT DE MALAHIDE in the Chair.

THE monthly scientific meeting of this Society was held on Monday evening, February 18, when

Professor DAVY read a Paper "On the Injurious Effects of Arsenical Pigments," and said that in consequence of the difference of opinion which exists on the subject; and as it had never been touched on in Ireland, except by Dr. Apjohn, who had published a letter on it in the *Farmers' Gazette*, he had thought it would be interesting to state the results of several experiments which he had made with a view to the detection of arsenic in paper hangings, material for ladies' dress, &c. He alluded to the case of Dr. Healy, of London, who had reported the symptoms under which he had suffered himself whenever he inhabited a room covered with green paper, and which immediately disappeared on his removing his place of residence. Professor Davy also alluded to the letter published by Dr. Hinds in the *Medical Times* for May 23, 1857. In consequence of these communications Mr. Phillips had been employed by the Board of Trade to investigate the matter, and had reported to them that although the paper might contain as much as a drachm of the arsenite of copper to the square foot, still that as the arsenic was not evolved at ordinary temperatures no ill results would accrue from inhabiting the rooms. This report was still made use of by the paper makers, who circulated it among their customers. In answer to this report, Dr. Healy wrote again to the *Daily News*, and Dr. Hinds to the October number of the *Pharmaceutical Journal*, reiterating their statements, and Dr. Taylor, who wrote in the *Journal of the Society of Arts*, corroborated their testimony, and said that Mr. Phillips' arguments might as well be proved by the state of the miners in Cornwall, some of whom did not suffer in the least from the practice of their trade. Dr. Taylor accounted for the immunity of some

persons by the fact that the flock on some papers was very loosely applied, and became diffused through the air by simple friction. He gave also the case of a man who had got very sore eyes from having dusted some books, the deposit on which, on being analysed, gave a large percentage of arsenic. Professor Davy also quoted the cases of two children who lately died in London from arsenical poisoning, supposed to be contracted by sucking toys which had been covered with green paint. In one case the paper on the walls was found to contain fifty-two grains of arsenite of copper to the square foot. Professor Davy remarked on the evolution of arsenic from superphosphate of lime which had been laid by, and attributed it to the existence of the metal in the sulphuric acid employed in its manufacture. He alluded also to cases which occurred in the Adelaide Hospital, and also to the Librarian of the Cork College, in whom inflammation of the eyes and ulceration about the nose and mouth appeared to be the most prominent symptoms. He showed that it was quite possible that the arsenic might be in a state of gaseous combination which would not be detected by the ordinary tests, but which might exert a very deleterious influence on the health, and that in this respect it was analogous to phosphorus and lead. Professor Davy had detected the poison in many forms of confectionery, in dyes for ladies' dresses, especially in green tarlatan, from a piece of which, nine inches square, he had extracted twenty grains of arsenite of copper, which was so loosely attached to it that it became diffused even on shaking it. He had also analysed pieces of wax taper, in ninety grains of which he had detected half a grain of arsenic by Reinsch's method—a process which does not give more than one-half the real quantity of the poison. Professor Davy concluded by detailing the symptoms under which paper-hangers, painters, and other persons who were liable to the contact of the poison laboured, and which appeared after even one day's or a few hours' work with the deleterious material, and hoped that ladies and persons who had a *penchant* for green colours would at least consider the results which must accrue to the workmen in these materials if their manufacture be encouraged.

Dr. BARKER mentioned that he had analysed a specimen of green confectionery for Dr. McClintock, and detected a very appreciable quantity of arsenic. He also alluded to the symptoms which attacked painters who worked exclusively with lead and turpentine, and argued that as arsenic was so much more volatile, it was the more likely to be disseminated.

Mr. HAMILTON mentioned that he had been assured when purchasing a green paper that it contained no arsenic, but that having perceived symptoms of poisoning among his family, he had had the paper analysed and detected the poison in considerable quantity.

Professor DAVY said that all green wall papers contained arsenite of copper, whether dear or cheap, French or English, dark or light, sized or unsized.

CHEMICAL SOCIETY.

Thursday, February 21, 1861.

WARREN DE LA RUE, Esq., F.R.S., Treasurer, in the Chair.

Dr. Murray Thompson, and Messrs. W. Weston and J. Napier were elected Fellows.

The following Papers were read:—"On the Action of Dilute Nitric Acid upon some Hydrocarbons of the Benzole Series," by Messrs. W. De la Rue and H. Müller. The authors endeavoured to obtain ampelic acid by acting on the benzol hydrocarbons obtained from various sources, with dilute nitric acid, but without success. But by acting upon purified coal-tar naphtha, with about fifteen times its volume of a mixture of one part of nitric acid and three parts of water, they eventually produced a yellowish-white flocculent substance, partly dissolved, partly

suspended in the liquid. After partial purification from nitro-compounds, &c., this substance was treated with a large quantity of boiling water, which left some terephthalic acid undissolved. The solution on cooling yielded a deposit which after drying and fusing was distilled. It yielded first a palmitic-acid-like distillate which solidified in the neck, and then a sublimate of large acicular crystals which deposited in the body of the retort. These crystals consisted apparently of an hitherto undescribed acid, bearing some resemblance to the anhydride of nitrophthalic acid. The palmitic-acid-like distillate was resolved by treatment with a Burmese naphtha product boiling at about 90,—probably hydride of capryl—into two acids, one a homologue of benzoic acid having the formula $C_8H_7O_2$, possibly collidinic acid and the other having the composition of benzoic acid but rather corresponding in its properties to Kolbe and Lautemann's recently-discovered isomer of benzoic acid, namely, salicylic acid.

Dr. THUDICHUM read a Paper "*On the Putrefaction of Bile and the Formation of Gall-stones.*" He had observed casts of biliary ducts occupying the very centres of gall-stones, from which he inferred that the morbid process by which these concretions had been formed was not confined to the gall-bladder. These casts were constituted of the brown modification of cholochrome termed cholophaeine. According to the author the essential constituents of gall-stones are cholochrome, cholic and choloidic acids, and earthy salts, being the products which are precipitated during the putrefaction of bile out of the body. Hence he inferred that gall-stones were formed by a process analogous to that of putrefaction, by which the bile acquired an acid reaction. Human gall-stones usually contain in addition a large quantity of cholesterine crystallised around the true biliary calculus. The author described from his own observations the process of the putrefaction of bile and agreed generally with Garup Besaney, but differed from him in considering the choloidic acid to result from the metamorphosis of the cholic, and not the cholic acid to result from the metamorphosis of the choloidic. He then described the methods of analysing ox gall-stones and human gall-stones, and of putrid ox and human bile.

Dr. GUTHRIE read a Paper "*On the Bisulphide of Iodine.*" No definite compound of iodine and sulphur has been hitherto described, despite the very definite character of one, at least, of the sulphides of chlorine. The author obtained bisulphide of iodine by reacting upon iodide of ethyl with bisulphide of chlorine according to the equation, $C_2H_5I + S_2Cl = C_2H_5Cl + S_2I$. By the spontaneous evaporation of the chloride of ethyl, the bisulphide of iodine separates in magnificent crystals. Its composition was determined by analysis.

Mr. R. ADIE read a Paper "*On Ground Ice.*" He observed that during the recent frost, despite its intensity there was very little ground ice formed, probably because the covering of snow preventing the frost penetrating to any depth in the earth. He was of opinion that the position of ground ice was one of lodgement merely. Masses of ground ice, seen in the river Eden in 1854, contained throughout their substance water-worn pebbles, and were inclined towards the current from which they were receiving continual supplies.

Dr. FRANKLAND considered that ground ice was really formed at the spots in which it was found. He repudiated the idea of its being produced through any radiation from the bottom of the stream, because water is quite opaque to rays of heat of low intensity. He supposed that in rapid, shallow streams the temperature throughout from top to bottom was rendered uniform by intermingling and that the presence of irregularities at the bottom determined the formation of ice crystals there, in accordance with the usual tendency of crystals to become attached to rough surfaces.

NOTICES OF BOOKS.

A Manual of Elementary Chemistry, Theoretical and Practical. By GEORGE FOWNES, F.R.S. Eighth Edition, Revised and Corrected. London: John Churchill. 1861.

FOWNES' excellent Manual is too well known to need any fresh commendation from us. It offers in one compact volume an interesting, well-written, and clear account, not only of chemistry proper, organic and inorganic, but also of chemical physics. The fourth part, devoted to Animal Chemistry, will be read with advantage by medical students, among whom this volume is justly popular.

The present edition differs in one or two particulars from the preceding issue of the work. In addition to the concise abstract of the history of ammonia and its numerous recently-discovered derivatives which the editors have inserted, they have given in the form of an appendix an account of Gerhardt's equivalents and notation, which, although it hardly answers to our expectations, will be found to be easily understood by students. We shall, on an early occasion, quote part of this essay on the Unitary System, only giving here a word of comment on further most important developments of Gerhardt's views, which the authors of this account have omitted to introduce. The new matter contained in the work before us is, of course, of considerable value; but we do not think that the present edition shows any marked advance over its predecessor in other respects. We did not expect to find, nor would it have been desirable to insert in it, notices of new substances of insignificant character or doubtful formula; but in looking through the volume, we were struck by several omissions of important discoveries the value of which has been generally recognised. As a warrant for this statement, we will point out a few places, selected from a much larger number to which we might refer, which seem to us to exhibit the fault which we have mentioned.

On p. 109 no mention is made of the silicon compounds obtained by Wöhler, particularly of the body $Si_2H_2O_8$, which corresponds so closely to all appearance with Brodie's graphic acid $Gr_2H_2O_8$ ($Gr_2 = C_{11}$).

The older equivalent of lithium, namely, 6.5 is retained on p. 280: it is not, however, quite certain that the more recent number 7, or rather 6.93 is really more correct.

We confess that we cannot quite grasp the meaning of these directions for obtaining aluminium, given on p. 295; we are told to heat "equal parts of chloride of potassium and cryolite, with 2 parts of sodium and 5 of cryolite."

On p. 353 we observe no reference to the beautiful and effective electrolytic process for the detection of arsenic, lately perfected by Professor Bloxam.

On p. 512 no mention is made of the binoxide of benzoyle described by Professor Brodie; a beautifully crystalline and refractive substance, exploding when gently heated, and possessing representatives in other organic series as that of acetylene; the binoxide of acetylene detonates violently on a very slight elevation of temperature. Not only their physical properties, but their chemical constitution also, causes great interest to attach to these bodies.

On p. 515 no attempt is made to elucidate the constitution of benzophenone as benzoyle-phenyle $C_{12}H_5$, $C_{14}H_5O_2$, for instance; its formula, $C_{26}H_{10}O_2$, is merely given without a word of explanation.

On p. 517 the almost if not quite obsolete and very costly process for preparing chloride of benzoyle by means of oil of bitter almonds and chlorine is given, no notice being taken of the more recent and perfect process of Gerhardt, by which this important substance is obtained, by acting with pentachloride of phosphorus upon benzoic

acid. It is also stated to boil at 196° F., this being the boiling-point on the Centigrade scale.

The interesting formation of toluylic acid from the cyanide of toluenyle obtained from toluole through the intervention of the chloride, C_6H_5Cl , is not noticed on p. 523. We have also looked in vain for any reference to that most important reaction, the type of a large class of reactions, by which Maxwell Simpson has succeeded in obtaining the bibasic acid, the succinic, from the bicyanide of ethylene, $C_2H_2Cy_2$.

On a former occasion we have referred to the intelligible view which Kekulé has given of melam, ammeline, melamine, ammelide, melam, and the mellonides: here, on pp. 570, 571, the crude formulæ are given without comment.

The bases of the chinoline and pyridine series are spoken of on pp. 608—610 as of uncertain constitution; the important observation that they contain no replaceable hydrogen being unnoticed. The fact that only such simple ethyle derivatives as ethyl-pyridyl-ammonium can be formed from these bases limits very materially our uncertainty as to their true nature.

To Borneo camphor is assigned the formula $C_{20}H_{18}O_2$, a typographical error for $C_{20}H_{18}O$. The interesting relation of Borneo camphor as the alcohol of the camphoric series to ordinary camphor as the aldehyde, and to campholone ($C_{20}H_{18}O$) as the hydride, is not alluded to.

Of caoutchoucine, the liquid produced by the destructive distillation of india-rubber, we find it affirmed that "it is probably a mixture of several hydrocarbons scarcely to be separated from one another," although Greville Williams has clearly shown that it consists chiefly, if not entirely, of two very definite liquids: isoprene $C_{10}H_8$, boiling at 37° C., and having a vapour density of 2.44; and caoutchine $C_{20}H_{10}$, boiling at 171° C., and with a vapour density of 4.65.

On p. 647 it is said that to kreosote "the formulæ (*sic*) $C_{14}H_8O_2$ has been given, but is not established beyond doubt;" while on p. 657 it is affirmed that cresylic alcohol, $C_{14}H_8O_2$, is isomeric with kreosote.

Although no list of errata appears in the volume, yet it is by no means free from typographical errors. Of these we have already noticed several, and we find (p. 194) chloride of iodine formulated IC instead of ICl. The melting point of potassium is given as 57° 1.77 C., whatever that may mean, on p. 255.

We have looked in vain for any mention of Anderson's satisfactory experiments with pyrrole, of the acetylene C_2H_2 of Berthelot, of the graphic acid of Brodie, and of many of the other interesting substances with which chemistry has been enriched during the last year. The recent researches of Andrews and Schönbein on oxygen and ozone scarcely receive any notice; and, although *pelopium* does not exist, we find descriptions of its oxide and chloride. But we will now leave the unpleasant task of enumerating the defects of a substantially good book as the present.

In another place we shall quote from the Appendix the account given of the Notation of Gerhardt, generally termed the Unitary Notation. This account is the more valuable since, while the new notation is constantly used by many distinguished chemists in their papers, and discussion on it is frequent, no compact statement of its main features has before appeared in England.

The defects in the sketch of Gerhardt's Notation, and in the Synopsis of Chemical Substances which follow, are chiefly those which are remedied in the important and able work of Kekulé, and to which we have before alluded in these columns. The manifest improvements introduced by this chemist in 1859 should certainly have found a place in a work published in the present year. The Editors of Fownes' "Manual" retain the hydrochloric acid type as distinct from the hydrogen type, a step of very doubtful propriety, and they ignore the 4th primary type, which includes the compounds of the tetratomic

elements, such as carbon. A comparison of the two methods will show clearly what we mean.

Editors of Fownes' Manual.

Kekulé.

Hydrogen Type.

Hydrochloric acid Type.



Hydrochloric acid Type.

Water Type.



Water Type.

Ammonia Type.

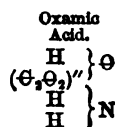
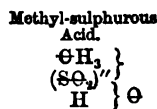
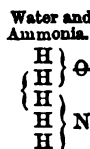
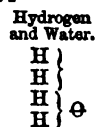


Ammonia Type.

Marsh-gas Type.



We cannot help regarding the omission of all notice of paired or conjugate types, in which two or more types are conjoined into one, as a mistake. By recognising such typical forms as



the system of types becomes much more comprehensive, and, at the same time, affords an easier and truer way of representing such compounds as methyl-sulphurous and oxamic acids.

CORRESPONDENCE.

The Adulteration of Food.

To the Editor of the CHEMICAL NEWS.

SIR,—I do not usually deem it necessary to reply to the statements or accusations of anonymous critics; but, in the present instance, the letters of a gentleman, with the singularly inappropriate signature of "Verax" (published in Nos. 63 and 64 of your Journal), afford me an opportunity of reiterating some of my former observations on the above subject.

Fair and open discussion I cordially invite upon any published results or remarks of mine. I cannot but regret, however, that "Verax" in his virulent animadversions upon my paper, should have so completely lost sight of that courtesy due to every individual from others of his species, and which alone is the sign of a really independent and unbiassed opinion.

In the first portion of his letter "Verax" inquires if "colours and essences of every description" are "to be henceforth decried," and answers his own query further on in these words—"We must all agree that the admixture of poisonous matter in any shape with food cannot be too strictly prohibited." These paragraphs evidently neutralise each other, and could not have been written by any person who had accorded to my paper a moderate amount of attention.

As any one (of competent analytical skill) who likes to take the trouble, can ascertain from actual experience the facts upon which my "sweeping assertion," that of London bread fully eighty-seven per cent. is more or less adulterated, is founded, I consider it unnecessary to say more on this point, than that I have only to repeat my original statement without the slightest qualification.

"Verax" next complains that he has not been told how many specimens I have examined, how many shops furnished those specimens (with the names and addresses of the vendors, doubtless!) what adulterant, and how much of it, was employed in every instance, what processes were adopted for the detection of these adulterants, and a few other little details of a like character. Will "Verax" have the goodness to say how (if he had written the paper) he would contrive to condense the amount of information just indicated, and which I calculate would occupy, at least, 800 pages of quarto, even if written most concisely and printed in breviter type,—into a total bulk of reading matter requiring not more than one hour and twenty minutes for its delivery?

Do authors and lecturers habitually give the exact and detailed grounds for all and every statement, trifling and important, that they may make, as they go on? If I had departed from the general rule in this respect, would not "Verax" have been the first to stigmatise my paper as prolix and tedious to the last degree?

Although I have stated "crumbly texture" and "brittleness of structure" to be indications of the probable presence of potatoes and rice-flour, respectively in bread, who could for a moment suppose that I regarded such characteristics as more than mere indications? I might describe a donkey as "a beast with long ears," without necessarily meaning that every long-eared animal must be a donkey! In a word, all the very simple tests for, and characteristics of the presence of, adulterants named in my paper are to be regarded solely as popular tests, no more suited to the delicate researches of a laboratory, than such researches are, to be carried on in a drawing-room, still, I hold that everyone should have it in his power to gain some little insight into what he is eating and drinking day by day, as by a small amount of trouble *Paterfamilias* might steer clear of many injurious adulterations, without, in every instance, calling in professional assistance.

I have detected the presence of bone-dust in bread and flour in five instances with the aid of the microscope; on this point "Verax" has arrived at a negative result, "in spite of numerous inquiries." In imitation of his peculiar diction, I might ask, How many inquiries made up the sum total he designates "numerous," and whom did he interrogate on the subject?

By no process with which I am acquainted can "brewers' grains be made to bear any resemblance to oats;" nor am I aware that flour possesses many points of similarity to cocoa, which, however, it is constantly employed to falsify. But does "Verax" innocently imagine that an adulterant must always resemble the adulterated article in appearance?

In 1856 a horse was killed, in Berkshire, with adulterated "rape-cake." I decline to publish more particulars, as the sample was sent to me for examination professionally.

"Verax" is of opinion that because I am acquainted, in common with many others, with the extent to which adulteration is practised in the matter of meat, that I ought immediately to acquaint the Inspectors, and become a common informer! However humble my mission in this world may be, it lies in a rather different direction to that just named.

Has "Verax" never heard of the Parliamentary Committees on the Adulteration of Food, and their results? Has he never seen the works of Accum, Mitchell, Pereira, Dodd, Normandy, and Hassall? I cannot otherwise account for the amount of astonishment with which I am flattered to find my paper has impressed him.

Surely "Verax" must peruse the columns of the *Times* and *Daily Telegraph* in a very careless manner, if he is not cognizant of the cases of "Diseased Meat" being exposed for sale at one or other of the London markets—an occurrence by no means rare; only a few days ago such an instance occurred at Newgate Market, and the trial was

noted in the principal papers, the defendants being convicted and fined. Now, for every single conviction, how many cases are there which escape prosecution altogether!

Then, again, with regard to confectionery, how long ago is it since two children of the name of *Carroll* were poisoned by the mineral colouring-matters with which their "sugar-sticks" were ornamented?

It is, indeed, but too true, that adulteration is carried on throughout the length and breadth of the land to an extent which may justly be denominated "fearful;" and the fact is gradually becoming evident to all classes, despite the ridicule of some, the determined opposition of others, and the ignorance or apathy of the majority, who are too much occupied with their daily avocations to give the subject due consideration.

"Verax" will doubtless be "astonished" to hear that I stated that "about 91 per cent. of the pickles sold in London are more or less adulterated," because out of 1127 samples examined, I found only 101 to be good and pure.

Now, with regard to tea, I may remark, that my observations are founded upon the result of the examination of several thousand samples, forming, I am inclined to think, a more reliable criterion than either the *ex parte* statements of tea-dealers and others whose interest lies in exactly the opposite direction to that in which truth is to be found. The remark that "tea cannot be adulterated with impunity and profit," is too evidently absurd to need comment of any kind.

Then, again, what kind of authority is "one of the largest bakers in Vienna" upon the question of the purity of bread? The adulterated "sugar-apples" exhibited at the Society of Arts, were distinctly "warranted all pure sugar" by the seller, although the presence of red lead was clearly demonstrated.

One paragraph of "Verax's" last letter I consider too important to pass without notice; it runs thus:—"It seems to me that Mr. Scott's suggestions as to the course that should be adopted to protect ourselves against the ill effects of an improper dietary, would do away altogether with any but the best materials for food." If "best" be held to signify "pure and wholesome," "Verax" has described my object exactly.

In conclusion, I beg to avow my decided opinion that the brown sugars of commerce (except the "crystalline" varieties) are detrimental to the public health, and consequently that their sale (retail) should be prohibited altogether; that certain varieties of unsound grain are also injurious, and should likewise be condemned; and that vegetables of all kinds should be subject to a closer surveillance than at present, with the view of preventing the sale of decomposing or otherwise injurious specimens.

I will not, Sir, occupy more of your space to-day in reply to the over-strained criticisms and puerile objections of your correspondent, as all attempts to mislead the public and persuade them that their food and drink is of the purest description imaginable, must inevitably, and of themselves fall to the ground—*Experientiam docet*.

I am, &c. WENTWORTH L. SCOTT.

Bayswater, London, W.

P.S. The fact mentioned by Mr. Horsley, of the presence of copper in bread and flour at Cheltenham, is very interesting. Would he kindly furnish me with a few specimens, and any further particulars he might deem it advisable to afford? I have myself detected copper in two instances only; but the grain in these cases had been affected with the "smut," so I attributed the copper to the probable employment of "bluestone" while in the ear.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Salts of Hyponitric Acid.—The compounds of hyponitric acid with bases are said by H. Rose (*Bericht*

der Akademie der Wissenschaften zu Berlin, 1860, s. 710.) to share in general the properties of the salts of tantalum and niobic acids. The potash salt is soluble in an excess of the hydrate and carbonate of potash; and hence it is difficult to prepare the neutral salt pure. When hyponiobic acid is fused with carbonate of potash, the latter loses just so much carbonic acid that the compound $3\text{KO} + \text{Nb}_2\text{O}_5$ is formed. The soda salt is the most important of the compounds of hyponiobic acid, since it can be used in the preparation of all the insoluble salts. It is somewhat more soluble in water than the niobate and tantalate of soda, and can be obtained in larger and more perfect crystals than these salts. It can be prepared with 5 or 7 atoms of water of crystallisation. Like the soda salts of niobic and tantalum acids, it is insoluble in a solution of hydrate or carbonate of soda, except when the solutions are very dilute, in which case it quickly dissolves on boiling. The niobates and tantalates do the same, so that the best way of separating these is to treat them with a solution of hydrate or carbonate of soda of a certain strength. At 100°C . the hyponiobate of soda loses the greater part of its water of crystallisation, and then is perfectly soluble in water, which essentially distinguishes it from the niobate, and particularly from the tantalate. When the neutral salt is heated it is changed into an acid salt and hydrate of soda, which can exchange its water for carbonic acid if the heating be carried on in an atmosphere of carbonate of ammonia. This breaking up of the hyponiobate takes place at a higher temperature than the similar breaking up of the niobate and tantalate of soda. The neutral hyponiobate of soda contains three times as much oxygen in the acid as in the soda; hence, the composition is NaONb_2O_5 . A basic salt, or rather a compound of the acid salt with hydrate of soda, can be obtained, as well as an acid salt, both of which are partly soluble in water. They are made by fusing together hyponiobic acid and carbonate of soda. By fusing together hyponiobic acid and carbonate of soda until the mixture no longer loses weight, a salt is formed which has composition $3\text{NaONb}_2\text{O}_5$. By treating this with water, it is decomposed into a neutral salt, while two atoms of hydrate of soda are set free.

A solution of the neutral hyponiobate of soda remains unchanged in the air, even on boiling.

Neutral hyponiobates of the metals may be made by adding a solution of the soda salt to a solution of a neutral salt of the metals. The author has prepared hyponiobates of magnesia, silver, mercury, and copper, in all of which the oxygen in the acid is three times as much as that of the base.

II. ORGANIC CHEMISTRY.

Test for Cholesterin.—Hugo Schiff describes the following reaction of cholesterine (*Annalen d. Chem. und Pharm.* Bd. cxv. s. 313). If cholesterin be moistened with hydrochloric or sulphuric acid containing perchloride of iron, it becomes of a magnificent violet colour. A mixture of two or three volumes of concentrated hydrochloric or sulphuric acid and one of a dilute solution of the perchloride will answer for the experiment, or even the ordinary commercial hydrochloric acid. A small piece of cholesterin should be rubbed by means of a glass rod with a drop of the reagent, and the mixture then gently warmed. It first becomes of a reddish, which as the temperature rises changes to a blue violet, colour. A strong heat destroys the colour. When sulphuric acid is used the mixture must be warmed more cautiously. It first becomes carmine red, then violet, and at a high temperature is carbonised. Nitric and phosphoric with the perchloride of iron will not give the reaction.

Metamorphia.—In an attempt to prepare morphia by Mohr's method from the residue of tincture of opium Scharf, an apothecary at Munich, obtained some crystals whose solution in an acid gave no precipitate with

ammonia. These crystals were further examined by Wittstein (*Wittstein's Viertel-jahresschrift*, Bd. ix. s. 481), who found them to be a salt of a new base which he has named *Meta-morphia*. The hydrochlorate of this base is soluble at the ordinary temperature in twenty-five parts of water; the solution is bitter and has a neutral reaction. It is less soluble in alcohol, and is insoluble in ether. Liquor ammonia dissolves it perfectly as do caustic potash and the carbonate. Perchloride of iron turns the aqueous solution blue; chloride of gold gives a reddish yellow, flocculent precipitate with a strong aqueous solution which disappears on the addition of much water: chloride of platinum gives a yellow precipitate which re-dissolves in a dilute solution. Tannic acid produces a yellowish-white cloudiness. Strong sulphuric acid sets free hydrochloric acid from the salt without colouring it; nitric acid dissolves the salt, giving a yellow solution. The analysis of the hydrochlorate shows it to contain 12.211 per cent. of HCl. The hydrochlorate of morphia contains only 9.7, and the hydrochlorate of codeia only 9.81, per cent. of acid. From the above analysis the author calculates the equivalent of the new alkaloid to = 3274. With the exception of pseudo-morphia, he says, (which shares many of the properties of the new alkaloid) no opium alkaloid has so low an equivalent. The author prepared the pure base by treating the hydrochlorate with sulphate of silver, and digesting the solution so obtained with carbonate of baryta. It crystallised from an alcoholic solution in stellated flattened prisms, which had very little taste. The new base is almost insoluble in cold water, but more so in hot. Alcohol takes up more; the solution has a weak alkaline re-action, and a very bitter taste. In ether it is almost insoluble. The caustic alkalies dissolve it. Strong sulphuric acid gives a brownish, and nitric acid a yellow colour with it. A strong solution of iodic acid added to the aqueous solution of the new base produces a yellowish colouration, and a strip of starch paper suspended in the mixture is turned violet. Perchloride of iron produces no change in the aqueous solution. Chloride of gold produces a yellowish cloudiness (not a blue colour as with the aqueous solution of morphia), which afterwards deposits as a brownish precipitate. Nitrate of silver gives a grayish-black cloudiness. The author gives no analysis of the base.

LABORATORY MEMORANDA.

On the Action of Sodium on Oil of Turpentine.

—When sodium is brought into contact with the ordinary oil of turpentine of commerce, an active disengagement of gas is immediately produced, the action being continued till the sodium has disappeared. If sufficient turpentine be used, two very different precipitates being at the same time thrown down, the one white and gelatinous, the other brown and inclining to red.

The black precipitate frequently so envelopes the sodium as very much to retard the action; but on stirring, the effervescence is continued as before; if, however, very pure oil of turpentine be used, a much smaller quantity of the dark-coloured precipitate is formed, the white precipitate and the gas being produced as before.

But if the turpentine be repeatedly distilled, at the same time preventing as much as possible all contact with the air, on introducing the sodium scarce any reaction takes place, its surface after a considerable time being only slightly covered with a brown film,—for which reason I attribute the action in the first place to the decomposition of the resinous matter held in solution by the oil of turpentine, after which the reaction appears to sustain itself.

When the reaction has ceased, and the liquid has been subjected to distillation (previously removing any pieces of sodium which may remain uncombined), desisting when coloured vapour passes over, two liquids will be found to have condensed in the receiver; the lighter of the two at first turbid, but becoming clear in the course of a day or so, by depositing a small quantity of a white matter on the sides of the receiver, and having little or no action on sodium, the other is heavy, and collects in globules at the bottom, having the smell and taste of camphor.

If the remains which are left in the retort after this distillation be washed out with ether, the dark precipitate will be dissolved, forming a deep red solution, the other precipitate which appears to be insoluble, or nearly so, in ether, may be separated by pouring off the red solution and drying.

But if the distillation be proceeded with after the coloured vapour begins to rise, a yellow oily matter will be obtained, a carbonaceous mass remaining behind, which if afterwards ignited in contact with the air, leaves caustic soda.

On collecting and examining the gas I failed to detect either carbon or sodium, consequently it must be pure hydrogen. B. PIFFARD.

MISCELLANEOUS.

The Chemical Society.—Arrangements have been made at the Chemical Society for the delivery of the following Lectures:—

March 21.—“On Thermodynamics in Relation to Chemical Affinity.” By A. W. WILLIAMSON, Ph. D., F.R.S.

April 18.—“On the Application of Electricity to the Explosion of Gunpowder.” By F. A. ABEL, Esq., F.R.S.

May 16.—“On the Colouring Matters Obtained from Coal-tar.” By W. H. PERKIN, Esq., F.C.S.

June 20.—“On the Application of the Induction Coil to Steinheil's Apparatus for Spectrum Analysis.” By H. E. ROSCOE, Ph. D., F.C.S.

The Adulteration of Food Act.—The following memorandum on the above subject has been issued from the Sewers' Office, Guildhall:—“Take Notice, that under the second section of this Act, the Commissioners of Sewers of the City of London, have appointed Dr. Letheby, of 41, Finsbury Square, as Analyst of all Articles of Food and Drink purchased within the City of London, and the liberties thereof. By the first section of the said Act, every person who shall sell any article of food or drink with which, to the knowledge of such person, any ingredient or material injurious to the health of persons eating or drinking such article has been mixed, and every person who shall sell as pure or unadulterated, any article of food or drink which is adulterated or not pure, shall for every such offence, on a summary conviction of the same, forfeit and pay a penalty not exceeding Five Pounds, together with such costs as to the convicting Justices shall seem reasonable. And if any person so convicted, shall afterwards commit the like offence, it shall be lawful for such Justices to cause such offender's name, place of abode, and offence to be published at the expense of such offender, in such newspaper, or in such other manner as to such Justices shall seem desirable. By the fourth section of the Act it is provided that any purchaser of any article of food or drink within the City of London, or the liberties thereof, shall be entitled, upon payment to Dr. Letheby (the Analyst appointed under the Act), of the sum of Five Shillings, to have any such article analysed by him, and to receive from him a certificate of the result of his analysis, specifying whether in his opinion such article is adulterated,

and also whether it is so adulterated as to be injurious to the health of persons eating or drinking the same, and such certificate duly signed by such Analyst, shall in the absence of any evidence to the contrary, be sufficient evidence before the Justices, or in any Court of Justice, of the matters therein certified, and the sum so directed to be paid for such certificate shall be deemed part of the costs. By the third section of the Act it is provided, that on the hearing of any complaint under this Act, the purchaser shall be bound to prove to the satisfaction of such Justices that the seller of the article of food or drink alleged to be adulterated, or his servants, had such notice of the intention of the purchaser to have such article analysed, and also such opportunity of accompanying the purchaser to Dr. Letheby, the Analyst appointed by this Act, as the Justices shall think reasonable, in order to secure such article from being tampered with by the purchaser. Arrangements have been made with Dr. Letheby, by which poor persons, whose circumstances do not allow them to pay the fee of five shillings, are at liberty to leave with him, at 41, Finsbury Square, any article of food or drink which they suspect to be adulterated, with a statement in writing of their own name and address, as well as the name and address of the seller of the article. And, if in the opinion of the Analyst, the matter is deserving of inquiry, the analysis will be made, and the certificate furnished free of charge. By order of the Commissioners of Sewers of the City of London, Joseph Daw, Principal Clerk.”

ANSWERS TO CORRESPONDENTS.

*. All Editorial Communications are to be addressed to Mr. CHURCH, and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

J. Quintan.—No preface was issued for our Second Volume. We know nothing of the other journal mentioned.

T. S. Beckett.—There is no foundation for the statement which has appeared in some papers, that silver has been recently produced artificially. The paragraph bears evidence of its absurdity on the face of it.

B. H.—You cannot do better than attend the College of Chemistry, in Oxford Street, for a term or two. Apply to the Secretary at the College, and you will learn all particulars as to fees, &c., which are very reasonable.

S. B.—Soft soap is soap containing potash instead of soda. We presume this is what you mean. It can be liquified by the addition of a small quantity of water and heating.

J. A. D.—We should never object to illustrate an article or letter if the subject were in our opinion of sufficient importance.

B. P.—The article would have been inserted last week, but owing to press of matter at the last moment it was unavoidably postponed. We cannot undertake always to insert communications immediately after they are received.

Reginald R.—Wintergreen-oil is salicylate of oxide of methyl, not, as you state, amyl. If you really wish to make it artificially, instead of obtaining it ready made by Nature, there would be no difficulty by following the ordinary plan for the preparation of the organic ethers. Ceanothic ether is obtained by distilling wine-les; ceanothic acid may be prepared from it by decomposing the ether with potash, and distilling this salt with sulphuric acid. It is now generally considered to be identical with pelargonic acid.

A Subscriber (Paisley).—Our Correspondent is informed that we are not responsible for the terms used by the Patenteses whose processes we quote. The provisional specification of the patent alluded to says, “Equivalent or other suitable proportions.” The final specification says, “Soluble prussian blue and carmine of indigo, when mixed in the proportion of their chemical equivalents, dissolve each other, and combine to form a new blue of definite composition.” The meaning is therefore sufficiently obvious. We regret that our correspondent should ask us “What is the equivalent of carmine of indigo, and in what does the carmine of indigo differ from the ordinary indigo of commerce?” because it shows that he was more eager to find fault than to read the article. Foreseeing that some of our readers would wish information upon those points, we were at especial pains to state the composition and formula of carmine of indigo in the very article alluded to. Does our correspondent at Paisley know how to ascertain the equivalent of a substance from its formula?

THE CHEMICAL NEWS.

VOL. III. No. 67.—March 16, 1861.

THE ADULTERATION OF FOOD AND DRINK.

(Continued from page 146.)

CLAUSE 4 enacts that in places where an Analyst has been appointed under this Act, the purchaser of any article of food or drink may take it to such Analyst, and, on payment of a fee ranging from two shillings and sixpence to half a guinea, require him to analyse it, and certify whether it is or is not adulterated, and, if adulterated, whether the foreign substance is injurious to health. This certificate, duly signed by the Analyst, "in the absence of any evidence to the contrary, shall be sufficient evidence before the Justices, or in any Court of Justice, of the matters therein certified, and the sum so directed to be paid for such certificate shall be deemed part of the costs."

We have heard of three instances of purchasers taking specimens of articles of food to be analysed. These were found to be adulterated; but not a single instance have we known of any prosecution having been grounded upon the certificate given. What the motive of the purchaser may have been is difficult to imagine, unless it were to use the certificate for purposes of extortion. In these particular cases the names of the offending parties were published in some of the newspapers, quite as a matter of chance, and because they were embodied in a report on other matters. The same publicity was likewise given to an analysis of a specimen of bread, which was found to be pure, the name and address of the baker being fully set forth. This may appear like impartial justice, but, in reality, it is not so. The public is benefited by the exposure of roguery; but to publish the name and address of a tradesman at whose shop a genuine article has been purchased is simply to give a man an opportunity of puffing his shop at a cheap rate by collusion with a friend, and possibly to use the services of a public official to aid him in imposing on the public. Of course we must assume that in these instances the purchaser was actuated by honest intentions; but we can say with confidence that, if the law remains unaltered, it will not be long before it is used for purposes never contemplated by those who made it. It is so utterly improbable that anybody will make a purchase for analysis with the intention of prosecuting in the event of its being found adulterated, especially while it is so uncertain if a conviction could be obtained unless he was prepared to prove that the vendor had himself adulterated it, that we shall pay little regard to the reports on the subject, though we anticipate that the next will contain a longer list of samples analysed and found pure than the last. This is another reason why our suggestion, that it should be left to the Analyst to direct the purchase of specimens for analysis, should be adopted.

Clauses 5 and 6 give Justices the power of ordering specimens of food and drink to be analysed, and, if they think fit, at the expense of either the complainant or defendant. Any person convicted by the Justices to

have the right of appeal to the next General or Quarter Sessions, on giving notice of his intention to do so within two days after conviction.

The object of the first of these Clauses is, no doubt, to give the Justices the power of ordering a fresh analysis when they are not satisfied of the competency of the first Analyst, and to this there can be no objection; but we question whether any good can result from giving the convicted person an appeal to Quarter Sessions. It seems to us that this cannot be necessary to prevent injustice, because the decision in the first instance will rest on a matter of fact, which no amount of arguing can alter; and that the only effect is to place another obstacle to the action of the law by preventing a man who might be philanthropic enough to sacrifice his time and possibly his money for the sake of exposing a vendor of adulterated food, if he could see where the process would end, from entering upon a crusade which might involve him in still greater expense, loss of time, and vexation. It is natural enough that where the convicted retailer is conscious that he did not himself adulterate the article found impure that he should feel aggrieved at having to pay a fine; but, instead of appealing, let him have the right of suing the wholesale dealer in the County Court, as we have already suggested, and we venture to say that the Justices at Quarter Sessions will be troubled with few appeals.

The remainder of the Clauses refer to minor matters connected with the working of the Act. They provide that persons convicted of selling adulterated patented articles may have a case stated for the opinion of a superior court; that the penalties inflicted shall be paid to certain local functionaries, to be applied to general purposes; the interpretation of what is meant by articles of food or drink, and specifying the sources out of which the expenses shall be defrayed of carrying the law into operation. There is a special Clause, which states that the Act shall not affect the power of proceeding against offenders by indictment, or in any other way heretofore existing. Thus, the severest form of proceeding against adulterators is still retained, and it was by this means that a batch of bakers were punished so heavily a short time ago for having alum on their premises, which they mixed with the flour.

There is another kind of fraud, which is not dealt with at all by this Act, but which we think ought certainly to be included in its provisions. If a man takes a quantity of horse-beans and converts them into flour, which he calls by some high-sounding name, of Latin or Greek derivation, to the full as unmeaning as that a potato salesman has painted over his shop, "Patent gravity-selected potatoes," and then vaunts his horse-bean flour as capable of curing almost every complaint to which human beings are liable, there is no law to prevent his doing so; nor, if he sold this at something like the market price of horse-beans, would any such law be required. But when this same person, taking advantage of the ignorance of the public of the real nature of

the article he vends under a magniloquent denomination, trades on that ignorance, and charges from ten to twenty times its real value, then we say that he is guilty of a downright fraud and ought to be punished. Substances of this kind are, undoubtedly, articles of food, and might surely form the subject of a Clause in an Act made expressly to protect our pockets as well as our digestions. If we remember rightly, the law of France punishes with great severity fraud of this kind, the amount of the fine being based on the extent to which the defendant profits by his fraud, and to the fine imprisonment is not unfrequently added.

Several months have now elapsed since the Act for Preventing the Adulteration of Articles of Food or Drink became law,—ample time, indeed, for it to have borne fruit if it had any vitality in it; yet it has remained, and still remains, almost a dead letter. Admitting that there are some trifling difficulties in the way of making it effective, we are quite sure that if it be really the desire of Parliament, as no doubt it is, to put an end to the system of adulterating articles of food and drink, which prevails to such an abominable extent, it will not hesitate to make such amendments in the law as will render it capable of effecting the object it has in view. As a preliminary to doing something, it would be well to know what has been done; and we would, therefore, suggest that some Member should move for a return of the number of Analysts who have been appointed, and of the number of convictions which have taken place under the Act.

[Since the above was written, we happened to pass the shop of the individual whose bread was mentioned as pure in the report referred to in this article, and found the window half covered with huge placards, on which were inscribed an extract from that report, a copy of a certificate from another Chemist, and an address from the baker himself, announcing that he had entered into a bond of 50*l.* to sell nothing but the purest bread, and some further nonsense of the same sort. Can anyone doubt, after this, of the real value of the certificate obtained from the Analyst under the existing law?]

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Microscopical and Chemico-Microscopical Characters of various Starches and Grain-meals, by JOHN HORSLEY, F.C.S.

In making the following microscopic observations, a quarter-inch lens, having an angular aperture of 75°, should be used.

All objects, immediately after being moistened on the slide with any chemical fluid, should be lightly covered with an over-glass and dexterously focussed, so as to obtain the entire view of the reaction which takes place.

WHEAT FLOUR,

Under Water, appears to consist of large and small granules, chiefly of a flat, round character, with sometimes faint markings, as of a central spot or hilum, but without concentric rings.

Under Acid Hydrochloric, is rapidly dissolved, leaving only fibrous matter visible.

Under Acid Sulphuric, is rapidly acted on, both starch and fibre, the latter becoming more or less discoloured, assuming, after a time, a deep-red tinge.

Under Liq. Potassa Dil. (1 pt. and 2 aq.), rapidly

swells out, becoming more or less altered in form,—in fact, gelatinised, after a time.

Under Liq. Ammon. F., no action is produced, but everything is rendered much more distinct than with pure water.

Under Polariscopes, polarises faintly.

WHEAT STARCH.

With the exception of the absence of discoloration by acid sulph., the effects of the other tests are precisely the same as with wheat flour.

POTATO STARCH.

Under Water the granules are found to vary in size and shape, some being small and circular, others large, sometimes of a protuberant, at others of a flat, ovate shape. The larger granules are easily recognised by the distinctness of their concentric rings or markings, together with the hilum, which is very prominent, sometimes existing as a mere spot in their narrowest end, and at others as a crack or fissure.

Under Acid Hydrochloric, is rapidly dissolved, but if held up to the light, or looked down upon at an angle of 45°, looks as if pitted and gelatinised.

Under Acid Sulphuric, a most beautiful reticulated appearance instantly takes place all over the granules, which resemble so many fir-cones, with serrated edges, which gradually soften down and become gelatinised.

Under Liq. Potass. Dil., is instantly dissolved and gelatinised, much quicker than any other kind of starch, forming a transparent jelly.

Under Liq. Ammon. F., no action is produced, but the definition is greatly improved.

Under Polariscopes, polarises strongly, with long, black crosses.

MARANTA STARCH (ARROWROOT).

Under Water.—Under this head I include arrowroot from different localities; for in their general character, under chemical re-agents, they are analogous, although there is some little variation in the form and size of the granules: for instance, those of St. Helena are pretty regular in size and of a protuberant, or round, ovoid shape; whilst those of Australia and Africa, though similar, are somewhat larger, particularly the African, which is known by a peculiar roughened ring on the centre as though it were an attachment. The hilum in these kinds is situated for the most part in the centre, sometimes existing as a spot, at others as a slit or fissure. West Indian arrowroots also vary in size and shape of their granules, which are for the most part of an oblong ovate, sometimes protuberant, at others flat, having a hilum at the thicker end, the larger granules being marked with concentric lines, of a less distinct character than are observable in potato starch. To notice the other kinds of Marantas would, perhaps, occupy too much space.

Under Acid Hydrochloric, is rapidly dissolved, forming a smooth, transparent jelly.

Under Acid Sulphuric, is very different in its action from that observed with potato starch, and, although apparently but slightly acted on (if entire or unbroken), yet retains its outward form, more or less for some time, becoming, however, gradually gelatinised.

Under Liq. Potass. Dil., the effect is very similar to potato starch, but not quite so rapid in its gelatinisation, although its form is instantly destroyed.

Under Liq. Ammon. F., no reaction is produced, but definition is greatly improved.

Under Polariscopes, polarises well, but differently from potato starch.

RICE STARCH.

Under Water, the granules are small, and for the most part of an angular form, with central depressions and raised edges.

Under Acid Hydrochloric, gelatinises slower than other starches.

Under Acid Sulphuric, is rapidly acted on and resolved into a transparent jelly.

Under Liq. Potass. Dil., gelatinised slower than other starches, and is quite transparent.

Under Liq. Ammon. F., no reaction is produced, but definition is greatly improved.

Under Polariscope, does not polarise.

SAGO STARCH.

Under Water, the granules are of an ovate shape, often truncated and mullar-like, many of the particles being more or less broken. Their external surface is somewhat irregular, the hilum appearing sometimes as a round spot, at others as a kind of slit or fissure, like that of a cross. The concentric rings or annuli are not very distinct.

Under Acid Hydrochloric, is rapidly gelatinised.

Under Acid Sulphuric, is rapidly acted on, with a degree of raggedness at the edges, becoming gelatinised.

Under Liq. Potass. Dil., swells up immensely, but retains its form, and is quite opaque when opposed to the light.

Under Liq. Ammon. F., no reaction is produced, but definition is greatly improved.

Under Polariscope, polarises well.

TAPIOCA STARCH.

Under Water, the granules are for the most part of a broken and irregular appearance, but in their entire state are either circular or mullar-shaped, according to their position. The hilum is surrounded by concentric rings and fissures or cracks of a star-like form.

Under Acid Hydrochloric, is gelatinised even still more rapidly than sago.

Under Acid Sulphuric, is rapidly acted on, with a somewhat similar reticulation as noted under potato starch, and gradually softening down into a jelly.

Under Liq. Potass. Dil., is readily dissolved, forming a transparent jelly, but not so soon as potato starch or arrowroot.

Under Liq. Ammon. F., unlike the other kinds of starch noticed, it is rapidly acted on by this re-agent, and passes into a gelatinous state.

Under Polariscope, polarises, but less so than sago starch.

CANNA STARCH (TOUS LES MOIS).

Under Water, this kind is easily recognised by their size, being the largest granules known. Their form is somewhat of a protuberant or oblong ovoid, having a well-marked circular hilum at the narrow end, with well-defined concentric lines or annuli, and a very satiny appearance.

Under Acid Hydrochloric, is rapidly dissolved, and runs to a fluid state.

Under Acid Sulphuric, is acted on in a similar manner as potato starch, but does not wholly gelatinise, as a residue is left.

Under Liq. Potass. Dil., is readily dissolved, forming a transparent jelly.

Under Liq. Ammon. F., no reaction is produced, but is greatly defined.

Under Polariscope, polarises like potato starch, but not so strongly.

According to this Table it would appear that the

various starches enumerated have each some separate and peculiar re-agent by which they may be readily distinguished from each other, *e. g.* :—

Tapioca is readily distinguished from other starches by the action of ammonia, in which the others are unaffected. With that exception, I recommend ammonia as the best medium of observation, affording as it does such clearer definition.

Wheat Starch by liq. potass. dil., from the length of time which it continues more or less recognisable under its action, whilst potato, Maranta, and Canna starch disappear almost instantly.

Sago Meal is readily distinguished from ground tapioca by its action with liq. potass. dil. Again, tapioca is readily distinguished from sago by the action of sulphuric acid, to be presently noticed.

Potato Starch possesses in sulphuric acid a remarkable test by the beautiful reticulation mentioned. Two other starches, however, viz., Canna starch and tapioca, are more or less similarly acted on, but the forms of each, as well as other tests, render their distinction easy.

Maranta Starch, again, possesses a special test in sulphuric acid, from its apparent resistance of action, whilst all other starches more or less rapidly lose their form and become gelatinised.

In mentioning the action of sulphuric acid on starches, it is worthy of note that if the acid be diluted with two parts of water and allowed to cool before using,—say, for instance, you are operating on Maranta or potato starch,—that the weaker acid rapidly finds its way into the granules through the hilum, and, as soon as all air-bubbles are expelled, the inner walls begin to give way and the globules suddenly disappear.

(To be continued.)

TECHNICAL CHEMISTRY.

On the Combustion of Wet Fuel in Furnaces, by B. SILLIMAN, Jun., Prof. Gen. and App. Chem. in Yale College.

(Continued from page 153.)

“When the operation is fairly commenced if the water in the wet charge amounts to say fifty per cent. by weight of the fuel, the dampers of the fire chamber should be nearly or quite closed to exclude the air; vapour from the wet charge will then descend through the corrugations and otherwise into the fire chambers and support the combustion therein, while other portions of the vapour will enter the mixing chamber and complete the combustion there. If the fuel, however, contains much smaller quantities of water, more air in proportion should be admitted at the damper, the object being to admit no more air than will supply the deficiency of the vapour.

“Little if any of the boiler should extend over the mixing chamber. If any considerable portion of the mixing chamber is covered by the boiler its cooling influence will prevent the decomposition of the vapour and defeat the object of my invention. Great care should be observed in giving proper dimensions to the mixing chamber, for the perfection of the combustion and the efficiency of the furnace depend greatly upon it. The principal object of this chamber is to give the combustible carbonaceous gases from the fire, and the aqueous gases from the mass of wet fuel an opportunity of mingling together in such a manner and under such circumstances that the aqueous vapour will be decomposed by the carbonaceous gases, and its oxygen given out to complete the combustion of the carbon, without the introduction of air into the mixing chamber, thus saving the caloric pre-

viously communicated to the wet charge, while drying it and charring its lower portions, and avoiding the cooling influences of cold air. This can take place effectually only in the presence of a high degree of heat and in the absence of a supply of free oxygen. If this chamber be too small to receive these gases as fast as the furnace is able to produce them the operation will of course be choked and impeded. If the chamber is larger than can be kept densely filled with these gases, of course atmospheric air will be found there at the commencement, and will continue to find its way into the chamber, and while atmospheric air is present the carbonaceous gases will take their oxygen from that source principally instead of decomposing the steam, and the heat in the chamber will be much diminished and the large quantity of nitrogen ($\frac{1}{2}$) contained in the air, which is neither a combustible nor a supporter of combustion, will at once greatly increase the volume of gases to be sent forward to the stack and proportionably decrease its temperature; and when the chamber becomes very large the cooling influences become so great that combustion will immediately cease, and smoke mingled with steam oxygen and nitrogen, will go forward, thus wasting the fuel and imparting only a faint degree of heat to the boiler.

"I have therefore fixed the size of the mixing chamber by many careful experiments—and that given above will produce the desired effect with wet bagasse. For dryer fuels furnishing less vapour, the mixing chamber should be proportionably increased in size to supply the deficiency with air and to effect complete combustion. Rules more precise would be inconsistent with the nature of the subject.

"A large and hot fire should always be preserved in the fire chamber below the grate, and directly under the charge of wet fuel, for the purpose of driving the vapour out of it and charring its lower portion—and the grate is left much more open than in furnaces for burning dry fuel of the same size, for the purpose of allowing the charred portions of the wet charge to fall through to supply fuel for this fire as fast as it becomes fit for that purpose, thus consuming the mass with little or no expenditure of other fuel.

"What I claim as my improvement in furnaces for burning bagasse and other fuels too wet to be conveniently burned in the usual way and well known ways is:—

"First, the combination of two chambers, the one above the other, and separated by a grate, the lower one for the combustion of any known dry carbonaceous fuel, and the upper one in immediate proximity therewith to receive heat therefrom for heating and drying the charge of wet fuel, with a mixing chamber, into which both continuously and simultaneously discharge their gases before reaching the thing to be heated, for mingling and mutual combustion.

"I also claim in combination with said fire chamber and wet fuel chamber or drying chamber making the grate upon which the wet charge rests sufficiently open to allow the lower portion of the wet charge as it becomes dried and charred to fall through into the fire chamber and keep a hot fire therein, supplying the place of other dry fuel, while the uncharred portions of the wet fuel is properly supported by the grate till dried as described.

"I also claim placing the mixing chamber of combustion in substantially the same position described relatively to the fire, and the wet charge, so that the products of combustion from the dry fuel may pass along the lower part of the wet charge, drying and charring it on their way to the mixing chamber, and reach it without being in any considerable degree obstructed or cooled by the wet charge.

"I wish it distinctly understood that I make no claim to any of the parts or combination above specified except in their application to the preparation and combustion of wet fuels."

It will be observed that in this mode of combustion the wet fuel is subject to a constant process of distillation by the fire in the ash-pit. The products of this distillation re-act on each other in the mixing-chamber in the manner already described, while, at the same time, a portion of watery vapour is decomposed in the ash-pit.

Theoretically, no more heat can be generated in this mode of combustion than is consumed in the transformation of water into steam and the conversion of fixed into volatile products. But it is by no means a matter of indifference whether the oxygen requisite for complete combustion is drawn from the atmosphere or is derived from the decomposition of water by carbon and its oxide. In the former case, not only is there a great loss of heat carried away by the inefficient nitrogen of the air, but the diluted oxygen can never produce so intense a heat with the carbon as is the result of the reaction of the nascent oxygen with that element. Although Mr. Thompson was no chemist, he did not fail, with his natural acumen, to perceive this advantage, and in his earliest patent he remarks:—

"After ample experiments, I have discovered that any results that can be produced by the use of dry fuel are inferior to those obtained from my process, in proportion to the quantity used, and that results like mine can only be obtained by the use of wet fuel fed into an intensely-heated chamber: under such circumstances the water in the fuel, in presence of the carbonaceous substances in the furnace, will be decomposed, giving its oxygen to the carbonaceous matter, dispensing with a draught and its cooling and wasteful influence, and rendering the combustion so perfect that no smoke is visible."

Although this mode of combustion of wet fuel is now in use on many sugar plantations in Louisiana, and in some tanneries of Pennsylvania and New York, no notice of it has, so far as I am aware, appeared in the scientific Journals. I am not without personal experience of its operation on a large scale, having, in 1857, enjoyed the opportunity of studying carefully the management of one of Thompson's furnaces, in three compartments, built for the combustion of wet peat. That fuel contained over 75 per cent. of its whole weight of water and was too wet for the best results. But with the use of one-fourth part of dry wood, even this extremely wet and otherwise valueless fuel was rendered efficient, three cords (of 128 cubic feet) of wet peat and one cord of dry wood doing the work of four cords of dry wood in driving a steam-boiler.

On the Alloys of Copper and Zinc,

by FRANK H. STORER.

(Continued from page 151.)

THE most contradictory statements have been made by different observers in regard to variations of colour among the alloys of copper and zinc. In the specimens which I have myself prepared, no abrupt change or peculiar modification of the true colour is to be perceived, although some very striking superficial characters present themselves. Starting from the red of pure copper, the colour of the alloys is less red and more yellow in proportion as they contain more zinc, until an alloy containing 75 or 80 per cent. of copper is reached, the colour of which is almost pure yellow; beyond this point, as the proportion of zinc contained in them is increased, the alloys become less yellow, with, perhaps,

a tinge of green, and more white continually, and this by the most gradual stages, until the well-known white alloys are reached. These remarks refer to the colour of polished surfaces, as obtained by the use of a fine file. Each of the alloys enumerated in the list having been carefully compared with the chromatic scales of Digeon.¹

In several of the alloys, especially in those containing about equal parts of copper and zinc, a beautiful yellow, or sometimes gray iridescent film forms upon the surface of the metal during the process of cooling. On fracturing it, moreover, the fibres of which the mass is formed present the same yellowish or grayish iridescence. This is removed at once by filing, when the true colour of the alloy is made manifest, as has already been observed by Guettier² in regard to hard solder. With the exception of Guettier, most modern observers have described this alloy, prepared by fusing together 50 parts of copper with 50 of zinc, as being of a deep yellow colour; while Karsten³ states that it is considerably darker and redder than ordinary brass.⁴ They have, evidently all been misled by the superficial coating just described, the colour of which is completely analogous to that which occurs upon the crystals of metallic bismuth, now so often to be found in chemical cabinets, or by the colour of an almost equally superficial layer of yellow alloy, which forms upon those parts of the ingot which are suddenly cooled. To these changes, produced by "tempering," I shall presently again allude. That the surface colour is most probably mainly due to oxidation, however, would appear from the fact that not only has the brilliant yellow coating of my specimens of braziers' solder, and of other iridescent alloys, not been dimmed by exposure during several months in a damp cellar, where ordinary polished brass soon became tarnished, but in several instances in which it had been removed by filing, a somewhat similar yellow coat has re-appeared after exposure to moist air. Although this iridescence is very strongly marked in the alloy known as braziers' solder, yet an instance perhaps still more striking presented itself in the alloy containing 41.48 per cent. of copper. Being very brittle, a specimen of this alloy was accidentally broken into several fragments before it had become thoroughly cold, when a beautiful, glittering, iridescent film, of a light-yellow colour, at once formed upon its surface. As the alloy was very compact and fine-grained, it could at first sight hardly be believed that the colouration was only superficial; but on breaking the mass after it had become cold, its true colour was found to be a very light gray,—indeed, almost white,—as was also proved by filing off a portion of the yellow film. The ingot obtained from the liquid portion of this alloy, having been suddenly cooled, was nearly white, and presented no appearance of iridescence. Upon its under surface, however, which had been instantly solidified by contact with the cold stone upon which it was poured, a thin layer of yellow, malleable,

tempered alloy was found. In another instance, the ingot obtained from a white alloy containing about 30 per cent. of copper having been broken while still warm, the fractured surface instantly assumed a permanent bright blue-violet colour. Indeed, it would appear as if, by properly regulating the process of cooling, a series of colours as varied as those assumed by steel, according to the different degrees of heat at which it is tempered, might be obtained. Alloys containing about 45 per cent. of copper exhibit a dull-gray colouration on fracture, which, combined with their other physical properties, often causes them, when unpolished, to resemble, as was remarked by Karsten,⁵ metallic sulphides far more than metals.

Although the iridescence which I have described is perhaps seen to best advantage in the alloy from equal parts of copper and zinc, and may perhaps be produced most readily upon it and the neighbouring alloys of a fibrous texture, it is by no means confined to these. I have noticed beautiful yellow coatings upon alloys containing 60, 53, and particularly on the one of 90.77 per cent. of copper. It may probably occur upon any of the yellow alloys, and is, doubtless, produced at will upon some of them by manufacturers of ornamental brass castings.

In this connection it may be mentioned that the colour of the oxidized surface of the thin sheets of alloy obtained by pouring upon stone varies from a grayish-black, tinged with blue, in the specimens containing about 90 per cent. of copper, to a dirty, dark, yellowish-green, as in the alloy of 65 per cent. of copper; and from a dull lead colour (alloy of 38.5 per cent. of copper) nearly to white, in the alloys rich in zinc.⁶

Marked changes, not only of colour, but also in other of the physical properties of some of these alloys, may be produced by varying the conditions in which they pass from the liquid to the solid state. These changes are very peculiar, and are evidently of great importance, not only in their practical bearing, but also from affording another instance of the phenomena of "tempering," which may possibly be of consequence in the study of this most difficult subject.

In preparing the white alloys containing less than 45 per cent. of zinc, I have frequently observed upon the under surface of the sheet obtained by pouring off the liquid alloy, a thin layer of a soft, tenacious, malleable alloy, of a yellow colour. The formation of this singular sheet was at first attributed to eliquation; but as it was difficult to conceive that an alloy apparently richer in copper should remain at the surface of the melted mass in the crucible,—which must have been the case in order that it should form the bottom of the ingot,—it was thought possible that volatilisation of the zinc might have increased the proportion of copper at the surface of the melted alloy. In order to decide this question, a new series of alloys was prepared, extending

¹ Loc. cit., s. 396.

² "Corcles Chromatiques de M. E. Chevreul, reproduits au Moyen de la Chromocoleographie," par R. H. Digeon. Paris: 1855. Chez Digeon, Rue Gалаnde, No. 65.

³ Loc. cit., s. 207.

⁴ Loc. cit., s. 394.

⁵ When a piece of solder is heated gradually, it assumes at first a light-rod colour, which subsequently passes to violet, or even to blue violet; a beautiful play of colours, alternating between green, yellow, and violet succeeds, as the temperature becomes more elevated, until at length, after the lapse of some time, the specimen becomes tarnished with a thick, dull coating of oxide. Since in the foundries where it is prepared this alloy is often taken directly from the ingot-moulds and pounded to powder without having first been allowed to cool, the observation of Karsten admits of ready explanation.

⁶ Observations upon the colour of the alloys of copper and zinc which accord very nearly with my own are those of Guettier (*Dingler's Polytech. Journ.* (cxiv., 204, from the *Moniteur Industriel*, 1848), and of Lewis (*Chemical Works of C. Naumann, Abridged and Methodised, with Additions*, by W. Lewis, London, 1759, p. 65), who long ago wrote:—"The proportion of calamine and the increase which the copper receives from it are different in different works; hence the deeper or paler colour of brass. I have observed in a large set of experiments on this subject that a little of the calamine—that is, of the zinc contained in calamine—dilutes the colour of the copper and renders it pale; that when the copper has imbibed about one-twelfth its own weight, the colour inclines to yellow; that the yellowness increases more and more till the proportion comes almost to one-half; that on further augmenting the calamine, the brass becomes paler and paler, and at last white."

from the one obtained by melting together 40 parts of copper plus 60 parts of zinc, to the mixture of 56 parts of copper and 44 parts of zinc, each alloy being made from a mixture containing 1 per cent. more copper than that used in preparing the preceding. After thorough stirring, the alloys were cast into ingots of about five pounds' weight. In the first four of these alloys—from 40 to 44 per cent. of copper—the yellow film was very clearly defined on the three sides of the ingot which had come in contact with the metal of the mould, while on the upper surface of the ingot, which had cooled in contact with the air, no trace of it could be detected. This yellow film is usually of extreme tenuity, hardly exceeding one-sixteenth of an inch in thickness in any of the instances which I have observed. It is, however, so very clearly defined in the white alloys containing 40 to 45 per cent. of copper, being readily bent, cut, or filed, while the remainder of the alloy is exceedingly hard and brittle, that no question as to its identity can possibly be entertained. In the alloys containing more than 50 per cent. of copper, which are naturally of a yellow colour, it is not so easy to detect a similar film. In the series of ingots just mentioned it could, nevertheless, be traced as far up as the alloy prepared from 54 parts of copper plus 46 parts of zinc.

An experiment made in order to ascertain whether this soft modification of the alloys could be obtained at will, afforded no positive result. An alloy of 42 parts of copper plus 58 parts of zinc being prepared, a portion of it was poured by small drops into a large mass of cold water. The surfaces of many of the granules thus obtained were covered with a yellow film, and this was especially true of the smallest pieces; but the film was exceedingly thin, and the interior of the granules consisted entirely of brittle white alloy. Other portions were thrown out upon cold iron plates, so as to form very thin sheets. A thin, yellow film occurred, it is true, upon the inferior surfaces of these sheets, but above it the alloy was white and brittle. Another portion of the melted alloy was cast in an iron ingot-mould, which had previously been heated "black-hot." The yellow film was still formed where the alloy came in contact with the ingot-mould; it was, however, exceedingly thin. On pouring some of the remaining alloy into a cold iron mould, a yellow film, of the usual thickness, was obtained.

Although the phenomena of tempering exhibited by these alloys are more readily perceived among those the normal colour of which is white, it is highly probable that all the alloys are liable to have their physical properties influenced to a greater or less extent from this cause. This would appear, not only from the evident analogy of these phenomena with those which are known to occur with alloys of copper and tin;⁷ but Dussaussoy⁸ has already shown that ordinary brass,—copper 65 to 70 per cent. plus zinc 30 to 35 per cent.,—is rendered softer, less tenacious, and less dense by tempering it,—a fact which Mallet⁹ has very properly urged in explanation of the differences between several of the specific gravities of alloys of copper and zinc observed by himself, and the numbers which had been previously obtained by other experimenters. I would here suggest that it explains equally well the very great variations

among the specific gravities of the different alloys studied by Mallet himself.¹⁰ It is also the most probable explanation of the alloys of abnormal colour which he has described. I should observe in this connection, that in the alloys of light-yellow colour the yellow appears to be made much deeper when the alloy is strongly compressed, as when cut with a cold-chisel or hammered. I may also incidentally mention, that in the series of alloys which I have prepared a constantly-increased degree of hardness has been observed, in proportion to the amount of zinc contained in the alloy,—at least, as far as the brittle, white alloys which are so hard that an ordinary file makes little or no impression upon them,¹¹—a result which agrees with those of Karsten,¹² of Mallet,¹³ and of Guettier.¹⁴ Calvert and Johnson¹⁵ have recently attempted to express these differences numerically. The alloy containing 54 per cent. of copper, which is still so tenacious that small bits cannot readily be broken from it, was found to be exceedingly difficult to cut with a cold-chisel; for, though still so soft that it could be filed without any very great trouble, it destroyed at once the edges of cutting instruments, though the latter were well tempered. This peculiarity appears to be connected with the fibrous structure of the alloy.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE IV.

Synthesis and Decomposition of Water—Oxy-water—Ozone—Solution—Crystallisation.

At the close of the last Lecture I endeavoured to show you that when hydrogen unites with oxygen under ordinary circumstances, that is when we ignite hydrogen and allow it to burn either in contact with atmospheric air, or even in contact with pure oxygen, that water, and water alone, is the result. I want, however, to prove a little more strictly the composition of water, as regards the two elements that form it, and the proportions in which those two elements unite. Before doing so, however, allow me to remark that water is not the only compound of hydrogen and oxygen with which we are acquainted. It is the only one which this element produces directly; but there are conditions under which the two elements can be made to combine in other proportions, and then in addition to this compound called Water, which consists, you will recollect, of one equi

¹⁰ I have found it impossible to construct a curve of any regularity from the observations of this chemist, although it is tolerably clear that the greatest condensation is at a point somewhere below the alloy composed of equal equivalents of copper and zinc, and that between this point and pure copper the specific gravity gradually increases with some degree of regularity in proportion as the amount of copper increases. Among the observed specific gravities of the white alloys rich in zinc, there is no regularity whatsoever. The determinations of Mallet are, nevertheless, unquestionably the best which have ever been made; possibly they are the best which could be made in any case. At all events, few chemists are in the position to improve upon the method of preparing alloys which was employed by this distinguished observer.

¹¹ From this category the alloys modified by tempering must, of course, be excepted.

¹² *Loc. cit.*, s. 393.

¹³ *Loc. cit.*, p. 307; also "Gmelin's Handbook" (Cavendish Soc. Edition), v. 479.

¹⁴ *Loc. cit.*, *passim*.

¹⁵ *Loc. cit.*, p. 199.

⁷ Vide d'Arcet, in "Dictionnaire de l'Industrie Manufacturière, &c., x. 500, Art. "Tam-tam" (Paris: Baillière, 1841); also *Ann. Ch. et Phys.* (2), liv. 331. See also Dussaussoy, *Ann. Ch. et Phys.* (2), v. 225.

⁸ *Loc. cit.*, p. 226, et *Tableau*, No. 5.

⁹ *Loc. cit.*, p. 264.

valent of each element, we get another compound which consists of double the amount of oxygen which is contained in water. Here we have the composition of water [representing the elements by the cubical blocks, as in previous Lectures] consisting of one atom of each body; and that [adding a block for oxygen] is the second body that is formed—the so-called “oxy-water” or binoxide of hydrogen, which contains, as I have said, double the amount of oxygen. Under favourable circumstances we can cause hydrogen to combine with a third equivalent of oxygen producing a teroxide which has been called *ozone*. There is, however, some doubt about the identity of this compound with ozone; but we have independent theoretical evidence that this body, the teroxide of hydrogen, does exist.

We will confine our attention to-day to the examination of the simplest combination of oxygen and hydrogen—liquid water. You will recollect that we collected in this apparatus the gases of which water is composed. We tore asunder, by means of the electric force, the two constituents of the water contained in this vessel, and we collected them in the two vessels placed over the poles of the decomposing cell. We will now examine the properties of these gases. You will notice that one of these tubes is entirely filled. Let us try first the properties of that gas contained in this tube entirely filled. [A light was applied to the end of the tube and the gas burned.] You see we have obviously here our old friend, hydrogen—inflammable, incapable of supporting combustion, but burning at the mouth of the vessel. The jar is inverted; and by the great levity of the gas it remains in the vessel for a considerable time. There can be no doubt that this is hydrogen. Now, let us try what kind of gas we have in the other tube in which we have been collecting. I believe we shall find this to be oxygen; we will, therefore, apply the proper test at once, namely, an extinguished piece of wood, which [lowering it into the jar] you see is at once ignited when it goes into the vessel, proving that we have free oxygen there present. This, therefore, is what we may term the analytical proof of the composition of water. We have taken it asunder and find that we get these two elements, oxygen and hydrogen, and that the hydrogen exists in the water in double the volume that oxygen exists, there being two volumes of hydrogen and one of oxygen; and inasmuch as that hydrogen is sixteen times lighter than oxygen, it follows as a matter of course that there must be eight parts by weight of oxygen to one of hydrogen. This [referring to the tube of hydrogen over the decomposing apparatus] weighs only one-eighth as much as the smaller quantity of oxygen we have collected in the other tube. By volume we have twice as much hydrogen as oxygen; by weight we have eight parts of oxygen to one of hydrogen.

In addition to this analytical proof of the composition of water I want to give you the synthetical proof of the same thing. We will analyse the water by means of the electric current as we have done before; but we have an arrangement here by means of which we can collect both gases together as they are given off from these poles. We can collect them in this receiver. A certain quantity has been collected before the Lecture, in order to save time, and now we are collecting a larger quantity merely to show the manner in which the experiment is made. Here I have a very strong glass vessel in which it is possible to ignite these gases without the slightest risk of the fracture of the vessel. We are pumping out all the atmospheric air which it contains, and then we are going to screw it on to this receiver, when by simply opening the stop-cock and connecting the vacuum with the jar in which the gases are collected they rush into the vacuum and we thus fill the strong jar with the gases. At the end of this jar we have platinum wires so arranged that an electric spark will be made to pass

through the mixed gases, and it will infallibly explode them. I had better, perhaps, show you more clearly before I screw it on to the stand that this vessel is perfectly dry and transparent. The quantity of water we shall produce will be very minute. When you consider the enormous volume of these gases we get a very small quantity of water by combining them; yet I expect from once filling this vessel we shall get sufficient to enable us to perceive, at all events, the dew upon the interior of the vessel,—perhaps not so much quite as to enable us to distinguish distinct drops, but I think you will notice that the transparency of the vessel is very materially diminished after we have exploded this gas. We have here an arrangement by which a spark may be passed. You will see a bright flash pass through that jar when the explosion takes place, due to the combination of two gases. [The electric spark was then passed through the mixed gases.] There, they have combined, and now let us examine our vessel. You see there is no doubt whatever that water has been formed—there are, in fact, numerous little drops of water upon it; and the opalescent appearance of the vessel will be visible to all in the theatre. This, then, gives us the synthetical proof of the composition of water, for if we mix the gases together and then explode them in this way, we have virtually a vacuum produced in the vessel. No gaseous matter exists there, and the compound which is formed is absolutely pure water.

Water possesses a great many important properties in all three states of aggregation. We are acquainted with this chemical compound in the solid condition in the form of snow and ice, in the liquid condition, and also in the gaseous condition in the form of steam. In all these three states it possesses great interest for us, but in the solid and gaseous conditions it is almost entirely devoid of any chemical interest, and I shall therefore pass over those forms. We must, however, examine some of the properties of liquid water, because this serves not only to assist the action of the chemical force itself, but water is a liquid which enters very frequently into chemical combinations, and it is quite necessary we should understand some of its general properties at the outset of our chemical investigations.

We have already seen that water is transparent, inodorous, and tasteless, but that it is not absolutely colourless: we have seen that it possesses a bluish-green colour, which first comes out when we look through a considerable stratum of it. Water is also very incompressible. This is not exactly a peculiarity of water, for all liquids possess this property to a very great extent. For a long time it was imagined that water could not be compressed at all, no matter what amount of force was applied to it; but it has since been shown that it is compressible to some extent. The experiment is exceedingly difficult to make; and it is still more difficult to demonstrate this compressibility. In the first place the experimenters enclosed the water in a metallic globe, and succeeded in squeezing it in a little; but then came the question whether in squeezing it in in one part it was expanded in another? but, even if it were granted that no expansion took place at the equator through the pressure at the poles, still the water might make its way out through the pores of the vessel. We are indebted to Oersted for the first demonstration of the compressibility of water; and the apparatus which he designed is one which I have here, and which I must explain to you in a few words as possible. It was an application of pressure to the external as well as the internal surfaces of the vessel containing the water, so that there should be no question as to the escape of the fluid or the expansion of the containing vessel. Oersted's apparatus consists of a very strong glass cylinder, which must necessarily be of small diameter to enable it to resist the great pressure. It is closed at both ends by ground-iron plates screwed together by rods and nuts. This cylinder is filled with water, and it is fitted

at the upper part with a little forcing pump, by means of which water from this reservoir can be forced into the cylinder which contains the vessel in which the compression of the water takes place. You cannot see it because it is turned away; but even if it were turned towards you it would not be visible to those at a distance. I will, however, project an image of it upon the screen; but before doing so, let me direct your attention to the apparatus in the interior of that cylinder. This [explaining from the diagram] is a thin glass bulb, or cylinder if you please, terminated here by a capillary tube, which tube is again re-curved upwards, and terminates in a smaller cistern open at its extremity. This cistern is filled with mercury to the entire exclusion of air, and the quantity of water is so regulated that the mercury stands somewhere about the middle of this scale which is attached to the instrument. Now, the whole of this, you will recollect, is contained in that cylinder of water, so that the pressure will take effect both upon the exterior and—through this opening here, communicating with the surface of the mercury—upon the liquids mercury and water. Now, if by the application of this pressure the water be really reduced in volume, it is evident that the water contained in this cylinder will be forced inwards towards this inner cylinder; or, in other words, that the liquid metal will rise up the stem towards the cylinder. Then there is another part of the apparatus which you will also see upon the screen which I must request you to take no notice of. This little air-gauge is simply a measure of pressure, and it is more for my own guidance than for the purpose of allowing you to observe the force applied. I want simply by this instrument to apply the pressure in such a way that I may not run the risk of applying too much, and, on the other hand, that I may apply enough. The pressure takes place on the mercury, and is propagated through the mercury to the air in the tube. The air will be reduced in volume according to the pressure applied. When reduced to one-half its volume it shows a pressure of two atmospheres. The pressure may be four, five, or six atmospheres, which is the extent of the scale, and I don't propose to go much beyond that. I will endeavour to get a clear image of this apparatus upon the screen. [The room was then darkened.] All the apparatus is here inverted upon the screen, and this is the column of mercury which, in reality, is extending upwards from that cistern which I pointed out in the diagram, and which in reality will pass upwards when the pressure is applied, but, being inverted on the screen, it will come downwards. If I produce any compression of the water by working the pump, this column of mercury, the brilliant surface of which you see here, will gradually creep down the scale, while at the same time the mercury in the pressure-gauge will also descend, but to that I do not direct your attention, as it is merely for my own guidance. Now I will work the pump. You see the column of mercury has come down very considerably. It is now here [pointing]. This is the column you will mind; not this one. I hope it will be seen by those at a distance, as the motion of the mercury there is very considerable. If you notice that column I think you will see that it will suddenly jump back again when I remove the pressure. I will bring up the pressure to about its maximum mark. Now I will remove it, and you see the column at once recedes. What we effected was the forcing of the mercury in this stem higher up towards the cylinder there. The mercury appeared, however, to be coming down, because the image was inverted. The amount of this property of compressibility Oersted determined, and found it to be fifty-one millionths of the volume of the water for one atmosphere; so that you see it requires a very delicate arrangement to make it perceptible, and very considerable magnifying to render it at all visible at any distance.

This property of almost perfect incompressibility is made

use of in a very interesting manner in Bramah's hydraulic press, of which I have here the model belonging to the Institution. It is an instrument by which the pressure exerted on a small piston is communicated to a larger one, every square inch of which is pressed upon by the same pressure as is applied to the smaller one. The mode of its action will be more clearly shown by the aid of this rough diagram. To the right is a pump, which forces water out of the cistern below. Here there is a small piston, and there is a much larger plunger, which is capable of rising up and going down, and carries a table with it. The pressure of the water under the smaller plunger forces up the larger one. That [referring to an auxiliary arrangement] is an arrangement of valves which I need not describe; it is merely for the purpose of preventing the water going back to its former position between the strokes of the pump. To return to the model of the apparatus. You see, if I work this pump, at every stroke this little platform rises to a certain extent; but it rises apparently so very deliberately, and so very softly, that one would not think that it would exert a very considerable amount of force. We shall see, however, that the force which it exerts is so enormous—for such a small instrument, at least—that it would be surprising to those who saw it for the first time. We will try to fracture a small oak beam with it, which obviously would require a great force to break it across in this way [transversely]. We will put this beam upon two bars of iron, and then put another bar above in the centre of it, so as to apply the pressure in the centre of the beam. Now, you see the table rises slowly and quietly, but it won't stop when the beam tries to arrest its motion [the oak beam began to crack, and at last broke in two]. It breaks it with apparently very great ease. This is the apparatus which is so much used in packing calico, and other bulky materials of that kind, for exportation. Those materials, made up into bales, are submitted to the enormous pressure of these hydraulic presses, and are compressed into a fraction of their original bulk, thus allowing a much larger amount to be stowed away in the hold of the ship than if they were packed in their loose condition.

There are several peculiarities of water, which are interesting from their bearing upon the different offices of this liquid in nature. The first of these peculiarities is that whereas most liquids on being heated expand, and on being cooled contract, water at a certain temperature does not possess this property. If you apply heat to water at its freezing point, the water of course becomes hotter, but instead of expanding it contracts. It diminishes in volume until you come to the temperature of 39.2° F. At that temperature it possesses its maximum density. It has then contracted to the greatest possible extent to which water can contract. If you apply heat beyond this point it begins to expand. Now, although this is very easy to explain verbally, I think I had better show you the phenomenon experimentally. The apparatus consists of what we may call a water-thermometer. There is a bulb, and it terminates in a capillary stem. If we reduce the temperature of the water contained in the thermometer to 32°, as we have done here by immersing it in ice, then we shall have obviously the volume of water at 32° marked upon this stem by the mercury in the instrument.

I intend to project that upon a small screen here, and then we will gradually apply heat to the bulb. You will see that the application of heat will cause the mercury to ascend in the tube until the water reaches its maximum density, when the water will begin to expand again. But here you must recollect again that the vessel will appear inverted, therefore the water, instead of going upward, will go downwards in contracting. We shall receive the image on this screen, which I have marked with the letter E at the top, and C at the bottom, to show that the mercury going in this direction

[towards the letter E] indicates Expansion of the water, and that the mercury in going in this direction [towards the letter C] shows Contraction. I will first show you the position of the mercury in the stem whilst the instrument is in the ice. [The room was again darkened, and an image of the apparatus projected upon a white screen by means of the electric lamp.] Now we have the mercury at this point. We will draw a line on the screen at the point where the mercury stands, and we will endeavour not to move the apparatus, but simply to take away the ice which here surrounds the little bulb, and I have no doubt that even by simple exposure to the air in that way, you will notice contraction taking place: you see that opaque portion of the column has passed considerably below the line already. Here is a beaker of water containing water at the temperature of the room simply; and in order to facilitate the heating of this bulb—the warming of it, at least—we will immerse it in this beaker. Now you see how that water has contracted—how it has descended below that mark, and on the further application of heat we may get a little more contraction; but the moment the expansion begins you will notice it by the mercury receding again towards that line, and it will ultimately, I have no doubt, repass the line. It is now beginning to recede. We shall have to wait for a few seconds in order to get the necessary temperature. It is now very obviously receding slowly, but it will do it more quickly when this lamp has made the surrounding water a little hotter. There it goes back: the water has reached its point of maximum density, and the mercury in the stem is now going back again, and would gradually pass down the tube until the water reached its boiling point.

This is one peculiarity of water, and a most important one it is too. You will readily understand the effect of it, if you consider what is the action of the cold of winter upon rivers, or upon lakes, for instance. The surfaces of these masses of water are acted upon by the cool air resting upon them, and are gradually, and in very frosty nights, of course, are rapidly cooled. As the water is cooled on the surface the particles become denser, and the particles go down below, and abstract the heat from the portions below. If this went on, the cooling would be a very rapid process—as rapid as the process of heating, in which the heat is conveyed throughout the fluid; but the moment the water has cooled to $39^{\circ}2'$, about 7° above the freezing point, it is no longer capable of contracting by the cold, but the water at the surface becomes lighter than the water below, and floating, forms a sort of blanket of water, which protects the water below from the cold, water being a very bad conductor of heat.

Another important point connected with water is the comparatively large amount of heat which it requires to raise its temperature from one point to another; or, what is the same thing, the large quantity of heat which it requires to get rid of, in order to have its temperature reduced from one point to another. It far exceeds all other substances in this respect. This property—its high *specific heat*,—is represented in a table we have here, water being taken, for comparison, as unity.

Specific Heat of Solids and Liquids.

Water	1.000	Copper	.095
Ice	.900	Zinc	.095
Alcohol	.660	Silver	.057
Ether	.510	Tin	.056
Nitric Acid	.442	Mercury	.035
Sulphuric Acid	.333	Gold	.032
Carbon	.241	Platinum	.032
Sulphur	.202	Lead	.031

Ice would require $\frac{1}{10}$ ths the amount of heat that would be required by water to raise its temperature through the same number of degrees. Alcohol would not require much more than $\frac{1}{10}$ ths; ether rather more than $\frac{1}{10}$; nitric acid between $\frac{1}{10}$ ths and $\frac{1}{10}$ ths; sulphuric acid little less

than $\frac{1}{10}$ ths; and so on, the numbers go gradually down here, till at last we get to the metals. Mercury, you see, will require only $\frac{1}{10}$ ths of the quantity of heat to be added to it in order to raise its temperature through the same number of degrees as water. Let us see if I can at all render this clearer by a simple experiment. I will pour into this vessel a pound of boiling water. We have a mark upon the vessel to indicate the proper quantity. I will now bring it near to a thermo-electric pile, which will act upon the magnetic needle there and show us any variation of the temperature of the contents of the vessel. I think the effect will be almost too strong if I bring it in contact with the pile. The only thing to be guarded against is the movement of the vessel, therefore I will endeavour, in the subsequent parts of the experiment, to hold it quite firmly. Now let me try to get the needle perfectly steady. This instrument, you see, shows us that heat is being radiated at a certain rate against the end of this thermo-electric pile, and the amount of that heat is correctly registered by the deflection of the needle. If I reduce the temperature of that water, if I pour into it a pound of cold water, for instance, I should reduce the heat to the mean between the two portions of water. That, I think, you will readily allow. But now I am going to pour successive pounds of mercury into the vessel, and we will watch the effect upon the needle. [A pound of mercury was then added to the hot water.] Now there is a quantity of mercury there exactly equal in weight to the water I have employed. I think you will see that it produces no apparent effect on the temperature—certainly some effect, but not an apparent effect. There is a second [pouring in another pound of mercury]; the needle manifests no tendency to go back to its original position. There is a third pound; and, in order to save time, we will put a fourth, a fifth, and a sixth pound into the hot water. There is a little reduction in temperature; the mercury has cooled the water a little, but you see the effect is very slight. The mercury has brought back the needle to a scarcely perceptible extent towards its original position. Although we have added six times its weight of mercury to this water, we have effected but a very small reduction in the temperature; and, in order to produce the effect of a pound of cold water we should have had to pour into the hot water rather more than twenty-seven pounds of mercury;—this would have cooled it only as much as it would have been cooled by the addition of one pound of cold water.

This peculiarity of water is of the highest importance in maintaining water in the liquid condition—the condition in which it is required in nature. This property renders it very difficult for water to exist long in any other than the liquid condition in which it is absolutely necessary for vegetable life, and certainly absolutely necessary for animal life. This high degree of specific heat which we have just now been demonstrating renders the cooling of a mass of water obviously a work of far more time than it would be if water possessed only the specific heat of mercury. Why, in such a case, an exposure to air below the freezing-point would effect the cooling of the surface of seas, of lakes, and of rivers twenty-seven times more rapidly than under present circumstances.

The next peculiarity of water is the great amount of heat which it absorbs in passing from the solid to the liquid condition—the great amount of “latent” heat, as it is sometimes termed, although perhaps rather unphilosophically. This amount of heat is that which is required to melt ice, we will suppose at 32° ; for water freezes and ice melts at that temperature. This amount of heat is that required to convert ice at 32° into water, not to raise its temperature, but merely to melt it. But in order to do this we require an amount of heat which, if not rendered latent, would raise the temperature of

the water through 142° F.; therefore the latent heat of water is said to be 142° F. as we have it in this table.

Latent Heat of Liquids.

	Measured by water.
Water	142° F.
Sulphur	$27^{\circ}14$
Lead	$11^{\circ}0$
Zinc	$48^{\circ}3$
Bismuth	$23^{\circ}25$

The latent heat of sulphur is $27^{\circ}14$; that is, it would raise the temperature of water through only that number of degrees. That of lead is 11, of zinc $48^{\circ}3$, and of bismuth $23^{\circ}25$; so that you see water is most strikingly pre-eminent as to the quantity of heat necessary to render it fluid, and consequently the amount of heat which must be given out by the conversion of liquid water into solid water or ice. Here you see a beautiful arrangement of Nature tending to cause water to retain its fluid condition for a very long time. If it were not for this provision a very large amount of ice would be formed in our mild climates during our comparatively short winters.

I have one or two memoranda here to which I ought to direct attention with regard to this peculiarity of water. We have just seen that "water is most dense at $39^{\circ}2$ F. At this temperature it expands both by heat and cold. Water requires more heat than the same weight of any other liquid or solid to raise its temperature through a given number of degrees. One cubic mile of water in cooling through one degree warms 3076 cubic miles of air through 4° ;" hence the effect of water surrounding portions of land—surrounding islands—the effect of the sea upon the climate of islands. The air passing over the surface of the ocean in summer is cooled by the water which absorbs heat without becoming much warmer, and gives out heat without becoming much cooler. "One cubic yard of ice in melting cools 21,000 cubic yards of air from 52° to 32° ;" hence you will understand the effect upon climates of dense masses of ice surrounding any sea coast, as the coast of Greenland, for instance, and the northern part of Iceland, the climate of which is known to be affected by the absence or the presence of this packed ice on the northern shore. Let me try to give you a proof of this high latent heat of water. Here in this beaker I have a mass of crushed ice, and I will place the beaker in contact with our pile. I think you will find that the needle will now go 'in the opposite direction. It will now be deflected, by the cold I am applying to the apparatus, in a direction opposite to that which it took before. And now I want to apply a considerable amount of heat to the ice. I will pour, in fact, a quantity of water upon it in a boiling condition, and we will see whether it will affect the needle or not. The needle will give us prompt indication of any heat which will be produced by the action of the boiling water, and we have seen that boiling water poured into a beaker, when the beaker is placed at some little distance from the pile, tells at once upon the needle, which is thereby powerfully deflected. I first want to get the needle stationary again. I will now pour upon this ice a quantity of boiling water, and mix them well up together [stirring the mixture of ice and hot water contained in the beaker with a rod]. You see our needle is quite stationary: notwithstanding this large quantity of boiling water that we have added to this ice, you see the needle stands at the same point, and I can go on adding boiling water until the ice is quite dissolved, the temperature of the vessel remaining still at the freezing point, as it was at the beginning of this experiment.

Water possesses very considerable solvent power, which is employed by the chemist to separate substances which are capable of being dissolved in water from those which are incapable of solution. When water has dissolved as much of a substance as it can dissolve, it is then said to be saturated. If you at this point remove some of this water by evaporation, or if the solution has been

effected by the agency of heat, and you then diminish the temperature without removing any of the water, a certain amount of this solid matter will be deposited, and deposited in beautiful geometrical forms, which we all know by the name of *crystals*. Here are some of them modelled in glass. I will try, however, to show you one or two actual processes of the formation of these crystals by means of the electric microscope. I will place upon a glass plate here a solution of a salt which is thus capable of crystallising on evaporation. Here is a solution of chloride of ammonium, which we will pour upon the plate. It is a nearly saturated solution, so that I hope the crystals will be very speedily formed; but still, in order to facilitate matters, we will warm the plate a little, so as to produce that effect of super-saturation which takes place when a portion of the water is removed from a saturated solution in the way I have just described [the room was again darkened, and a magnified image of the plate was projected on the screen]. Now, I dare say, if we look carefully at this plate for a few moments, we shall see this operation going on there. There is the separation of this solid body, the chloride of ammonium, branching out in those beautiful vegetating forms; and you will also see, I dare say, presently, the effect of little nuclei of solid bodies in determining the direction of the crystals. There are usually particles of dust floating about in a solution which generally determine the crystallisation in a particular manner. There we have the formation of a crystalline mass of this kind upon a little thread, that has been present in the solution, and that is gradually increasing in size, as well as those which are forming upon the margin of the plate. How beautifully they shoot! There, you see now our crystals from the string are growing. There was a little particle of dust, no doubt, at the commencement of that point—that sword-like crystal at the bottom of the screen. I will not detain you longer with this salt; but I want to show you the crystallisation of a salt from a solution of alcohol. This is a solution of iodide of stanethyl, which usually crystallises very beautifully on glass; and the solution evaporates much more rapidly than an aqueous one, so that we have not so long to wait for our results. It is sometimes difficult to get our solution into the microscope before the crystallisation actually begins [a glass plate covered with the alcoholic solution was then introduced into the microscope and projected on the screen]. There we have some of them; I think they will begin to grow immediately. How beautifully they shoot across there from the side! I think we have now quite covered our plate with crystals. Every part of the solution seems already to have crystallised.

These, then, are some of the more remarkable properties of water, both in its physical and its chemical aspects. From the latter point of view, with special reference to its occurrence in nature, and the kinds of water with which we are usually supplied as a beverage, we shall have to study it a little more in the next Lecture.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, March 1, 1861.

On Bunsen and Kirchhoff's Spectrum Observations, by Professor H. E. Roscoe.

(Continued from page 155.)

To turn now to the second, and, if possible, the most interesting part of the subject, namely, the conclusions drawn from these observations respecting the composition of the sun's atmosphere. It has already been said, that the importance of these experiments in extending our knowledge of the composition of the earth's crust is obvious. We have seen that those bodies supposed to be rare can be shown to be most widely distributed;

elementary substances, whose existence has hitherto been overlooked, by these means have been discovered; but we do not so clearly see what these observations have to do with the sun, or how it is possible from such experiments to draw any conclusion respecting the existence of bodies well known on this earth, as iron, magnesium, potassium, and such like, in the sun. I shall endeavour, as shortly as possible, to answer this question, and to prove to you that we are now as certain that iron, magnesium, nickel, potassium, &c., exist in the sun as that they exist upon our globe.

The solar spectrum invariably contains, as I have before said, a large number of dark lines, or spaces, or shadows. These have been called "Fraunhofer's lines," from the name of the discoverer. What do these lines show? They show us that in the sun's light, certain kinds of rays are absent, and as these black lines are always present in exactly the same position, we see that certain kinds of rays are always wanting in the sun's light. There are many thousands of these lines in the whole length of the spectrum. I have here a representation of a few of the most important lines in only a short portion of the spectrum between E and F [pointing to the diagram of the solar spectrum]. This shows thousands of dark lines; it is the most accurate drawing we have at present of the lines in this portion of the spectrum, and represents the lines occurring from the line E in the green to the line F in the blue. Between these points we have hundreds and hundreds of lines which you may notice here. And through the whole length of the spectrum, from the outer red to the violet there are thousands of these dark lines, which are always present, and which we must remember are the characteristics of solar light only, for the light from a fixed star or other self-luminous body is not similar to the light that we have from the sun: there may be dark lines in their spectra, but they are not the same dark lines as are in the sun's spectrum, or arranged in the same manner.

What causes these constant dark lines in the sun-light, for we must remember that it is in sun-light alone these particular lines occur? It is the discovery of the cause of these dark lines by Kirchhoff which gives such peculiar interest to this subject, because it enables us to draw conclusions respecting the composition of the sun's atmosphere, and I do not know that I can make the matter more clear to you than by reading the following sentences, into which I have endeavoured to put the points of the case. First we will take Kirchhoff's discovery.

1. The solar spectrum invariably contains certain fixed dark lines, called "Fraunhofer's lines."

2. The spectra produced by the luminous vapour of all metals contain certain fixed bright lines invariable and distinct for each metal. I have tried to show you on the screen what you would have seen if you had looked through these instruments. All and each of the bright lines so produced by certain metals,—for instance, by sodium, potassium, magnesium, and iron,—are found to coincide exactly with certain of the dark fixed lines of the solar spectrum. Hence there must be some connection between the bright lines of the metal and the dark solar lines. Perhaps it would be well if I were to explain to you what I mean by "coinciding exactly." If you look at the solar spectrum, and at the same time you allow not only the solar light, but the ray, for instance, from sodium to fall on it, you see the sodium line coincide with, that is to say, falls absolutely and exactly upon, one of the solar dark lines. If you take another metal, such as iron, which contains a great number of these bright lines, all as fine as a spider's web, and of different degrees of shadow and of colour; if you allow the iron spectrum and the solar spectrum to fall together, for every bright line which the iron spectrum produces there is a corresponding dark line, absolutely identical with it in tone and breadth, in the solar spectrum. Hence there must be

some connection between those bright lines of metal, and the dark solar lines.

The connection is as follows:—

Each of the dark fixed lines of the solar spectrum is caused by the presence in the sun's atmosphere of the luminous vapour of that metal which gives the coincident bright line. Perhaps I shall make the matter a little more clear by taking the one single instance of sodium. Why do we conclude that there is sodium in the sun's atmosphere? The light emitted by luminous sodium vapour is homogeneous; the sodium spectrum consisting of one double bright line. This bright double sodium line is exactly coincident with Fraunhofer's dark double solar line D, as will be seen by reference to this diagram [pointing] and to the image on the screen. And this we observe in the case of other metals where there are hundreds of lines, not one line only, as is the case with the sodium. The spectrum of a Drummond light is a continuous one. It contains no dark lines or spaces. If—and this is an important point to which I shall wish to draw your attention—if between the prism and the Drummond light a soda flame be placed, a double dark line is produced. That dark line is absolutely identical with Fraunhofer's double line D which we found in the solar light, and we have in this way artificially produced a kind of solar light, or, at any rate, one of its characteristics, the line D.

If, instead of using Drummond's light, we pass sunlight through the soda flame, we see that the dark line D becomes much more distinct than when sunlight alone is employed. The soda flame has therefore the power of absorbing the same kind of light as it emits; it is opaque for the yellow D rays. Hence we conclude that luminous sodium vapour in the sun's atmosphere is the cause of Fraunhofer's dark double line D, the light given off from the sun's solid body producing a continuous spectrum. In a similar manner the presence in the solar atmosphere of iron, magnesium, nickel, and chromium has been proved. I have here a tube which was a moment ago a hydrogen vacuum (but which I have unfortunately broken at one end), containing metallic sodium. I have here the power of producing a very large sodium flame—a yellow flame of soda. If I heat the sodium which is contained in the tube, we shall get the latter filled with the vapour of the metal sodium. Now, the vapour of this metal sodium is opaque to the yellow soda light; so that if I hold this tube up between the large soda flame and this white screen, we shall see that the tube is black. But if I hold it between a coal-gas flame and the screen, you will see that the vapour of sodium is perfectly colourless.

Kirchhoff's own views may perhaps render this point clearer. He has observed that the sodium vapour is absolutely opaque to the light which is given off by a flame coloured yellow by soda. His theory, then, is, that there is in the sun a glowing gaseous atmosphere surrounding a solid nucleus of possibly a still higher temperature. "If we could see," says Kirchhoff, "the spectrum of the solar atmosphere without that of the solid nucleus, we should be able to see in it the bright lines characteristic of the metals which that atmosphere contains. The more intense luminosity of the internal nucleus does not, however, permit the spectrum of the solar atmosphere to become apparent; it is reversed according to my newly-discovered proposition; so that, instead of bright lines, which the luminous atmosphere by itself would have shown, dark lines only appear. We do not see the spectrum of the solar atmosphere itself, but a negative image of it. By it we are, however, able to detect, in an equal degree of certainty, the metals present in the sun's atmosphere. All that we require for this purpose is a very accurate knowledge of the solar spectrum, and of the spectra of the individual metals." So says Kirchhoff, who is at present engaged in continuing these observations, and although only eighteen months have elapsed since the first

discovery was made he has already traced more than seventy lines on the solar spectrum between D and E produced by iron, and that the green line, known as "small b," is caused by magnesium. There are, likewise, coincident lines to prove the presence of nickel, chromium, potassium, and sodium in the sun's atmosphere: I much regret that I am unable to show you even a drawing of these coincident lines, as no representations of them have yet been published. On the other hand, it has been observed that lines produced by other metals, possessing very distinctly-marked spectra are not seen to coincide with any of the dark solar lines; and hence the conclusion can be drawn, that these metals—for instance, silver, copper, gold, lead, tin, antimony, etc.,—are not present in the solar atmosphere. I shall not soon forget a visit which I paid to Heidelberg last autumn, when I saw, for the first time, the phenomena here described. In the lower half of the field of the telescope there were, at least, seventy brilliant lines belonging to the iron spectrum of various colours and of all degrees of intensity and breadth; whilst in the upper half of the field the solar spectrum exhibited its steady light, cut up as it were by hundreds of dark lines. Situated exactly above each of the seventy bright iron lines was a corresponding dark line in the solar light. These lines did not only coincide with a degree of sharpness and precision perfectly marvellous, but the intensity and breadth of each bright line was so correctly preserved in its dark representation, that the fact that there must be iron in the sun flashed on my mind at once.

Any reflections of my own on these discoveries would be out of place. I would simply remind you that these discoveries are still in their infancy; they seem to herald the birth of a new kind of terrestrial and stellar chemistry. Let us then look forward into the vast ocean of unexplored truth which thus opens out to our view, and endeavour, if possible, to realise the vast magnitude of the knowledge which will thus be granted to mankind.

PHARMACEUTICAL SOCIETY.

Wednesday, March 6, 1861.

T. N. R. MORSON, Esq., President, in the Chair.

Professor BENTLEY made some "Remarks on the Adulterations of Senna." It will be remembered that, some twenty years ago, Alexandrian senna was extensively adulterated with argol leaves. The adulteration was no doubt intentional, and was sometimes carried so far, that as much as one-third of a sample consisted of argol. In consequence of the unpleasant effect produced by Alexandrian senna, which was generally ascribed to the argol, its use was discontinued in the Dublin and Edinburgh Pharmacopœias, and Tinnevely was introduced in its place. At that time several members of the Pharmaceutical Society joined in a protest, and agreed that, if the senna was not sent pure, it would not be sold, the consequence of which would be a considerable loss to the Egyptian Government. The effect of this remonstrance was that the adulteration diminished at once, and in time almost ceased, and thus a systematic fraud was stopped by the influence of the Pharmaceutical Society. In the year 1852 Professor Bentley had found it impossible to obtain a specimen of argol. In 1858, however, he had occasion to examine some bales of Alexandrian senna, and then found a considerable quantity of argol. He did not know then to what to ascribe the adulteration. There might have been a short crop of senna, or the remonstrance might have been forgotten. But, since 1858, he has repeatedly examined samples of senna, and found the adulteration to be going on as much as ever. Several specimens of Alexandrian senna were exhibited to the meeting. In one the argol made up one-fourth of the weight, in others it amounted to a fifth or an eighth. A hundred grains of one specimen

which had been carefully separated by one of the pupils, yielded 42 grains of senna leaves, 33 grains of argol leaves, 9.5 grains of flowers and seeds, principally of argol, and 10 grains of stalks, the remainder being dust, &c. All this adulteration takes place abroad. On the question whether argol was useful or injurious, the Professor said authorities differed. In Egypt it was taken alone. Dr. Christison asserts that it does gripe, and also produces sickness; but others have said that it has no action at all. It has been said that the unpleasant effects of senna are produced by the stalks, but that appears to be an error. In conclusion the Professor said that there was no doubt that the effect of the senna was impaired by the presence of argol in such quantities as he had found it, and it was the duty of Pharmacutists to set their faces against such adulterations. He, therefore, urged the Society again to exert its influence to stop a gross and systematic fraud.

Mr. WAUGH inquired whether argol grew in the same neighbourhood as senna, because in that case the admixture might be accidental.

Professor BENTLEY said it grew on the spot, but there could be no doubt the adulteration was intentional. Sometimes the senna crop was short while that of the Cynanchum, or argol, was good, and it was used to make up the deficiency of senna.

Mr. SQUIRE said it required more experiments to prove the purgative effect of the Cynanchum. We should be careful how statements relative to the action of medicines were stereotyped. It was a common opinion that the resin of aloes was the cause of the griping effect of that medicine; but experiments had been made with the resin carefully separated, and it had been found to have hardly any action.

Professor BENTLEY said that argol leaves had been tried by several experimenters with various and contradictory results, the action of them, however, was not the question before the meeting.

Mr. EVANS thought that the adulteration of senna with Cynanchum was not so common now as some years ago. One thing might account for the difference in the effects ascribed to argol. The leaves were commonly covered with a brown resin which might interfere with the solvent action of the water in making an infusion. A tincture would perhaps be more active.

Mr. GALE said the samples exhibited had been collected quite recently, and the one with 26 per cent of argol had been sold as picked senna. In 1851 he had himself tried to get a specimen of the Cynanchum and could not find any.

The CHAIRMAN remarked on the absence of the senna pods in the samples. They were formerly to be found in considerable quantities, but now they were almost entirely absent from the senna used in this country. The fact was these pods or follicles were largely employed on the Continent where their action and remedial effects are considered superior to the leaves. He thought it was desirable that they should be tried in this country.

Professor BENTLEY said he had found the legumes absent from the sennas he had examined for the last four or five years. He had seen some in the docks and had been told that they went to the continent. The action of the legumes was not so powerful as the leaves, and the griping effect was totally absent.

Mr. EVANS observed that the quality of Tinnevely senna had much deteriorated. The fine leaves could not now be obtained. He inquired the relative medicinal value of Alexandrian and Tinnevely senna.

The CHAIRMAN said that the testimony of the medical profession was in favour of the Alexandrian.

Professor BENTLEY then called the attention of the meeting to a model of the ripe fruit of the *Indian Bael*, and some preparations of it which were placed on the table.

Dr. EVANS explained that the *Dietetic Bael* was made by

mixing the pulp of the fruit with arrowroot, and then drying it in the sun, spread out on plantain leaves.

A discussion followed, which was mainly devoted to the question whether Bael had any remedial effects,—at all events, in this country.

Professor REDWOOD exhibited some specimens of *Spermaceti Ointment*, which had been prepared by Mr. Hills, one with bleached, another with unbleached olive oil, and a third with almond oil. They had been made three months, and the specimen made with bleached oil was now quite rancid, while the others were perfectly sweet. That made with almond oil was of an excellent colour, and Mr. Hills considered that this was the best oil to be used in the preparation.

A short Paper followed "*On the Iodide of Potassium and Compound Iodine Ointments*," by Mr. H. HALL. The author proposes to avoid the objectionable grittiness which is always present when these ointments are too quickly or carelessly prepared by first dissolving the iodide of potassium and iodine with a small quantity of glycerine, and mixing the solution with the lard. The formulas he suggests are the following:—

Iodide of Potassium Ointment.

Iodide of potassium	2 drachms.
Glycerine	4 "
Lard	1½ ounces.

Rub the iodide to a powder, add the glycerine, and, when the salt is dissolved, mix with the lard.

Compound Iodine Ointment.

Iodine	½ drachm.
Iodide of potassium	1 "
Glycerine	4 "
Lard	1½ ounces.

Rub the iodide of potassium to powder, then add the iodine and afterwards the glycerine, and, when dissolved, mix with the lard.

In this way, the author says, the ointments may be made perfectly smooth, and in a very short time. When prepared in quantity labour may be saved by heating the lard to a creamy consistency, and stirring in the solutions.

Mr. SQUIRE said that too much reliance should not be placed on glycerine preparations; they did not keep well. The yellow colour of the iodide of potassium ointment on the table was also objectionable.

The CHAIRMAN thought the ointments good, and the colour not objectionable. The glycerine certainly would not favour crystallization of the iodide.

MANCHESTER
LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, February 19, 1861.

Dr. J. P. JOULE, President, in the Chair.

A Paper was read by J. C. DYER, Esq., entitled, "*Brief Notes on the Freezing, Thawing, and Evaporation of Water, and on the Condensation of Steam, with a View to Enquire into the Cause of those Changes.*"

In these Notes the Author merely aimed to place before the Society the apparent agency of heat, in the changes that water undergoes in passing alternately from one to the other of its conditions of ice, water, and steam, and *vice versa*; and that these mutations are caused, by the taking up and giving out of heat, in its sensible or latent state, by the transitions reciprocally from one to the other of those states. The actual amount of the thermometric heat passing from the latent to the sensible state was given, as taken from the common tables. On referring to the two ancient theories of heat, the one defining it to be "a material element, *sui generis*, and pervading matter," &c.; the other holding it to be "no other than the

motions, mechanically excited in the minute particles of bodies," &c.—it was contended that, by this latter or the "force heat theory," the melting of ice by the action of the sun's rays could not be explained, since it is not by their force, but by the matter of heat that enters and becomes latent in the water. The Author then submitted that the only solution, hitherto offered of the absorption of sensible heat, in water and steam, as latent in these, and the re-appearance of the same measure of heat, in a sensible state, by the acts of condensation and freezing, is to be found in the application of Dr. Black's "Latent Heat Theory." Considering the force heat theory, as directly conflicting with that of latent heat, they cannot both be sustained; and as the latter stands in elementary works as an established law in physics, and as it affords a clear explanation of the aqueous changes adduced; it seems incumbent on those who deny the entity of heat, to account for the alternate exhibition and extinction of the thermometric heat, that is, in fact, evolved and absorbed by those mutations of water.

The Author again referring to the long standing controversies concerning the essential nature of heat, stated that more than one hundred works have been published on the subject during the last 200 years, and yet no conclusion has been arrived at, as to the soundness of either of the original theories above cited, nor have any discoveries been made that explain the agency of heat, in the mutations of water, since the days of Bacon and Boyle, with the sole exception of those of Dr. Black, which appear to prove its latent state in bodies. And since we have no settled doctrine as to its essence, we must allow that the subject is of philosophical interest, and especially this branch of it concerning latent heat, in defence of which these notes are offered.

The PRESIDENT said that some very interesting experiments made by Mr. Dyer, many years ago, were in favour of the dynamical theory of heat, which he believed to be fully able to explain the phenomena ascribed by Black to "latent heat."

A Paper was read by Dr. R. ANGUS SMITH, "*On the Production and Prevention of Malaria.*"

The Author did not pretend to enter on the whole subject, but to give a few observations which he considered fitted for its illustration.—Malaria has unquestionably been proved to be caused by the decomposition of organised bodies. If so, it must exist to some extent everywhere. By the mode of testing the air invented by the Author, every place tried at home and abroad was found to have some oxidizable matter in it, although in some this was extremely small. In such cases the matter was probably oxidized to a state in which it would be innocuous. This oxidizable matter no doubt rises, in a great measure from vegetation. Vegetation does not merely grow; it dies. This death may be caused by various circumstances, but two conditions are remarkable, one where the agents are animals, and the other where the agents are chemical. Animal life may act in various amounts on vegetation in the soil, from the large vermin to the microscopic classes. These do not prevent chemical action, on the contrary, it is probable that they further it exceedingly. Decomposition goes on in the soil at various rates, and in various ways. In a rich, highly-manured soil kept warm, the soil will be found alkaline. Soils generally are acid. The Author had shown in a Paper, read in 1847, that in an alkaline, peaty district, cold weather produced acidity in a few days. It would appear as if the acids of the mould (so elaborately described by Mulder) were incapable of further decomposition in the cold, and were thus retained and increased. Our great struggle with the soil is to produce alkalinity, or at least to diminish acidity and where most acids exist we use most lime. Where most alkali exists there is a greater facility for the escape of vapours, such as we suppose to be hurtful. So far as the vapours of putrid

substances have been examined by the Author, they have shown indications of containing substances composed somewhat like protein, at least the carbon and nitrogen have had relations to each other similar, or nearly identical with those found in protein, and formed the mass of the substance.

The extreme condition of putrescence may be very readily produced in a soil by artificial means; the use of a little ammonia, for example, more than vegetation will bear. The substances putrefy until the whole becomes fetid in the highest degree. We have then a soil rich in organic matter and undrained. It is a swamp of the worst form if the soil be not very poor; worse, perhaps, than was ever seen in nature. Such a soil would bring death everywhere. It is artificial malaria. We can, then, produce malaria from the soil by fostering some of its tendencies; and we see by the rapid acidification of soil, in colder weather, why malaria is diminished by a lower temperature.

As we can imitate malaria of some kinds, so can we also imitate the methods by which Nature prevents it. The warm alkaline soil, moistened, and washed with air and water, becomes acid, it sends forth less volatile matter—decomposition is stopped to a great extent, the matter is preserved. Cold prevents the action, drainage assists oxidation by a more active state of soil. By these modes and others, the soil is disinfected by Nature, when these do not act sufficiently, we may use disinfecting agents. By their means decomposition may be interrupted without fear of diminishing the power of the plant to take up food. By the use of disinfecting agents, Mr. McDougall had been able to feed sheep and cattle, and retain them in health, on meadows constantly wet by irrigation with liquid manure. The disinfectant used is from the products of the distillation of tar, the amount required is small. Animal life is rapidly destroyed by it, and chemical decomposition is stayed. All climates can furnish this, where coal lies or where trees grow.

It would be possible to irrigate great districts at a very small expense. The result would be as certain on a large scale as on a small one; and it is probable that, in some cases, one or two applications would be sufficient, for a long period at least. By the new state of things, destructive insects would also be destroyed. Although, according to Dr. McCulloch, there are many parts of our own islands infected by malaria, most, if not all, can be cured by good attention to well-known agricultural maxims. This new method is especially applicable to other countries, and to more violent stages of the disease. The author hopes to have it tried on extensive districts in Italy. The method arose out of an advice everywhere neglected, but still cherished as true, to disinfect whole cities by beginning with the sewers, the origin and reservoir of all the mischief.

The Author believes that he has shown that decomposition, to a most pernicious extent, is possible in soils, that this is not a mere opinion, but a fact readily demonstrated; but that decomposition may be arrested artificially to the preservation of health without the destruction of vegetation, and that in these facts we have not only a surer basis in our reasonings on the origin of malaria, but an almost certain process for its ultimate and total extermination.

Ordinary Meeting, March 5, 1861.

Dr. J. P. JOULE, President, in the Chair.

Mr. JOSEPH SIDENOTHAM read a Paper "*On the Structure of the Luminous Envelope of the Sun*," being a communication to him from James Nasmyth, Esq., of Penshurst. Mr. Nasmyth has made the discovery, that the entire surface of the sun is composed of objects of the shape of a willow leaf. These objects average about 1000 miles in length and 100 in breadth, and cross each other in all

directions, forming a network. The thickness of this does not appear to be very great, as through the interstices the dark or penumbral stratum is seen, and it is this which gives to the sun that peculiar mottled appearance so familiar to observers. These willow leaf-shaped objects are best seen at the edges of a solar spot, where they appear luminous, on a dark ground, and also compose the bridges which are formed across a spot when it is mending up; the only approach to symmetrical arrangement is in the filaments bordering the spot, and those composing the penumbra, which appears to be a true secondary stratum of the sun's luminous atmosphere. Here these bodies show a tendency to a radial arrangement. Although carefully watched for, no trace of a spiral or vortical arrangement has been observed in these filaments, thus setting aside the likelihood of any whirlwind-like action being an agent in the formation of the spots, as has been conjectured to be the case. The writer does not feel warranted at present in hazarding any conjectures as to the nature and functions of these remarkable willow leaf-shaped objects, but intends pursuing the investigation of the subject this summer, and hopes to lay the results before the British Association during their meeting in this city. The paper was illustrated by three beautiful drawings. No. 1 represented one of the willow leaf-shaped objects; No. 2 the luminous surface of the sun as being entirely composed of these objects; and No. 3, a large drawing of a solar spot as seen on the 20th July 1860, exhibiting the surface of the sun composed of these objects, as also the penumbra and the bridges across the dark portion of the spot in which the exact shapes of these objects were to be seen most clearly. Mr. Sidebotham stated that the image of the sun was examined by Mr. Nasmyth with a mirror of plane glass, set at an angle of 45 degrees; nearly the whole of the light and heat of the sun passed through the glass, and the rays used were those only reflected from its surface.

Mr. CHARLES O'NEILL read a Paper "*On Changes of Density which take place in Rolled Copper by Hammering and Annealing*." The results of his experiments proved that the best commercial rolled copper actually lost density by hammering instead of gaining as might have been anticipated. In the first series of experiments, ten pieces of copper were cut from a sheet of the thickness of $\frac{1}{8}$ inch, the pieces weighed from 250 to 320 grains each, their mean density was 8.879. The pieces were then separately subjected to the action of a powerful compressing machine, acting on the principle of the *genou*, about fifty blows being given. The density of these hammered pieces showed a mean of 8.855, being a loss of 0.024. The same pieces were annealed by being placed in red hot sand, and cooled slowly; when cleared from adhering oxide, the mean density was found to be 8.834, being an increase of 0.029 on the hammered pieces, and 0.005 on the original pieces. A second series of experiments, made with very great care, corroborated the first in the main points. The pieces were from another and better sheet of copper; ten pieces, weighing each from 420 to 520 grains, showed a mean density of 8.898, being hammered by the same machine; their mean density became 8.878, showing a loss of 0.020 by hammering; upon annealing in a charcoal fire, the mean density of five out of the ten pieces was 8.896, showing a gain of 0.018 upon the hammered pieces, and a loss of 0.002 upon the original. A third series of experiments upon the change of density in a bar of copper by successive hammerings showed a loss of density from 8.835 to 8.867. The author considered there was a connection between these phenomena and the heat disengaged in the hammering of the copper. He conceived it possible that the expanded state of the copper while heated by hammering was retained, and that the effect of annealing might be to allow the molecules or particles to recover the state in which they were in before being disturbed by the heat produced in hammering.

CORRESPONDENCE.

M. Du Motay's Blue Dye.

To the Editor of the *CHEMICAL NEWS*.

SIR,—A recent number of the *CHEMICAL NEWS* contains a notice of a blue colour patented by a M. Du Motay, of Paris. This gentleman makes a mixture, or, in his opinion, a combination of soluble Prussian blue and carmine of indigo, which he recommends for dyeing, and asserts that the blues, and consequently the greens, prepared in this manner retain their tone by artificial light.

Now it has been found, after many attempts, that soluble Prussian blue, even of a quality much superior to that specified by Du Motay, gives a dye very deficient in beauty and lustre. Again, dyers and printers have long been in the habit of using Prussian blue formed upon the fibre along with carmine of indigo, or with certain red colouring matters, to correct its greenish hue and to give depth, body, and softness. In so doing they have been in advance of M. Du Motay, since a Prussian blue formed upon the fibre is brighter and much cheaper than any dye produced by soluble Prussian blue.

Ordinary Prussian blues "topped" with indigo retain their tone by artificial light quite as well, if not better, than the colours of Du Motay's patent.

His assertion that the Prussian blue and extract of indigo form a true combination may be correct, but chemists will require very clear evidence before admitting a reaction so improbable. Had he been familiar with extract of indigo he would have laid little weight on the circumstance that his compound is more soluble in lukewarm than in boiling water.

Practical men, in short, cannot help seeing that M. Du Motay has merely invented and patented an imperfect and costly method of getting results which the trade already produce in a far better manner.—We are, &c.,

ROBINSON AND SLATER.

Huddersfield.

On the Injurious Effects of Arsenical Pigments.

To the Editor of the *CHEMICAL NEWS*.

SIR,—In the last number of the *CHEMICAL NEWS* (for March 9) I find a short abstract of a communication of mine "On the Injurious Effects of Arsenical Pigments," which I read before the Royal Dublin Society at its last Scientific Evening Meeting; as, however, the writer of the article appears to have misunderstood several of the statements which I made in my communication, permit me now to correct a few of the more important mistakes appearing in that abstract.

It is asserted that I said that the subject of "The Injurious Effects of Arsenical Pigments" "had never been touched on in Ireland, except by Dr. Apjohn," whereas, I stated that I believed that gentleman was the first to bring the matter prominently before the public in this country by a letter of his which appeared in the *Farmer's Gazette*.

Again, it is stated that I "quoted the cases of two children who lately died in London from arsenical poisoning, supposed to be contracted by sucking toys which been covered with green paint." The case which the writer alludes to, is that described by Dr. Metcalfe, published in the *Lancet* of December 1, 1860; but though two children were attacked with the symptoms of arsenical poisoning, only one of them died, and the effects were attributed to the arsenical dust from the green paper which covered the walls of the room the children were playing in, as well as the interior of the cupboard they were assisting to clear out, and not from toys painted with arsenical pigment. Again, it is stated that in the cases I noticed of those who had suffered from the injurious effects

of arsenical pigments in room-decoration, viz., those which occurred in one of the wards of the Adelaide Hospital in Dublin, and of the Librarian of the Cork College, that "inflammation of the eyes, and ulceration about the nose and mouth, appeared to be the most prominent symptoms;" these, though they are the usual effects which are observed in those who are employed in the manufacture and application of arsenical pigments, were not the symptoms (with the exceptions of the inflammatory condition of the eyes) which I described as occurring in those and other cases, which I mentioned where injurious effects were experienced by those inhabiting rooms covered with arsenical paper hangings. Again, on referring to my having detected arsenic in different articles of ladies' dress, &c., it is stated that from "a piece of green tarlatan, nine inches square, I had extracted twenty grains of arsenite of copper." This, indeed, would be an incredibly large quantity. What I stated, however, was that from a piece of green tarlatan, nine inches square, and weighing about twenty grains, I had obtained, by mere washing in cold water, five grains of green colouring matter, which consisted almost entirely of arsenite of copper. Finally, the article states that I asserted "that all green wall-papers contained arsenite of copper, whether dear or cheap, French or English, dark or light, sized or unsized." Here again the writer has misunderstood me, for all I stated was that every specimen of green paper of the above descriptions which I had examined, I found contained arsenic; but there may be some green room-papers where the colouring matter is not arsenical.

Hoping that you will be so good as to insert the above remarks in your next number of the *CHEMICAL NEWS*,—

I am, &c. EDMUND WM. DAVY.

Laboratory of the Royal Dublin Society, March 12.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Carbonic Acid in the Soil.—Van den Broek says (*Annalen der Chem. und Pharm.*, Bd. cxv. s. 87) that a solution of carbonic acid percolating through the soil, is, up to a certain limit, robbed of its carbonic acid, so that the filtrate no longer causes any turbidity with lime-water; and, if a stream of hydrogen gas be passed through a layer of earth, the carbonic acid can be displaced. The author lays stress on this property of the soil holding carbonic acid, as supporting Liebig's views on the subject of the nutrition of plants.

The Yttrio-tantalum and Yttrio-niobium Minerals.—Berzelius noticed three different varieties of yttrio-tantalum minerals—the black, yellow, and dark yttrio-tantalite. Professor Nordenskjöld has since found (*Journ. für prakt. Chemie*, Bd. lxxxi. s. 193) that these minerals exist in, at least, two different crystalline forms, the rhombic and the tetragonal. The rhombic, which is the same as Berzelius' black, contains tantalic acid, while the tetragonal or Berzelius' dark, contains niobic acid, and corresponds to Greenland Fergusonite. The compounds of yttria with tantalic and niobic acids at present known in Sweden have been divided into three classes. Yttrio-tantalite, Fergusonite, and Hjelmite. The first is found sometimes as a pure black crystalline mass, with a slightly metallic lustre, and sometimes as a straw-coloured amorphous powder. Fergusonite is found crystalline, and of a dark brown colour. The crystals are always indistinct, short four-sided prisms, transparent at the edges. Hjelmite is a pure black mineral with no shade of brown. The author could only discover in it indistinct traces of crystalline structure.

Solubility of Sulphate of Ammonia in Water.—After many determinations, A. Vogel has decided (*Buchner's N. Report*, Bd. x. s. 9) that one part of

sulphate of ammonia is soluble at the ordinary temperature in 1½ parts of water.

II. ORGANIC CHEMISTRY.

The Nitrile of Leucinic Acid.—O. Hesse and H. Limpricht state (*Annalen der Chem. und Pharm.* Bd. cxi. s. 201) that the so-called leucinic acid nitrile, $C_{12}H_{11}NO_2$, may be easily separated, but only in small quantity, from the rough tyrosin made by boiling horn with sulphuric acid. The tyrosin must be treated with boiling spirit, the alcoholic extract evaporated, and the resulting deposit purified by repeated crystallisations from hot absolute alcohol. So obtained, the body forms long white silky crystalline tufts of needles, which sublime unchanged between 173° and $180^\circ C.$, are not very soluble in ether, but quickly so in strong alcohol, especially in the warm, and hardly soluble in cold water, but more so in hot.

III. ANALYTICAL CHEMISTRY.

Estimation of Soap.—Dr. Buchner gives (*Polym. technisch Centralblatt*, 1860, s. 1484) a method by which the amount of hard soap in a specimen can be calculated from the amount of fatty acid obtained when a given amount of the specimen is decomposed by a strong acid. The author makes use of a flask, the neck of which is graduated into cubic centimetres; into this flask, half full of water, he puts half-an-ounce of soap and dissolves it. He then adds the acid, either commercial hydrochloric, or dilute sulphuric acid, and warms the mixture, whereupon the fatty acids are set free. He now puts sufficient water to allow him to read off the number of cubic centimetres the acids measure in the neck of the flask. The fatty acids from different sources differ a little in weight; but the author found that the average weight of a cubic centimetre is .93 gramme, which is near enough to the truth for ordinary practical purposes. As the acids are combined with $\frac{1}{2}$ glycerine, it is easy, knowing the weight of the acids, to calculate the weight of the fat used; and as on the average 100 lbs. of fat give 155 lbs. of good hard soap, the weight of the real soap can be calculated when the weight of the fat is known. These calculations may be made by the use of a table which the author has constructed, from which we extract the important parts. The results are not to be considered scientifically accurate, but are near enough to the truth for ordinary business purposes. The process requires only one weighing, is executed in a few minutes, and is so simple that it can be performed by a common workman.

I. Cubic centimetres of fat acids separated from half-an-ounce of soap.

II. Per-centage of water, ley, glycerine, &c., in the specimen.

III. Per-centage of good hard soap.

I.	II.	III.
½	97	3
1	69	31
6	63	37
7	57	43
8	51	49
9	44	56
10	38	62
11	32	68
12	26	74
13	20	80
14	13	87
15	7	93

IV. TECHNICAL CHEMISTRY.

Application of Hypochlorite of Zinc.—Balard has shown that when hypochlorite of lime and sulphate of zinc are mixed together, sulphate of lime and oxide of zinc are precipitated, and a solution of hypochlorous acid remains. Sacc has availed himself of this

reaction in the bleaching of stuffs dyed with madder. He prints a cloth, which has been dyed red, with a mixture of 4 parts of sulphate of zinc, 5 parts of gum, and 10 part of water, and when dry, he passes it through a cold bath of chloride of lime of 2 degrees Baumé. The printed parts are immediately bleached, and the remaining red takes an increased brilliancy of colour. This energetic bleaching power depends upon the ready decomposibility of the hypochlorite of zinc and the consequent setting free of hypochlorous acid. This process, the author says, has many advantages.—Dr. Varrentrapp in *Mitth. für d. Gewerbever der Herzogth. Braunschweig*, 1860, s. 24.

MISCELLANEOUS.

Seleniferous Deposit from the Sulphuric Acid Chambers.—Ores of Tellurium.—Any gentleman who may be able to place the Editor in possession of some of the above, by purchase or otherwise, will confer a great favour by communicating particulars to Mr. Crookes at the Office of this paper.

Chemical Society.—The next meeting of this Society will be held on Thursday next, the 21st inst., when a lecture will be delivered "On Thermo-Dynamics in Relation to Chemical Affinity," by A. W. Williamson, Ph.D., F.R.S.

Royal Institution.—The following is the arrangement of Lectures for the ensuing week:—Monday, March 18, at 9 o'clock, M. P. Chailu "On Personal Narrative of Travels in Central Africa." Tuesday, March 19, at 3 o'clock, Professor Owen "On Fishes." Thursday, March 21, at 3 o'clock, Professor Tyndall "On Electricity." Friday, March 22, at 8 o'clock, Professor H. D. Rogers "On the Origin of the Parallel Roads of Glen Roy." Saturday, March 23, at 3 o'clock, Dr. E. Frankland "On Inorganic Chemistry."

ANSWERS TO CORRESPONDENTS.

*. All Editorial Communications are to be addressed to Mr. CROOKES, and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

A Letter from "Verax," on the Adulteration of Food, has been received, but too late to be inserted this week.

Communications declined with thanks.—G. F. R., J. A. Davies, and C. L.

M. A. B.'s request shall meet with consideration, and, if possible, be acceded to.

C. O. M. Wallace.—Apply to the Editor of one of the Photographic periodicals.

A Constant Reader.—The assertion respecting the Electric Light is quite correct. It is now used in lighting up one of the squares in Paris, and is cheaper than gas for that purpose.

H. K. S.—You had better purchase the Magenta dye. It can be got for a much less sum than you could make it for small quantities, even supposing you to be successful in the attempt. The best mode of its preparation is a trade secret.

A. Ashbey.—"Faraday's Chemical Manipulations" has long been out of print. A copy may occasionally be picked up at a second-hand bookseller's, but it always fetches a good price.

A Metallurgist.—We have not space to answer all your questions Dr. Percy's book on metallurgy is expected to be out shortly and will contain full information on all these points. There are also several good works on metallurgy which would give you an account of what you require.

Silver in Sea-Water.—This occurs in an excessively small proportion. In large bulks of the water, however, it becomes an important quantity; for instance, it has been calculated that a cubic mile of the water of the Atlantic contains nearly fifty tons' weight of silver dissolved in it in the form of chloride. Whether it can, or will, ever be economically extracted from it, we will not venture to say. The copper bottoms of ships, after long voyages, are found to contain more silver than the metal did originally; but the copper is dissolved in a larger proportion than the silver is precipitated on it.

THE CHEMICAL NEWS.

VOL. III. No. 68.—March 23, 1861.

ON THE METHODS PROPOSED FOR PREVENTING THE FURTHER DECAY OF THE HOUSES OF PARLIAMENT.

SINCE our last article touching on this subject, a tendency to reaction appears to have taken possession of the public. The narrow escape of policeman X. 1999—who, forsaking the ordinary beaten track of his professional brethren, nearly fell a victim to the brittleness of amorphous dolomite—has ceased to impress us. But, although it is certain that the decay has been exaggerated, we are not to suppose that the building is sound. It is, moreover, evident that the authorities are fully resolved to take some steps to remedy the existing evil, be its magnitude less or more than is now generally believed.

It is a melancholy thing for the honour of Chemistry, as a science, that, no matter how simple the matter upon which we may attempt to take the sense of its Professors, they never come to anything approaching to a unanimous verdict; and this, to a great extent, arises from the circumstance that there is no science of which it is so easy to acquire a smattering, or of which it is so difficult to acquire a mastery, as Chemistry. It is also notorious that shocking discredit is beginning to attach itself to numerous teachers, who disgrace themselves and the science they endeavour to identify themselves with, by dubbing themselves "Professors," without any just claim to the title. These men have generally some pet process, to the success of which they devote themselves, and happy are they if a "Company" can be found to back them with funds to carry out their chimerical ideas. Let us hope that the Commission appointed to consider the best method of preserving the Houses of Parliament will steer clear of any plans put forward by such people, and carefully sift the claims of the inventors who may offer their assistance. There are, among the names already before the public in connection with this matter, several men of worth and talent, whose processes deserve the fullest trial and investigation; and there are others whose plans are obviously absurd, and, therefore, may without difficulty be thrown aside.

Among the inventors before the public in connection with stone-hardening, there are some whose only fault has been a too sanguine belief in their own powers. Let us, therefore, in a matter of such moment, pause before deciding, and endeavouring, as far as the weakness of human nature will permit, to throw aside our preconceived ideas, carefully consider how far any of the proposed plans fulfil the demands upon them.

It has been urged by one of the most eminent Chemists in this country that any organic preparation must of necessity fail, owing to its perishability.¹ There is an element of weakness in this argument, owing to the

fact that no composition can ever be arrived at which will *never* require renewal. No substance, we fear, can be applied to the stone *in situ* which shall penetrate entirely through, and render it as imperishable as the hardest and best material known to builders. The problem, if looked at from this point of view, becomes simplified, because we are only required to select that preparation which shall require the least amount of renewal. This element (renewal) enters as an important item in the cost, inasmuch as it is evident that the mere manual labour of covering so vast a pile will entail an immense expense, to say nothing of the extreme annoyance to the eye of perpetually seeing scaffoldings erected and platforms suspended, breaking every line of beauty, and hiding the lavish ornament upon which such labour and money has almost unavailingly been spent.

In a recent Number we took occasion to animadvert somewhat strongly upon a pamphlet published by M. Szerelmey. In that work the author states that his processes are a revival of those known to the Ancients. He, however, not only offers no real evidence of his methods being even founded upon the ancient ones, but the entire work is a mere vague, rambling treatise on the advantages of the preservative processes of the Ancients, clothed in language affecting to be scientific, but betraying, in numerous instances, an extremely feeble acquaintance with the science which, of all others, is most essential to the elucidation of an ancient, or the construction of a modern method. We think it, however, but just to M. Szerelmey to say, that, however unfavourable an impression his pamphlet and his secrecy may have made upon us, his process has obtained a very favourable verdict from a philosopher whose judgment all scientific men respect,—we allude to our great countryman, Professor Faraday. At a meeting of the Royal Institute of British Architects, held a few weeks back, Mr. Edward M. Barry, in the course of a very manly and straightforward speech, read a correspondence between Mr. Faraday and the First Commissioner of Works. From the reading of these letters it transpired that Mr. Faraday, after examining the work performed upon a certain portion of the Houses of Parliament by Mr. Ransome, and another portion by M. Szerelmey, came to the conclusion that the method of the latter gentleman would prove the best.

Mr. Faraday was requested to inform the First Commissioner whether M. Szerelmey used oleaginous or other organic matter; we have not been able to find whether any or what answer was returned to this important question. After reading his pamphlet we fancied we had a misty idea that he did employ some greasy or oleaginous substance, but it is scarcely necessary to say that we could get no definite information from a work written with the very object of preventing a conclusion from being arrived at.

Dr. Hofmann has expressed his opinions upon the subject freely, and his statements are always entitled to respect. He, like everyone else who has no pet process

¹ The preservative constituent of silicic ether being, of course, silicic acid, it becomes an exception. The Chemist alluded to meant where the preservative constituent itself was an organic compound.

to push forward, does not attempt to cut the knot, but candidly confesses the numerous difficulties which surround the matter. It is somewhat amusing to observe how his devotion to organic chemistry shows itself in his advocacy of silicic ether as a substance adapted for the purpose of hardening the surface of the stone. It would be easy to draw a parallel between the distinguished Professor's advocacy of silicic ether for defending the Houses of Parliament, and the currier's (in the fable) advocacy of leather for defending the city; but, however ridiculous it may appear to talk of employing a re-agent which only one chemist has as yet been bold enough to offer to manufacture in quantity, and even then at a price of £6 per pound, it is, however, certain that no substance appears to contain within itself so many elements of success. The price, which at present renders its use hopeless, will doubtless one day be no objection. Let those who have received Dr. Hofmann's idea with unsparing ridicule remember that, with chemicals, the rarest to-day is sometimes a drug on the morrow.

Professor Tennant, whom the Chairman (Mr. Digby Wyatt) introduced to the meeting previously alluded to as "one of the greatest mineralogists in this country," addressed the audience in a speech from which we have been unable to extract anything of which the least use could be made, or from which anything could be taken. It is true that he drew a parallel between the condition of the Druids and the Moderns as regards the difference in their respective facilities for carrying stone, which parallel must be very gratifying to every lover of progress, and that he "referred to the compact sandstones of the coal measures, compact limestone of the mountain limestone, syenite of Mount Sorrel, near Leicester, and the stone on the Malvern Hills;" but, unless it be the rebuilding the Parliament Houses with one or more of the stones alluded to by the Professor, we do not see that anything is to be gathered from his speech; certainly, nothing as to the object before the meeting.

As regards the experiments yet made, it appears that the following results have been obtained. We get our information from Mr. Scott's report on the experiments made on rapidly-decaying stone in Westminster Abbey.

1. *Water-glass*.—Tried in 1857 and 1858. Stone hardened, but decay only partially arrested.
2. *Paul's Aluminate of Potash*.—Tried same date Same result as No. 1.
3. *Ransome's Silicate of Lime Process*.—Same result.
4. *Szerelmey's Secret Process*.—Stone much hardened. Decay only slightly visible.
5. *Soap and Alum Process*.—Effects appear to have ceased.
6. *Rocha's Silicate*.—Effects remain, but efficiency not reported on.
7. *Shellac in Spirits of Wine*.—Admirably successful where protected from rain. Scarcely so successful where exposed to rain.
8. *White Wax in Turpentine*.—Failure.
9. Same as No. 8, but with addition of Stearine.—Better, but failing.
10. *Daine's Oil and Sulphur Process*.—Tried since July, 1859. Almost entirely successful.

We submit that the above results are most encouraging. When, in a matter of such import, 30 per cent. of the methods tried are almost complete successes, every hope may be entertained that soon some processes may be discovered quite equal to the demands upon them.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Extreme Difficulty of Removing the Last Traces of Carbonic Acid from Large Quantities of Air, by Messrs. C. W. ELIOT and FRANK H. STORER.

IN the course of our research upon the impurities of zinc, we instituted a series of experiments, in order to ascertain whether the disagreeable odour of hydrogen gas, as generated from common zinc by means of sulphuric acid, could be attributed to the presence of any gaseous compound of carbon,—an apocryphal doctrine which seems to be quite generally credited.

The results of these experiments were entirely negative, in so far as they related to the point in question, and we should not have thought of publishing them had it not been proved to us, by a memoir recently printed in *Poggendorff's Annalen*, that one of the phenomena which we then observed had not been sufficiently dwelt upon by chemists.

In our experiments above referred to, a jet of hydrogen was burned in a glass globe, 30 centimètres in diameter, through which was drawn, by means of an aspirator, a steady current of air. Before entering the globe, this air had passed through an apparatus, described in full below, which was intended to deprive it of all its carbonic acid, and on leaving the globe it was drawn through a bottle containing lime-water, carrying with it, of course, the products of the combustion of the hydrogen. We first used, in order to absorb the carbonic acid completely from the air in which the hydrogen burned, two cylinders, 27 centimètres high and 5 centimètres in diameter, filled with fragments of pumice-stone, moistened with caustic potash; not satisfied with this large absorption surface, we next added to the cylinders two Wolfe bottles, containing a concentrated solution of caustic potash, and finally substituted for the Wolfe bottles three five-bulb potash-tubes. We repeatedly maintained a burning jet of hydrogen in the globe for periods varying from four to six hours, with air purified by passing through the three potash-tubes and two cylinders described, and always obtained the same result,—viz., there was never any perceptible cloud of carbonate of lime in the bottle containing lime-water during the progress of the experiment; but after standing twelve hours, an unmistakable deposit of crystalline carbonate of lime was invariably found at the bottom of the lime-water. We might have regarded this as sufficient evidence of the presence of an infinitesimal amount of carbon in the hydrogen, had we not found, by repeated trials, that the burning of the hydrogen had no influence whatever on the formation of this crystalline deposit. 156,000 cubic centimètres of air (the contents of our aspirator), passed through the purifiers we have described, still retained sufficient carbonic acid to produce a deposit of crystalline carbonate of lime, when allowed time to separate from the lime-water by crystallisation.¹ In

¹ The fact that carbonate of lime may at first be dissolved by lime-water, has been clearly shown by Vogel (*Schweigger's Journ. f. Ch. u. Phys.*, 1821, xxxiii. 207). It is moreover very distinctly affirmed in the following passage from an article which has recently fallen under our notice, by Berthollet (*Annales de Chimie*, 1789, lii. 68):—"I am indebted to M. Welter for an observation upon the use of lime-water, which may be useful in cases where one wishes to detect small quantities of carbonic acid. Lime-water has the property of dissolving a little carbonate of lime, of which one can assure himself by blowing into it with a tube; the air expired produces a cloud which re-dissolves entirely, until the lime-water is saturated with the car-

order to render the experiment irreproachable, with reference to the determination of carbon in the hydrogen, we should have been obliged to extend indefinitely our absorbing apparatus, and to force the air through the whole apparatus, instead of sucking it through, as in the experiments above described. The object in view was not worth such trouble and expense, and we, moreover, had not the necessary appliances, so that we were reluctantly forced to give up the inquiry.

Our interest in the subject has been awakened anew by some experiments recently published by H. Karsten,² upon the oxidation of dry non-nitrogenous organic substances by the action of atmospheric air at ordinary temperatures. The method upon which Karsten chiefly relied in these experiments was, in its essential features, identical with the one employed by ourselves, but the difficulty to which we have referred, of removing carbonic acid from the air employed by any common absorption apparatus, is altogether ignored by him,³ as will appear from the following description of his apparatus, quoted from page 349 of his article:—"In order to purify from carbonic acid and water the air which I allowed to flow in a slow, continuous stream over the organic substances, I placed before the vessel which contained them chloride of calcium tubes several feet in length, and in front of these a tube containing dry caustic potash, preceded by a bulb-tube filled with concentrated solution of caustic potash; by this solution the air was first washed and freed from carbonic acid; it was then led slowly over the dry caustic potash and through the long chloride of calcium tubes, before it came in contact with the organic substances, which had been dried in the water-bath." It is evident, at all events, that this apparatus was far less adequate than our own for the difficult operation of removing carbonic acid from the air.

We do not in the least seek to deny the truth of Karsten's assertion, that carbonic acid is really formed by the action of air at ordinary temperatures upon the substances in question. The fact is not only probable *a priori*, but would seem to be proved by his incidental statement (p. 348), that carbonic acid was formed when these compounds (sugar, cork, &c.) were exposed during some months to the action of air or oxygen in tubes sealed with mercury in the pneumatic trough. In so far as charcoal is concerned, De Saussure⁴ has long ago shown the extreme probability that it is oxidised by the air, even when dry, as it is when wet. Neither do we wish to assert that it is impossible to deprive air of every atom of its carbonic acid. We insist only upon the facts, that it is a matter of no inconsiderable difficulty to do this, that Karsten's apparatus was entirely inadequate, and that nothing in his paper would indicate that he has allowed for this source of error.

It should be distinctly borne in mind that in the experiments of Karsten, as well as in our own, the question raised is not at all whether the amount of car-

bonic acid which escapes absorption can be estimated with the balance; for so long as the experiments are qualitative only, and conclusions are based upon the precipitate which is formed in lime-water, it is clearly necessary to remove every trace of carbonic acid from the air employed, no matter how "imponderable" this trace may be. We do not believe that the carbonic acid which escapes absorption in ordinary experiments can be of sufficient amount to be mentioned as a source of inaccuracy in the determination of the carbonic acid of the air, by the method which has been used by so many eminent chemists; for the extent of the error thus introduced must be far less than that of several others to which the absorption process, as commonly employed, is exposed, and which have been pointed out by Hlasiwetz,⁵ and in part also by the brothers Rogers.⁶

So far as we know, those observers who have previously touched upon this subject have been occupied with quantitative considerations only. They have, therefore, very properly rested content when by experiment they have satisfied themselves that the last potash-tube of their series no longer increased in weight during the space of time occupied by a single experiment.⁷ It must, however, be evident to any one who will perform the experiment, that the presence of an amount of carbonic acid which could not be detected by any weighing of potash-tubes may readily be made manifest by precipitating it as crystallised carbonate of lime. In this connection it should be mentioned that Brunner⁸ has distinctly called attention to the extreme difficulty of completely absorbing carbonic acid from the air. Brunner could accomplish this neither by means of a solution of baryta, nor by a mixed solution of chloride of barium and caustic ammonia,⁹ by bits of sponge moistened with baryta or lime-water, nor even by fragments of caustic potash, or asbestos moistened with a solution of potash; he finally chose slightly-moistened hydrate of lime as the best absorbent, and maintains that his method of determining carbonic acid in the air, by this means, is sufficiently accurate for all ordinary cases.¹⁰—*Proceedings of the American Academy of Arts and Sciences.*

² *Wiener Akad. Bericht*, 1856, xx, 189.

³ *Am. J. Sci.* 1848 [2], v, 115; *Edin. New Phil. Journ.* xlv, 150.

⁴ Compare, for example, Dumas and Stass, "Sur la véritable Poids Atomique du Carbone," *Ann. Ch. et Phys.* [3], i, 18.

⁵ *Poggendorff's Ann. der Phys. u. Ch.*, 1832, xxiv, 571.

⁶ Having had repeated opportunities of observing the great difficulty—not to say impossibility—of absorbing carbonic acid from mixed gases, especially if these contain so much as one or two per cent. of it, by means of these and similar liquids, I am glad to bear witness to the entire accuracy of this much-neglected statement—F. H. S.

¹⁰ It is a curious act, which not only corroborates Brunner's observation, but also suggests a more extended use in the laboratory of his favourite absorbent, that manufacturers of coal-gas find in practice, that carbonic acid, when not present in very abnormal quantity, may be readily removed from the impure gas by passing the latter through several layers of dry hydrate of lime, spread in fine powder upon perforated iron grates, or upon shelves of basket-work ("dry-lime purification"); while it is practically impossible to absorb all the carbonic acid from similar gas by the wet system, in which the impure gas is forced through milk of lime, contained and agitated in several successive purifiers. Yet with the other chief impurity of coal-gas, sulphuretted hydrogen, the reverse of this is the case; for, by means of the wet-lime purification, this substance can, in ordinary cases, be very readily and completely removed, with an expenditure of but little lime, while with the dry purification this result is far less easily attained. Moreover, this non-absorption of carbonic acid in the wet-lime purifiers cannot be due to any interference caused by the sulphuretted hydrogen; for it is just as difficult to absorb all the carbonic acid from rosin-gas, which contains no sulphuretted hydrogen, as it is to absorb that in the gas prepared from coal.

bonate of lime which has been formed. If, therefore, it is desired to detect small quantities of carbonic acid by means of lime-water, it is necessary to agitate some carbonate of lime with the latter, in order that it may be saturated, before filtering it. That the carbonate of lime separates, after a time, from the lime-water which had previously held it in solution has also been shown by one of us (*Am. J. Sci.*, 1858 [2], xxv, 42)—at that time entirely ignorant of the experiments of Vogel and Welter.

² *Poggendorff's Ann. der Phys. u. Ch.*, 1860, cix, 346.

³ The reader of Karsten's Memoir will observe that, like ourselves, he obtained for the most part only crystalline carbonate of lime, no immediate cloudiness.

⁴ *Gilbert's Ann. der Phys.*, 1814, xlvii, 119, note.

*Note on Sources of Error in the Employment of Picric Acid to Detect the Presence of Potash,*¹ by M. CAREY LEA.

PICRIC acid enjoys a high reputation as a test for potash. Employed in its alcoholic solution, or as soda or ammonia salt, sometimes as magnesia salt, it gives with potash solutions a dense yellow crystalline precipitate. If the solution containing potash be very dilute, the precipitate may not make its appearance till after some hours' repose, and it then forms long, delicate needles.

Rose remarks that this re-agent "is even more sensitive than the solution of chloride of platinum." In his summing up, he observes, that, of the various re-agents, chloride of platinum, tartaric acid, picric acid, perchloric acid, sulphate of alumina, and hydrofluosilicic acid, the latter is insufficient to distinguish between potash and soda; and that the chloride of platinum and sulphate of alumina give the same reactions with ammonia as with potash.² No qualification is made with respect to the certainty of the indications afforded by picric acid. Plattner makes the same observation as to the greater sensitiveness of picric acid compared with bichloride of platinum and equally without qualification as to its reliability.³

I therefore believe that the remarkable insolubility of otherwise soluble alkaline picrates in alkaline solutions has not been before pointed out. If an alcoholic solution of picric acid be poured into a solution of carbonate of soda, it occasions an immediate dense yellow precipitate, not to be distinguished in appearance from a precipitate of picrate of potash, and liable to be mistaken for it with the greatest facility. The picrate of soda formed in the case just mentioned is the most soluble of all the alkaline picrates, and *a priori* we should not expect to find it precipitated under these circumstances. An aqueous solution of picrate of soda added to one of carbonate of soda acts in precisely the same manner.

To ascertain if these reactions were extended to various compounds of picric acid, examinations were made which gave the following results:—

Alcoholic solution of picric acid added to—

solution of carbonate of ammonia, gave an immediate dense yellow crystalline precipitate.

" carbonate of soda gave not so instantaneous a precipitate, but one which, after standing, became even more dense.

Aqueous solutions of picrate of soda gave with

solution of carbonate of ammonia	} abundant precipitates.
" sulphate of ammonia	
" carbonate of soda	
" phosphate of soda,	slight precipitate.

Aqueous solution of picrate of ammonia gave with

solution of carbonate of ammonia	} abundant precipitates.
" sulphate of ammonia	
" carbonate of soda	
" phosphate of soda,	none.

Aqueous solution of picrate of magnesia gave with

solution of carbonate of soda	} abundant precipitates.
" carbonate of ammonia	

When the alkaline solutions to be tested are otherwise than very dilute, there is absolutely no difference in the appearance of the precipitates. When the amount of

the precipitate is small, the following differences may be observed:—

The potash precipitate forms longer needles, and when these are found only after standing, or when the precipitate is re-dissolved by heat and allowed to crystallise by slow cooling, they exhibit a beautiful play of red and green colours.

The soda salt is of a lighter and brighter yellow than either the potash or ammonia salt. Its needles are also shorter than those of either of the other compounds, and when the precipitate has been re-dissolved by heat, and allowed to crystallise by slow cooling, they tend to agglomerate themselves together in spherical masses. This is a very marked character, and when exhibited is unmistakeable, but is not always shown.

The ammonia salt when crystallised in quantities is very distinct in appearance from the potash salt; but when thrown down in small portions, even by slow crystallisation, cannot be distinguished from it with certainty. The prisms are flatter, but these differences are not apparent in very slender needles. The play of colours, which the ammonia salt exhibits to a less degree than the potash, disappears entirely in the soda salt.

The above reactions indicate that extreme caution must be used in employing picric acid as a test for potash. The precipitates above described do not re-dissolve in any excess of the picrate or of picric acid; nor is it to be supposed that concentrated alkaline solutions are necessary to cause them,—on the contrary, in many cases very dilute ones are sufficient. To determine within what limits such reactions are produced, the following trials were made:—

A. Dilution of the alkaline solutions. Limits obtained, (1.) With carbonate of soda,

A solution containing 5 per cent. anhydrous carbonate of soda gave

with alcoholic solution of picric acid, an immediate abundant precipitate; in ten minutes the tube could be inverted without spilling the contents.

with solution picrate of soda, an immediate precipitate, becoming in the course of an hour a very dense one.

A solution containing $2\frac{1}{2}$ per cent. carb. of soda gave with alcoholic picric acid, a slight precipitate, increasing very much by standing, so that after eighteen hours the tube could be inverted without spilling the contents.

with picrate of soda, none.

A solution containing 1 per cent. carbonate of soda gave with alcoholic picric acid, slight precipitate, after eighteen hours' repose.

with picrate of soda, none.

(2.) With sulphate of ammonia:—

A solution containing 5 per cent. anhydrous sulphate ammonia gave

with alcoholic picric acid, immediate dense precip.

with picrate of soda, " "

Solution containing $2\frac{1}{2}$ per cent. sulph. ammonia gave with alcoholic picric acid, immediate dense precip.

Solution containing 1 per cent. sulph. ammonia gave with alcoholic picric acid, immediate dense precip.

with solution picrate of soda, immediate slight precipitate.

The solution, on standing, gave a beautiful crystallisation of long needles, with bright play of colours, exactly resembling the reaction of potash salts.

with picrate of magnesia, by long standing, a very faint precipitate.

with picrate of ammonia, none.

¹ American Journal of Science and Art.

² Rose, "Handbuch der Analytischen Chemie," Band i. s. 6—10.

³ Plattner, "Die Probirkunst mit dem Lothrohre," Third Edition, p. 178.

Solution containing $\frac{1}{100}$ th anh. sulph. ammonia gave with alcoholic picric acid, after a short interval, an abundant precipitate.

Solution containing $\frac{1}{100}$ th anh. sulph. ammonia gave with alcoholic picric acid, after a short interval, a considerable precipitate.

Solution containing $\frac{1}{1000}$ th anh. sulph. ammonia, gave with alcoholic picric acid, no precipitate, even after twenty-four hours' repose.

B. Limits obtained with respect to indications of diluted solutions of alkaline picrate:—

A solution of picrate of ammonia, in 200 parts water, gave, with an equal volume of strong solution of carbonate of ammonia, an immediate precipitate of small, yellow needles. In ten minutes a considerable quantity settled at the bottom of the vessel.

A solution of same salt in 400 water gave, with an equal volume of solution carbonate ammonia, signs of a precipitate in a few minutes, and, after some hours, an appreciable quantity settled at the bottom of the vessel.

With 1 part picrate ammonia in 800 water no precipitate was produced by solution of carbonate ammonia even after twenty-four hours' repose.

The conclusion to be drawn from these results is,—

That alcoholic solution of picric acid or aqueous solution of picrate of soda will produce a precipitate in almost any alkaline solution, whether of soda, ammonia, or potash, except under circumstances of great dilution, especially if allowed to repose for twenty-four hours.

That picrate of ammonia and picrate of magnesia give the same results, but in a less degree.

That picric acid is, therefore, wholly unreliable as a test for potash, the results obtained being such as would tend altogether to mislead those who are not extremely familiar with the appearance of the precipitates, and that in some cases the results are so deceptive that even eyes most familiar with these reactions might be deceived; for example, in the result obtained above by testing a solution containing 1 per cent. sulphate of ammonia with solution of picrate of soda. In this case a crystallisation of picrate of ammonia was obtained perfectly simulating that of the potash salt.

Picric acid is, in fact, a better test for soda than for potash, because with a soda solution it gives a precipitate which, re-dissolved by heat, generally, but not always, gives a characteristic spherically-radiated, bright canary-yellow crystallisation, whereas the precipitate obtained from a potash solution can never be positively distinguished by its appearance from that afforded by an ammonia solution; and we have just seen that a solution containing $\frac{1}{100}$ th of sulphate of ammonia, or even less, is capable of producing such a precipitate.

On the Microscopical and Chemico-Microscopical Characters of various Starches and Grain-meals, by JOHN HORSLEY, F.C.S.

(Continued from page 163.)

BARLEY-MEAL.

Under Water, the starch granules are not unlike those of wheat, as at first sight to be easily mistaken for them, but on comparison it will be found that the smallest grains are considerably less in size than the corresponding ones of wheat, whilst here and there some of the larger ones have a slight cleft or groove running through them.

Under Acid Hydrochloric, the starch granules in-

stantly dissolve, leaving behind cellulose matter, and on pressing the over-glass a little closer, globules of oil collect. Here I take occasion to correct an apparent omission when treating of wheat flour, which, like barley, contains oily matter, and would be rendered evident by hydrochloric acid.

Under Acid Sulphuric, the granules are rapidly but irregularly acted on at their edges, and the cellulose reddened.

Under Liq. Potass. Dil., the starch granules immediately swell out, then collapse and curl over, presenting the appearance of a slit or furrow down their entire length, and passing rapidly into a transparent jelly. By this test barley is readily distinguished from wheat, which is less rapidly acted on.

Under Liq. Ammon. F., no reaction beyond greater definition and the discoloration of the cellulose matter.

Under Polariscopes, polarises very faintly.

OATMEAL.

Under Water, these starch granules are readily distinguished from every other by certain peculiarities. The larger ones are of a round or oval shape, rather dark shaded, of an aggregate or composite character, as of several smaller granules of a diamond-like or angular form being dove-tailed or clustered together.

Under Acid Hydrochloric, as was the case with barley, the starch is rapidly dissolved, but a larger quantity of oil is produced than with barley or wheat.

Under Acid Sulphuric, the peculiar reticulated appearance of the compound globules is instantly destroyed by the dispersion and gelatinisation of the diamond-shaped particles. The cellular matter and fibrous structure are also rendered beautifully evident, sometimes acquiring a deep-yellow or brown tinge, with patches of pink.

Under Liq. Potass. Dil., is instantly dissolved, forming a viscid but opaque jelly, and, besides the discoloration of the cellulose matter, some small oil globules are seen floating about.

Under Liq. Ammon. F., no reaction is produced beyond the clearer definition and discoloration of cellulose matter.

Under Polariscopes, does not polarise.

INDIAN CORN, OR MAIZE.

Under Water, but for their larger size, the granules of starch resemble the smaller or detached grains of oats, being, like them, angular or diamond-shaped, with cupped central depressions, and sometimes radiating lines.

Under Acid Hydrochloric, the starch granules of the whole-ground corn are rapidly dissolved, with the deep discoloration of the cellulose, and the production of a much larger quantity of oily or fatty matter than was previously noticed in other grains; but the starch in its pure state, free from colour and cellulose, as Polson's corn flour, or Oswego flour, readily dissolves, forming a transparent jelly.

Under Acid Sulphuric, the granules continue more or less intact for a long time; they, however, lose their angular form, and become changed to a flat-round, with a dark spot in the centre. Whole-ground Indian corn (both the white and yellow kind) becomes instantly discoloured of a deep golden-yellow by this re-agent.

Under Liq. Potass. Dil., the pure white starch swells up immensely, retaining its shape more or less for some time, becoming at last a viscid, transparent jelly. The impure or whole-ground kind has the cellulose changed to a deep yellow by this re-agent.

Under Liq. Ammon. F., no reaction, but, as in the previous case, a deep-yellow colour is imparted to the cellulose.

Under Polariscope, polarises very faintly.

BUCK WHEAT.

Under Water, these starch granules are polygonal, very regular in size, and not unlike those of rice, only somewhat larger, perhaps, between rice and Indian corn. Scarcely any markings are observable, but there is generally some brownish colouring-matter of a very finely-reticulated character visible.

Under Acid Hydrochloric, they are readily dissolved; the cellulose matter discoloured, and oil globules liberated.

Under Acid Sulphuric, the starch is rapidly dissolved and gelatinised, the cellulose matter becoming of very dark brown colour.

Under Liq. Potass. Dil., is rapidly acted on, forming a thick transparent jelly, the membrane or envelope surrounding the masses of starch granules (for in the original seed they are packed together in triangular or conical-shaped parcels), is readily recognised from its becoming highly discoloured by this re-agent.

Under Liq. Ammon. F., no reaction is produced, but the envelope and cellulose matter is rendered of a deep yellow colour.

Under Polariscope, does not polarise.

RYE MEAL.

Under Water, consists of nearly equal proportions of very large and small grains, the larger ones being of a full moon-shape, or, rather more protuberant than wheat starch, whilst the smaller ones are of a polygonal shape. The surfaces of the larger ones are sometimes irregular and uneven, at other times a triangular slit is observable across the granule.

Under Acid Hydrochloric, the starch granules readily dissolve, leaving behind cellulose matter with a tolerable quantity of oil globules.

Under Acid Sulphuric, the granules rapidly dissolve and gelatinise, whilst the cellulose matter becomes almost black, with here and there a patch of bright pink colour.

Under Liq. Potass. Dil., the starch is rapidly gelatinised, and the cellulose highly discoloured.

Under Liq. Ammon. F., no particular reaction beyond the discolouration of the cellulose.

Under Polariscope, polarises but faintly.

PEA MEAL.

Under Water, the starch granules are of various shapes, but are principally, when entire, (which is seldom the case with the meal of peas which have been desiccated by heat) of a flat round, long oval, or kidney-shape, of a pearly aspect, with either a deep straight furrow or a triangular slit or crack. They have also strong indentations, and sometimes wrinkled and granulated attachments. One great peculiarity is their concentric lines or markings which (if the grain is perfect and the light good) may be seen to commence at their edges as a kind of long horizontal ring or series of closely-fitting rings up to about the centre of the granule, which may be supposed to be the situation of the hilum, but none appears. In addition to this, the taste and flavour of peas are of easy recognition.

Under Acid Hydrochloric, the starch granules rapidly dissolve, and a very large quantity of oily or fatty matter is separated. The cellulose is also discoloured.

Under Acid Sulphuric, the starch granules retain their form more or less for some time, being (if perfect)

but slowly acted on, the cellular matter rapidly acquiring a deep yellow colour, which is succeeded by a bright pink.

Under Liq. Potass. Dil., is gelatinised almost instantly, leaving only cellulose matter (which appears discoloured and full of pin-holes) undissolved.

Under Liq. Ammon. F., scarcely any reaction is produced, but definition is greatly improved, the cellulose matter, as with potash, being discoloured.

Under Polariscope, polarises a little.

LENTILS (GROUND).

Under Water, the microscopic appearance of the pure starch granules is so closely analogous to those of peas as to render any further description of them unnecessary, but in their mixed state, as meal, they are of reddish colour, by reason of the cellular matter, whilst pea-meal is yellowish.

Under Acid Hydrochloric, the starch granules readily dissolve, and, like peas, furnish a large quantity of oil globules.

Under Acid Sulphuric, reaction the same as with peas.

Under Liq. Potass. Dil., the same as peas, the starch being dissolved, and the cellulose discoloured.

Under Liq. Ammon. F., no reaction, only the cellulose being discoloured.

Under Polariscope, polarise faintly.

BEAN MEAL.

Under Water, although not unlike pea starch, yet the granules are sufficiently distinguishable by their greater regularity, both in size and form, being for the most part of a long oval or lozenge shape, with strongly-marked slits or furrows running down the granule, which is not unfrequently cracked or split in halves, or nearly so. The concentric lines, however, are certainly less distinct than in peas and lentils. The peculiar taste and flavour of beans are recognisable by the palate. Boiling them with water also develops their odour.

Under Acid Hydrochloric, the starch is readily dissolved, the cellulose matter discoloured, and a large quantity of oil globules liberated.

Under Acid Sulphuric, the reaction is similar to that of peas and lentils.

Under Liq. Potass. Dil., is rapidly gelatinised and the cellulose discoloured.

Under Liq. Ammon. F., no reaction, only the cellulose discoloured.

Under Polariscope, polarises faintly.

TECHNICAL CHEMISTRY.

On the Impossibility of Puddling Iron which contains Copper, by Dr. C. LIST.

It has been stated as a matter of belief among practical iron-workers in Germany, that pig-iron which contains copper cannot be puddled; assertions having even been made, that when one puddler wishes to annoy another he will sometimes throw a bit of copper—a small coin for example—into the furnace, so that the iron cannot be made to "rise."

Without giving full credence, as yet, to this statement, Dr. List mentions that he has observed two instances which go to prove that it may be correct. In the case which he has more particularly described, none of the phenomena which ordinarily occur when iron is puddled appeared. Some 400 lbs. of pig-iron having been placed in the furnace, were melted in the course of half-an-

hour, at which time a sample taken from the molten mass was perfectly white, but the usual evolution of carbonic oxide, and consequent swelling or "rising" up of the mass of scales, &c., about the iron, did not ensue. On the contrary, by the time that the balling together of the iron should have commenced, it was evident that the charge could not be worked off; it was therefore removed from the furnace, after having remained there about three-quarters of an hour. As the melted metal was flowing out it emitted numerous beautiful blue sparks, which were also produced when the metal in the furnace was stirred. The sparks were regarded by the workmen as an indication that the iron contained copper.

The amount of metallic iron which remained weighed 240 lbs., 160 lbs. having been lost in the scales and slag. Analyses [for details of which see the original memoir] were made of the original pig-iron, (I.); of the sample taken, as previously mentioned, from the melted iron, as it lay beneath the scales (II.); and of the iron after it had been removed from the furnace (III.).

	I.	II.	III.
Silicon . . .	1'32	0'29	—
Sulphur . . .	0'28	—	0.20
Manganese . .	3'56	—	0'48
Copper . . .	0'35	0'38	0'57

It was evident, therefore, that the 400 lbs. of pig-iron used did really contain nearly a pound and a-half of copper. It appears, moreover, that copper cannot be removed from iron by puddling. Calculating how much copper ought to be left in the iron which was finally removed from the furnace, in case none had been lost in the slag, it is found that there should be 0.58 per cent., almost exactly the quantity which was obtained in analysis No. III.—*Dingler's polytechnisches Journal*.

On the Preparation of Oxide of Lead free from Copper and Iron, by T. WICHMANN.

OF the many plans which have been proposed for utilising the sulphate of lead which is formed in considerable quantity as a secondary product in the preparation of alumina mordants at print-works, the method of reducing it in furnaces to the metallic state has been most frequently resorted to, since the quality of the lead thus prepared is of peculiar excellence. Instead of thus reducing the sulphate of lead, Wichmann proposes to convert it directly into oxide of lead, maintaining that the oxide thus prepared, being uncontaminated with copper or iron, will be well suited for the manufacture of white glaze for pottery—(probably also for making flint glass)—it being somewhat difficult to obtain in commerce oxide of lead which does not contain traces of these impurities. In his method, suggested by that of Mohr¹ for

¹ To a boiling solution of caustic soda, of 1.10 to 1.15 sp. gr. of known strength, an equivalent weight of powdered nitrate of baryta is added. As soon as the baryta salt has completely dissolved, the hot solution should be rapidly filtered in order to separate any sulphate or carbonate of baryta which may have been formed from impurities of the soda, and the filtrate set aside in a cool place. An abundant crop of crystals of hydrate of baryta soon forms. These crystals are to be drained from the solution of nitrate of soda, upon a funnel loosely plugged with cotton and the drying completed [according to Mohr] by means of a centrifugal machine. If required for purposes where the adhering nitrate of soda should be avoided the hydrate of baryta may be re-crystallized. In place of the nitrate of baryta one may use chlorid of barium—or in place of the soda, potash; but as a rule the presence of chlorine in the product would be more objectionable than that of nitric acid. On the small scale the process of boiling is best conducted in a glass flask, but where larger quantities are desired a covered iron kettle may be used—as in the common method of preparing caustic alkalies.—From *Archiv der Pharmacie*, lxxxviii, 38; in *Chemisches Centralblatt*, 1856. [N. F.] i. 834.

preparing caustic baryta, a quantity of caustic soda lye, of from 28° to 30° B. = about 1.25 specific gravity, having been brought to boil in an iron kettle, the sulphate of lead is added to it little by little, with constant stirring, oxide of lead and sulphate of soda being produced. The amount of sulphate of lead which shall be added to a kettleful of soda, and which is somewhat less than the quantity which theory would indicate, must be determined once for all by direct experiment, sulphate of lead being added until the sharp caustic taste of the soda is no longer manifest. It is well, however, to leave some free soda in the solution, lest a portion of undecomposed sulphate of lead should contaminate the oxide of lead which is formed, although a small amount of the latter will combine with the soda and remain in solution. During the boiling the decomposition is rapid and complete, unless the soda lye contained much carbonate of soda, in which case a white scum of carbonate of lead will separate.

When the proper quantity of sulphate of lead has been added to the lye, and the decomposition is finished, the contents of the kettle must be thrown into a vessel containing water, in order that the sulphate of soda which has been formed may be retained in solution, which would not be the case if no water were added to the mixture. The oxide of lead, which is in the form of exceedingly fine crystalline scales of a light yellowish red colour and of great purity, is readily deposited; after being thoroughly washed, to remove the sulphate of soda, it is dried and finally ignited in a reverberatory furnace—the charge being subsequently allowed to cool as slowly as possible.

As thus prepared, the oxide is an exceedingly soft powder of a yellowish-red colour like prepared litharge; it contains about 2 per cent. of alumina, which cannot readily be removed, but which is not at all detrimental to its use in glazing pottery, and also a larger or smaller quantity of carbonate of lead, according as the soda lye was more or less contaminated with carbonate of soda.

The crude sulphate of lead, as it comes from the print-works, should be agitated and washed with water—or, better, forced through a fine sieve, in order to break up lumps, and to remove any foreign substances—before being used.

The residual solution of sulphate of soda being evaporated, together with the first portions of the wash-water, affords Glauber's salt, containing alumina, plumbate of soda, acetate of soda (the acetic acid and alumina being both derived from the crude sulphate of lead), and chloride of sodium (from the soda lye); being free from iron, it is suitable for glass-makers' use.—*Polytechnisches Centralblatt*.

On Cleansing Mordanted Cloth before Dyeing, by J. LEWENTHAL.

IT is well known that cotton cloth upon which alumina or iron mordants have been printed must be submitted to the so-called dunging process before it can be dyed.

The action of the ingredients used for dunging is:—1, To completely fix the mordants; 2, to remove the substance (starch, gum, &c.) with which the mordant was thickened for printing; (3, as well as to remove the

² For a ready method of obtaining the weight of moist sulphate of lead, see Mr. F. Mayer's paper in the March No. of the *American Journal of Science*.

³ If a more concentrated soda lye be used, say of 40° B., somewhat larger crystals of a dark red colour are formed.

excess of mordant, and by combining with it prevent it from soiling those portions of the cloth which are to remain uncoloured.—E. Kopp.)

Giving special prominence to the second of these desiderata, that of removing the thickening agent, Löwenthal proposes to employ diastase, as contained in malt, in order to convert the starch into sugar, thus rendering it readily soluble so that it can be easily removed from the cloth.

By direct experiment he has satisfied himself that the addition of malt to baths of bran (previously boiled) or of cowdung, is decidedly beneficial; cloths which have been printed with an alumina mordant thickened with starch being very rapidly cleansed in this manner. With iron mordants a somewhat longer time is required than with those of alumina, but they are nevertheless cleaned much more rapidly than when no malt is used.

A temperature of 35° to 40° R. [= 111° to 122° F.] is sufficient that the malt shall act powerfully.

When the dunging-bath contained water-glass (sesquioxide of soda) the use of malt gave rise to the formation of spots upon the cloth in the process of dyeing. The author thinks, however, that this objection might perhaps be obviated if purer diastase were used instead of malt.

In this connection Barreswil remarks that he has employed pepsine with success in removing colours fixed with albumen.—*Journal für praktische Chemie.*

On the Value of Different Kinds of Soap, by R. GRAEGER.

COMPLAINTS of consumers in regard to the value, or rather efficacy, of samples of soap, which to the best of the manufacturer's knowledge have been well prepared, are not uncommon.

It is very probable that the usual explanation which is offered, whenever a soap fails to fulfil the expectations of its consumer, viz. that it contains too much water, may be in many cases correct. Admitting this, and various other contingencies, which are of importance in deciding upon the value of a soap, there appears to be another obvious reason why different soaps containing equal amounts of water may still possess different degrees of efficacy.

It is evident from the different equivalent weights of the various fatty acids, that the amounts of caustic alkali taken up by them in the formation of soap must be of unlike magnitude.

If it be true, that the detergent power of soap is entirely dependent upon the amount of alkali which it contains, of course it follows that those soaps which contain the largest proportion of alkali,—or in other words, those containing a fatty acid, the equivalent weight of which is small,—must be the most efficacious.

Since the difference between the equivalents of the common fatty acids are not large, these considerations are perhaps of little or no importance in so far as concerns the consumption of soap in household economy—the total amount used in a single family being but small. In a manufacturing establishment, however, where fifty or a hundred thousand pounds of soap may be used in the course of a year, differences which cannot be deemed insignificant must exhibit themselves.

For example, the equivalent weights of several soaps (regarded as anhydrous), in common use, are as follows:—

Oleic acid (red oil) soap . . .	= 3800·95
Palm oil . . .	= 3588·85
Tallow . . .	= 3300·95
Cocoa-nut oil . . .	= 3065·45

Calculating from these weights how much of each of the other soaps would be required to replace 1000 pounds of tallow soap, the following quantities will be found:—

Pounds of	Per cent.
1151 oleic acid soap, i.e., 15·1 more than tallow soap.	
1087 palm oil " i.e., 8·7 " "	
928 cocoa-nut oil, i.e., 7·2 less than " "	

Differences like these must certainly be of importance in practice; and could, doubtless, be detected by direct experiment, if any one would undertake a comparison of the various kinds of soap—a research which would not be easy, however.—*Böttger's polytechnisches Notizblatt.*

PHYSICAL SCIENCE.

Early Researches on the Spectra of Artificial Light from Different Sources.

Now that the attention of scientific men is being drawn to the method of analysis by means of spectrum observations, our readers will feel an interest in knowing that many of the observations which are now being followed up by Continental savans, have been investigated in a more or less perfect manner by English experimentalists. Wheatstone and Talbot, whose names have been unaccountably passed over in silence by recent writers on the subject, have, in fact, observed and recorded so much connected with the spectra of artificial light from different sources, that we have thought it advisable to collect together some of the more important results which these experimentalists have obtained. They will be read with more interest, inasmuch as Professor Wheatstone has placed at our disposal several papers and memoranda of experiments made by him nearly thirty years ago, many of which have either not been published at all, or have only appeared in abstract in the *Transactions of Learned Societies.*

The first public account of any of these researches was communicated by the Professor to the British Association for the Advancement of Science, at their meeting in Dublin in August, 1835. With the exception of a short abstract of the paper which was furnished by the author for the volume of *Transactions* published by the Association, the only account of this paper which appeared was a short notice in the "Proceedings of the Fifth Meeting of the Association"—a local publication,—issued without the sanction of the Sections, and consequently uncorrected by the authors. In this it states that on the 12th of August, 1835:—

"Professor Wheatstone detailed the results obtained by him when attempting to form by the prism a spectrum or coloured image of the spark obtained from an electro-magnetic apparatus, as also of the sparks produced under various circumstances from simple galvanic influence, and from an electrical machine. In the spectrum or coloured image formed by a beam of solar light when analyzed by a prism, the colours succeed each other without any interruption, except that when the prism is of very good glass, certain dark bars cross the image at fixed though unequal distances.

"Mr. Wheatstone examined the light produced by a

revolving electro-magnet, expecting to find something of a similar effect; but to his surprise he found that the simple coloured lines that presented themselves were separated by great intervals, in which no light whatever could be seen,—and this with such regularity as to afford a test, not only of the substance through the instrumentality of which the light was procured, but also a test of the strict identity of the light produced by galvanism and by electro-magnetism, and a proof that the light of common electricity was essentially distinct from either. When mercury was used as the means of producing the spark, Professor Wheatstone found the series of colours and their distances from each other, to be readily distinguished from the series and distances of the colours resulting from the spark produced when any other metal was used; and the same circumstances for each of the other metals which he was able to use in the fluid state, were so characteristic as to afford means of distinguishing them. He also found that the light produced by the combustion of each of these metals was so dissimilar from that produced from the electro-magnetic, galvanic, or electric spark, that Professor Wheatstone came to the important conclusion that these sparks cannot result from the combustion of the metals, but rather from a portion of the metallic conductor carried off by the electric discharge and ignited. This communication created much interest; and Professor Wheatstone was kind enough to promise on a future occasion to exhibit these curious appearances to any members of the Association who might feel an interest in the subject. This he accordingly did on two succeeding occasions, when we had the pleasure of seeing the various phenomena, and had an opportunity of observing how very accurately the Professor had detailed the appearances."

The following abstract was prepared by Professor Wheatstone himself for the purpose of being included in the official account of the transactions. It is taken from the fifth report of the British Association for the Advancement of Science, 1835, page 11 of the "Transactions of the Sections":—

"The following is a brief notice of the principal results stated in this communication: 1. The spectrum of the electro-magnetic spark taken from mercury consists of seven definite rays only, separated by dark intervals from each other. These visible rays are two orange lines close together, a bright green line, two bluish-green lines near each other, a very bright purple line, and lastly a violet line. The observations were made with a telescope furnished with a measuring apparatus; and to ensure the appearance of the spark invariably in the same place, an appropriate modification of the electro-magnet was employed. 2. The spark taken in the same manner from zinc, cadmium, tin, bismuth, and lead, in the melted state, gives similar results; but the number, position, and colours of the lines vary in each case. The appearances are so different that, by this mode of examination, the metals may be readily distinguished from each other. A table accompanied the paper, showing the position and colour of the lines in the various metals used. The spectra of zinc and cadmium are characterised by the presence of a red line in each, which occurs in neither of the other metals. 3. When the spark of a voltaic pile is taken from the same metals still in the melted state, precisely the same appearances are presented. 4. The voltaic spark from mercury was taken successively in the ordinary vacuum of the air-pump, in the Torricellian vacuum, in carbonic acid gas, &c., and the same results were obtained as when the experiment was performed in the air or in oxygen gas. The light, therefore, does not arise from the combustion of the metal. Professor Wheatstone also examined, by the prism, the light which accompanies the ordinary combustion of the metals in oxygen gas, and by other means, and found the appearances totally dissimilar to the above.

5. Fraunhofer having found that the ordinary electric spark examined by a prism presented a spectrum crossed by numerous bright lines, Professor Wheatstone examined the phenomena in different metals, and found that these bright lines differ in number and position in every different metal employed. When the spark is taken between balls of dissimilar metals the lines appertaining to both are simultaneously seen. 6. The peculiar phenomena observed in the voltaic spark, taken between different metallic wires connected with a powerful battery, were then described, and the paper concluded with a review of the various theories which have been advanced to explain the origin of electric light. Professor Wheatstone infers from his researches that electric light results from the volatilization and ignition, not combustion, of the ponderable matter of the conductor itself—a conclusion closely resembling that arrived at by Fusinieri from his experiments on the transport of ponderable matter in electric discharges."

The original paper, of which only the above brief abstracts were made, was never published. Engrossed by his more important telegraphic researches, the Professor, after reading it before the meeting, put it on one side, intending to continue the experiments when he had more leisure, and from that day to the present it has scarcely seen the light until it was placed by the author in our possession. Subsequent researches of the Professor on the same subject might have been added to it, and the illustration, which is an exact copy of the one exhibited at Dublin, might have been enlarged and improved, but it has been thought better to print it *verbatim et literatim* as it was read before the Association, and to embody the subsequent observations in a separate paper which will shortly follow in this Journal. The complete paper will be given in our next number.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE V.

Crystallization—Influence of Cohesion on Affinity—Precipitation—Normal Constituents of Water as it Occurs in Nature—Sources of Impurity in Water for Domestic Purposes.

We have seen that when certain solid substances are placed in contact with water they undergo a gradual disintegration and finally disappear altogether, in other words, they *dissolve*. The cohesion between their particles is broken up, and the cohesion between those particles and the particles of water becomes stronger than that between the solids themselves. If by evaporation we reduce the quantity of water present we recover the substance in a solid form, but in thus passing from the liquid to the solid form it generally undergoes the process of crystallization; it assumes certain general geometrical shapes which we term crystals. The same effect is generally produced when, instead of evaporating the water, we let the substance dissolve at a higher temperature, for generally speaking the higher the temperature of the water the more solid matter will it dissolve. There are a few exceptions to this rule, but they are very few. There are several other modes of forming crystals besides this one of solution; and the solution, I need scarcely say, may take place in any other solvent besides water. When a solid body is formed by evaporating a solution, the body in thus passing from the liquid to the solid state, assumes a crystalline form, as a general

rule. So in passing from the gaseous state to the solid state it assumes a crystalline form. In the case in which we produced a solid from two gases in a previous lecture we should have seen, if we had closely examined the substance, that it was in the crystalline form. However, at present it will not be convenient to consider this mode of forming crystals more in detail. The crystals which separate from water generally carry along with them, or combine with, at the time of crystallization, a certain quantity of that liquid which afterwards exists in the solid crystal. Some of these crystals when exposed to the dry air lose a portion or the whole of the water with which they have combined during crystallization, they fall to powder, and are said to *effloresce*. On the other hand, some crystals will attract a larger quantity of water, and will actually dissolve in the water which they thus attract from the atmosphere. Such substances are said to be *deliquescent*. Here we have in this vessel a sample of both of these kinds of salts. There is the first on the floor which is parting with its water to the air, whilst the other is absorbing the water which is given off by the salt on the floor, and the air in this jar is kept dry by the action of the deliquescent salt. This lower salt will finally crumble into a fine powder, and the other will dissolve in the upper part of the apparatus.

But there is another point connected with crystallization that we must just notice in passing before leaving this part of our subject; it is this—that crystals, in being formed by any of the processes to which I have just alluded, are liable to assume two or more forms—very frequently two forms. They crystallize in two shapes, and such bodies are said to be *dimorphous*, or crystallizable in two forms. One of these forms is generally less stable than the other—less firm in the cohesion which knits particles together; and this less stable form very frequently resolves itself into crystals of the more stable form. We have a curious instance of this kind of crystallization—this dimorphism—in iodide of mercury, some of which we have spread here upon a sheet of paper. This salt is capable of crystallizing in two forms; one of those forms is stable, or permanent, and the other is comparatively unstable. These scarlet crystals we have upon the paper constitute the stable form of this salt; but if we apply heat to it by means of this spirit-lamp I think we shall find that we shall transform this scarlet salt into a beautiful yellow salt. [The lecturer passed the plain side of the sheet of paper backwards and forwards at a short distance above the flame of the lamp. The change of colour gradually became apparent.] I want to get a larger portion of this transformed, I think we have now a sufficient surface transformed. You see the difference—the remarkable difference between the colour of the two forms—the two crystalline forms of this same salt. Now, the yellow salt we have thus obtained is an unstable crystal; and if I were to scratch one of these yellow crystals with a pin's point it would be transformed into a mass of scarlet crystals, and the scarlet crystallization would propagate itself over the whole surface; that, however, would require hours to take place. You see wherever I draw a line across this sheet of yellow crystals that line is red again. Wherever this pressure is applied, there they crumble down into the scarlet form.

A similar alteration in crystalline form, and sometimes accompanied also by remarkable changes of colour, occurs when salts are made to crystallize with different amounts of water in them. Here, for instance, is a drawing of a cockatoo, and it is coloured with some preparations of cobalt. The colour you see is a very faint pink, so faint, indeed, that it is hardly visible. Now we will expose this to heat so as to alter the crystalline form of those salts with which it has been painted; and we will see whether we produce any effect upon our cockatoo. [The picture was warmed before the fire.] There, I think

we have effected a considerable transformation in our bird. You see the pink salt with which the wings were painted has become intensely blue, whilst the pink of the other parts, which were painted with nitrate of cobalt, has become much stronger. The change is owing to the different forms of crystallization reflecting different rays of light. The crystals on the wings which before reflected the pink rays now reflect the blue rays.

This separation of solid substances from solution is due to the action of a force called *cohesion*; and this force under such circumstances very frequently exercises considerable influence upon chemical affinity. It is possible, in fact, so to arrange the affinities of two different compounds that when mixed together a mere touch—the slightest exertion of this force of cohesion—will be sufficient to determine the decomposition of those bodies and form two new compounds. For instance, I will pour upon a glass plate here—upon a smooth glass plate—a mixed solution of acetate of potash and tartrate of soda. You see they remain perfectly transparent when first mixed; and I hope we shall be able to get them on the glass still in a perfectly transparent condition. [Some of the solution was then poured over the surface of the glass.] You see this solution is still transparent, I want to get it as much spread over the glass as possible; and now the salts are in such an unstable state that I can determine a new form of combination simply by the touch of my finger. I have written there the word “cohesion” upon that glass; and I dare say that the simple motion of my finger over the glass, through presenting surfaces favourable to the exertion of this force, cohesion, will be sufficient to produce the figure of that word distinctly developed. We shall have a bitartrate of potash crystallized upon the surface of the glass. I will pour off the residual fluid, and here you have the word distinctly brought out. [The word “cohesion” appeared, written in white crystals on the glass plate.] Let me explain this action a little more clearly. We had, in the first place, acetate of potash and tartrate of soda; and simply by this action of cohesion we get an entire reversion of the combination. Here [in the residual fluid] we get acetate of soda; and there where the word is written is tartrate of potash by itself, simply owing to the insolubility of the tartrate of potash. Here we have the cubes which will show us the reaction. This [arranging the cubes according to the original compounds] was the state of affairs before this trifling amount of cohesion was brought into play. This mere movement of the finger through the solution effected a transfer of the acetic acid from the potash to the soda, and of the tartaric acid from the soda to the potash.

Now, this modifying influence of cohesion upon chemical affinity is due entirely to the formation of a body comparatively insoluble in the liquid in which this force is made to act; and we have a great number of chemical decompositions which occur in solution almost entirely through this action of cohesion.

If, by any possibility, on the mixture of two or more solutions, an insoluble body can be produced by the mixture, that body will be almost sure to be formed. For instance, here we have a clear and transparent solution of acetate of lead, and here we have a similar transparent solution of chromate of potash. I pour them together,—you see we have the chromic acid leaving the potash, and combining with the lead of the acetate of lead. We get a yellow-coloured insoluble compound, which, upon examination, we should find to be chromate of lead. The chromic acid has gone over to the lead, and the acetic acid has gone over to the potash. To give yourselves a clear idea of the mode in which this takes place, you must imagine—which, in fact, can be proved to be the case—that when you mix these solutions you have there not only two salts, but four salts. There is, then, a division of the two acids between the two bases, and of the

two bases between the two acids. There would then be produced acetate of potash and acetate of lead, and chromate of potash and chromate of lead. The chromate of lead is, however, insoluble; it is thrown down, a further portion of the acetic acid combines with some more of the potash, forming more of the acetate of potash; the chromic acid and lead thus set at liberty produce a further quantity of the chromate of lead, which, like that first formed, is precipitated; and so the decomposition continues till, in a time too short to be appreciated, all the chromate of potash and lead have united, and the compound is precipitated, leaving only the acetate of potash in solution. That this really takes place quite independent of the action of cohesion may be proved by adding together colourless salts which are not capable of producing in this way a solid salt. For instance, I will add to a solution containing perchloride of iron, another solution containing sulphocyanide of potassium. If this action of which I have been speaking takes place the sulphocyanic acid will divide itself between the iron and the potassium. If this be the case, then, sulphocyanide of iron will at once be formed, and if this be formed we shall see it by its colour, which is an intense blood-red. [The solutions were then poured together.] Here we have it, proving that we have really this transfer between the acids and the bases quite independent of precipitation; for the mixed liquid is quite clear, and might be filtered without leaving any trace upon the filter through which it passed.

Water, in its chemical relations, is what is termed a neutral body—a neutral substance. It possesses neither acid nor alkaline reaction. Although it is capable sometimes of playing the part of an acid, and sometimes of a base, yet it is, in its reactions what chemists call a "neutral substance." Let me explain what I mean by this. All solutions are said to be acid when they possess the property of reddening litmus paper, paper which is coloured blue by an infusion of litmus, a kind of colouring matter which is obtained from a description of lichen. If I dip this paper into a solution which reddens it I know at once that the solution has an acid reaction. If I first redden the paper with acid and then immerse it into an alkali the blue is restored, or, if instead of using the litmus paper, I take this yellow turmeric paper and dip it into a solution which browns it, this proves alkalinity of reaction. By dipping the browned turmeric paper into an acid solution the yellow colour is again restored. Now, solutions which do not manifest either of these properties—either the reddening of the litmus, or the browning of the turmeric—are said to be neutral. Such a solution we shall get, I dare say, by pouring the acid solution into the alkaline solution, if we have the proper strength. [The acid and alkaline solutions were then poured together.] If we have now produced a neutral solution it will have no effect upon our papers. It will not affect the blue nor the red litmus; and it does not affect the turmeric paper. It is *neutral*, in fact. So by these simple tests we can ascertain at once whether a solution is acid, alkaline, or neutral.

Water is, however, as I have just mentioned capable of uniting both with acids and with bases. It unites with such acids as sulphuric acid and nitric acid with so much force that it is almost impossible to separate it again, except by indirect means, from these acids when it has once combined with them. With phosphoric acid also, for instance, it unites with such force that we can expose the hydrate of phosphoric acid to a most intense heat without driving off the water from the compound. Water plays the part of an alkali or base, when it unites with an acid, and of an acid, when it unites with an alkali. Water sometimes unites with salts, and it then constitutes what is called the *water of crystallisation*; and when water is thus combined with salts in a crystalline form, as with that carbonate of soda, for instance, and with this sulphate of soda, although these salts have the temperature of this room and we can

raise their temperature without liquefying them, still the water which is combined with them is not liquid water, but is contained in the salts in the solid form of ice. Now, I think we shall be able to demonstrate that. If the water be in the position of ice it must be capable of absorbing a large amount of heat when the salt is made to dissolve in any liquid. The ice will then pass into the liquid condition, and we have seen that ice in passing into the liquid condition absorbs a great amount of heat—142 degrees. In the same way, if it be really ice it must give out as much heat when it crystallizes as it is afterwards capable of absorbing when it is subsequently dissolved. Now, here is some of this same salt, the sulphate of soda, reduced to a rather fine powder, and I will cause it rapidly to dissolve by adding to it an acid which will not merely dissolve it but also decompose it. I shall add to it some hydrochloric acid; and we will now place it opposite this thermoelectric pile. If we have cold produced, the red end of the needle moves towards you; if we have heat produced, we have an opposite effect; then the blue end of the needle moves towards you. You see if I place this vessel containing sulphate of soda in front of the pile it has no effect before the acid is added. We will now make the same experiment with the hydrochloric acid poured upon the salt. This acid itself has the same temperature as the room; it fumes, but the fuming is not due to heat; it merely arises from its combining with atmospheric moisture. Now we will see if we get any effect upon that magnetic needle. You see it is beginning to move; the red end is moving towards you. We have cold produced here; we are liquefying the ice in the sulphate of soda. It is now no longer ice; it is water, and it has absorbed 142 degrees of heat, from the hydrochloric acid, from the water of solution, and from its own self. Now let us see whether we can get a similar evidence of the heat coming out of the water when it becomes solid. This sulphate of soda is peculiarly capable of demonstrating this point, for it can suspend its crystallization. If you take a saturated solution of this sulphate of soda you can allow it to get perfectly cold without crystallizing. Here is such a super-saturated solution, and if we just throw into it a crystal of sulphate of soda the crystallization of this whole mass is determined. [The flask containing the sulphate of soda was placed opposite the pile.] I want you to see that at the present time this flask does not affect the pile. If we have heat produced by the crystallization of this solution the blue end of the needle will move towards you, and the red end towards me. [The piece of bladder covering the mouth of the flask was then cut.] There, even by the admission of air, and by the slight tremor caused by the cutting of the bladder at the top of the neck, the crystallization has commenced. You see how beautifully these crystals are shooting down. The mass is gradually solidifying. The water is, in fact, solidifying, and you can see the expansion caused by that action. The needle is also coming round, indicating an elevation of temperature.

Here we have had, in one case, water cooled by the melting of a salt; and in the other case heat emitted by the solidifying of a salt in solution.

(To be continued.)

CHEMICAL SOCIETY.

Thursday, March 7, 1861.

Dr. Hofmann, F.R.S., Foreign Secretary, in the Chair.

Mr. J. J. Coleman was elected a Fellow.

Professor Field read a Paper "On some Minerals from Chili." Black oxide of copper was for a long time known only in Vesuvian lavas, until its discovery in North

America, in the Valley of the Mississippi, and on the borders of Lake Superior. The author met with a dark-brown earthy mineral at the extreme north of Chili which contained 43 per cent. of black oxide of copper, mixed with carbonate of lime and horn blende. He also met with, a few miles north-west of Coquimbo, a black, amorphous, earthy mineral, which proved to be sulphate of lead, coloured with protoxide of iron. Its specific gravity was 6.20. It contained 96.7 per cent. sulphate of lead, and 3.2 per cent. protoxide of iron. Fibro-ferrite occurred in botryoidal masses, each nodule being built up of fine, radiating, silky fibres, of a pale golden-green colour. Its composition corresponded to the formula $\text{Fe}_2\text{O}_3 \cdot 2\text{SO}_3 \cdot 10\text{HO}$. It lost 2 equivalents of water by a comparatively short exposure to air, after which its weight remained permanent. It retained 3 atoms of water when dried at 212°F , and became anhydrous between 500° and 600°F . When acted upon by hot water it was decomposed, with separation of an insoluble compound, according to the equation, $3(\text{Fe}_2\text{O}_3 \cdot 2\text{SO}_3 \cdot 10\text{HO}) = 2\text{Fe}_2\text{O}_3 \cdot \text{SO}_3 + \text{Fe}_2\text{O}_3 \cdot 3\text{SO}_3 + 2\text{HO} \cdot \text{SO}_3 + 2\text{HO}$. The insoluble compound is also found native. Bournonite was found in South America for the first time in 1858, by the author, in a mine near Huasco, in the northern part of Chili. Its composition agreed exactly with that of Cornish bournonite, and was represented by the formula $\text{Cu}_2\text{S PbS SbS}_2$. The author found that when sulpharsenites were treated first with strong nitric acid, and then with an excess of potash and an alkaline hypochlorite, the sulphuric and arsenic acids formed could be completely dissolved away from the insoluble oxides of copper, nickel, &c., but that the separation of antimony from sulphuric antimonides, by the same process, was very imperfect. Lastly, the author described a new double sulphide of lead and copper, which he termed Alisonite, having the formula, $3\text{Cu}_2\text{S PbS}$, Chili cuproplumbite being $\text{Cu}_2\text{S} \cdot 2\text{PbS}$.

Dr. HOFMANN gave an account of Mr. Greiss's further researches upon Nitrogen Substitutions.

MANCHESTER

LITERARY AND PHILOSOPHICAL SOCIETY.

PHYSICAL AND MATHEMATICAL SECTION.

February 23, 1861.

Mr. G. V. VERNON, F.R.A.S., read a Paper, "*On the Irregular Oscillations of the Barometer at Manchester.*"

This Paper is an addition to one upon the same subject brought before the Society by Mr. Baxendell, F.R.A.S.

The tables appended to it give the total amount and number of the barometer oscillations at eight a.m., for each month from 1849 to 1860, inclusive; the mean daily amount; the monthly fall of rain, and its difference from the average of twelve years, and Dr. Dalton's forty-seven years' average. For the month of August, the Author was indebted to the kindness of Mr. John Curtis, in supplying him with barometer observations, which were deficient in his own register.

The maximum amount of oscillation takes place in January, and the minimum in July, to which may be added a second maximum appearing to take place in October. The mean total amount of oscillation for the year is 61.410 inches, and the total number 175.5. It appears that a fall of rain, in excess of the average, is generally accompanied by an increase in the amount of oscillation. In the months of February, April, May, June, July, October, and December, a temperature below the mean for the month appears to increase the amount of oscillation; whilst in the remaining months of January, March, August, September, and November, the opposite appears to hold good. A number of oscillations above

the average appears to be accompanied by a greater fall of rain than that accompanying a number of oscillations below the average, in the months of January, February, March, June, October, November, and December. In the remaining months of April, May, July, and September, a number of oscillations above the average appears to be accompanied by a diminished fall of rain.

If the month of June is regarded as abnormal (which is probable as I have no observations for this month in 1851, 1857, 1859, and 1860), we appear to have two distinct laws, one of which holds good in the winter months, and the other its direct converse in the summer months. Taking the entire year, these two laws appear very nearly to balance one another.

Mr. BAXENDELL, F.R.A.S., read a Paper "*On the Irregular Oscillations of the Barometer at Lisbon.*"

In this Paper the Author gives the results derived from a valuable series of barometrical observations made at Lisbon during the twelve years 1849-60, by John Martin, M.D., and kindly presented by him to the Section through Mr. Mosley. These observations were made daily at nine a.m., the barometer being permanently fixed at about fifty feet above the mean level of the Tagus, and the readings uniformly reduced to the temperature of 32° Fahrenheit.

From the tables which accompany the Paper it appears that at Lisbon the maximum amount of oscillation occurs in January, and the minimum in July, precisely as in the British Islands, and therefore agreeing with the law of disturbance announced by the Author in a former Paper. There is a small maximum in March, and a rather sudden and considerable increase of disturbance in October. The mean annual amount of oscillation is 31.686 inches, and the mean annual number of oscillations 156.9. The mean range and mean duration of oscillation are both greatest in winter, and least in the summer months. The greatest range occurs in January, but the greatest duration of oscillation is in November. It is remarkable, too, that the range is also greater in November than in any other month except January. These results for November are probably due to the great barometric wave which Mr. Birt some years ago pointed out as frequently occurring in or about this month.

In England the mean duration of oscillation is greater in the summer than in the winter half of the year, and, assuming that the breadth of a barometric wave is some function of the time taken to complete an oscillation, it follows that at Lisbon the areas of disturbance or breadths of the barometric waves are greatest in the winter and least in the summer months, whilst in England, on the contrary, they are greatest in the summer and least in the winter months. It seems probable, therefore, that at some intermediate latitude they are the same in both halves of the year, and this latitude may possibly be the nodal line separating a zone of high barometer from one of low barometer.

Comparing the six years of greatest amount of oscillation with the six years of least amount, it is shown that in years when the total amount of oscillation is above the average, the distribution of the increase in the different months is by no means in the proportion of the numbers representing the mean daily range of oscillation. The increase is much greater in the winter than in the summer months, but in April and September there is a diminution instead of an increase in the amount of oscillation. The total increase for the six winter months is 20.84 inches against 4.06 inches for the six summer months, while the total amounts of disturbance for the respective periods are in the ratio of 126.13 to 76.44. The Author adds, that the ratio of the amounts of increase in the winter and summer halves of the year appears to increase rapidly with increase of latitude; thus at Lisbon it is as 5 to 1; at Brussels as 6 to 1; and at Stockholm as $8\frac{1}{2}$ to 1.

Mr. ATKINSON exhibited a Chart, showing at one view

the barometric oscillations during the month of February last, as observed by him at Thelwall, and also the strength of the wind, corresponding to each observed altitude of the barometer.

Mr. Mosley read an extract from the *Gibraltar Chronicle* of the 11th February, 1861, giving details of a violent gale experienced at that place, and on the coast of Morocco, on the 9th of February. The barometer fell to 29.261, and it oscillated violently during the gale, the mercury being jerked up and down more than .100 inch. The range of the barometer between the 26th January and 10th February, was 1.240 inches, the range for the whole year 1860 having been only .865. The range for twenty-four hours, commencing 9th February, at eight a.m., was .625 inch. The storm appears to have been a cyclone moving on a southerly course. It was encountered off the coast of Morocco by the steamer *Egyptian*, veering from S.E. by S.S.W. to N.W. It was met with by the *Indus* mail steamer between Lisbon and Cape St. Vincent, on Saturday, the 9th February, and blew a hurricane from N. and N.E. It did not reach the coast of Morocco until eight o'clock on the following morning. As the occurrence of a cyclone on the Portuguese coast, moving in a southerly direction, is unprecedented, this gale will be further investigated.

MICROSCOPICAL SECTION.

February 18, 1861.

Letters were read from Captain Anderson, R.M.S. *Canada*, and from Dr. Wallich, respecting the pamphlet on "*Life in the Deep Seas*."

Mr. SIDEBOTHAM described his experience in mounting Desmidiæ, and the difficulty he found in discovering a suitable medium for their preservation. He had tried syrup, Goadby's fluid, and a number of other chemical preparations, but the specimens, in course of time, were spoiled from one cause or other; the fluid which has best withstood the effects of time is simple distilled water; the cells being made of gold size and Japan black. Mr. Sidebotham exhibited desmidiæ, mounted in distilled water, in the years 1842 to 1846, in which the chlorophyll is comparatively little altered.

Professor WILLIAMSON observed that Dr. Carpenter had mounted star-fishes in glycerine, and had found the colours well preserved. He himself had used a mixture of glycerine and distilled water for volvox, and had found it to answer well.

Mr. SIDEBOTHAM also exhibited specimens of Diatomaceæ, mounted in 1844. The specimens (*Isthmia enervis*, *Biddulphia*, &c.) were obtained fresh, immersed in spirits of wine to absorb the water, and mounted in balsam; the green colour of the cell contents is yet perfectly preserved.

Professor WILLIAMSON exhibited some scales of fish, prepared by Dr. Kölliker, of Würzburg, containing remarkable examples of fusiform lacunæ. He also pointed out how these and other similar discoveries, to which he referred, confirmed his previous conclusions in the *Philosophical Transactions*, viz., that fusiform lacunæ were not characteristic of reptilian bones, as some had supposed, but that they existed in many fishes; he especially referred to the salmonidæ as presenting this oblong form of bone corpuscle.

Mr. BROTHERS exhibited a modification of the kaloscope, and objects to illustrate the same.

Soundings were received from the steamers *Canada*, from New York; *Armenian*, coast of Africa; *Tagus*, from Lisbon, and from several vessels of war, from different parts of the world, which were duly acknowledged. Incrustations from the boilers of several sea-going steamers were also presented by Mr. W. A. Hayman, of Liverpool.

NOTICES OF PATENTS.

Obtaining Colouring Matter from the Lentisc or Mastic Tree.
By EDWARD T. HUGHES. Provisional Protection only.

THE Patentee takes the leaves or berries, and boils them in water. He then filters and precipitates the fluid with a solution of a salt of iron. He thus obtains a deep blue liquid, which will communicate to fabrics a good black dye, provided they are left in the solution while in a state of effervescence until a reaction takes place. The word effervescence is evidently a ludicrously feeble attempt at an English rendering of a French word for boiling. To understand this, it is necessary to mention that the patent is a communication from "Christopher Muratore, Merchant, of Algiers, Africa, Empire of France." The solution above mentioned, may be used instead of Campeachy wood, nut galls, or other tanning materials. It also gives a black dye to all articles which have been immersed in a solution of iron. To facilitate the carriage of the dye, it may be reduced to powder. The powder may also be used for painting by being mixed with oil, &c. The colouring matter of the mastic tree will produce other colours than black, according as it is mixed with other salts or acids. Iron yields yellowish colours; lead, white; copper, brown; mercury yellow. The branches or stems yield the same colouring matter, but not so much as the leaves or berries. "So that the whole of this tree, which is very plentiful, almost inexhaustible, in Africa, and very cheap, may be made to supersede Campeachy wood, gall nuts, and other expensive colouring matters." Without going so far as the patentee in his ideas of the usefulness of the lentisc tree as a source of colouring matters, it is at least probable that the patent will do good by directing attention to an abundant source of a useful astringent.

Improvements in the Production of Colours for Dyeing and Printing. By WILLIAM STEVENS SQUIRE. Provisional Protection only.

THE patent is for the use of peroxide of mercury in conjunction with a neutral salt of aniline, or its homologues, for the production of colours. The patentee takes an intimate mixture of a salt of aniline and peroxide of mercury, and heats them together until effervescence takes place; he then continues the heat for some time until the action is complete. He then dissolves the mass in spirit, and filters. This solution, the patentee states, may be used for dyeing, when diluted with hot water; but, in order to purify it, he precipitates the colouring matter from the alcoholic solution by means of water. The precipitate is then to be dissolved in alcohol, and diluted with hot water for dyeing purposes. When the salt of aniline is not easily fusible he adds paraffine oil or wax to the mixture, and heats as before, until the action is complete. The paraffine oil or wax is poured off from the mass containing the colouring matter previous to dissolving the latter in spirits. In making the mixture of the salt of aniline and peroxide of mercury, the patentee prefers freshly-precipitated peroxide, and adds it to a warm saturated solution of the salt of aniline, and evaporates the mixture to dryness.

This patent was evidently taken out under the idea that oxide of mercury and a salt of aniline would react like aniline itself and a salt of mercury. If it was intended to make Magenta by this process, it is pretty evident that the scheme has proved a failure. No processes of this kind yield products in the quantity, and of the brilliancy and purity, now required for commercial purposes. Moreover, even if the process described afforded a good colour, the method of purification is obviously insufficient to give a dye of the requisite solubility in cold water, and freedom from tarry impurities.

CORRESPONDENCE.

The Adulteration of Food.

To the Editor of the CHEMICAL NEWS.

SIR,—I am at a loss to conceive to what portions of my letters, published in Nos. 63 and 64 of your Journal, Mr. Scott alludes when he speaks of "virulent animadversions," nor am I aware in what respect I have lost sight of that courtesy "due to every individual from others of his species." If for public as well as scientific ends a fair and open discussion of the Adulteration question is invited by Mr. Scott, I cannot but think that he might have favoured us with fuller information on the points to which I more particularly called attention, and that his remarks generally might have been a little more explicit.

I have yet to learn that to write under an assumed name is in any way discreditable, unless such a course is adopted as a screen for false statements to the detriment of others, or for the use of language which the writer would be ashamed to avow openly; and I cannot think that to discuss a question of general interest anonymously is at all undesirable, inasmuch as in such a case the value of the communications necessarily depends upon the force of the arguments used and is not influenced by the name of the individual by whom they are advanced. If I have been so unfortunate as to select a "singularly inappropriate" *soubriquet* it is to be regretted; although a matter of no great consequence, and one which cannot have the slightest bearing on the discussion. Passing over Mr. Scott's remarks as to my "peculiar diction, overstrained criticisms, and puerile objections," as equally irrelevant to the subject, let me come at once to the points at issue.

Mr. Scott states that my enquiry whether "colours and essences of every description are to be henceforth decried," is answered by my own remark that "we must all agree that the admixture of poisonous matter in any shape with food cannot be too strictly prohibited." This can be only understood as an indirect affirmative, implying that Mr. Scott regards all colours and essences employed to please the eye or taste as poisonous, and would recommend the prohibition of their use. I cannot allow that the employment in any article of food of such substances as cochineal, turmeric, saffron, almond, orange flower, lemon, vanilla, and other essences of a similar character, can be considered as injurious; but at the same time all people of common-sense will admit that the practice of introducing acknowledged poisons, such as chrome yellow, Scheele's green, vermilion, and acetate of copper, into food of any description ought not to go unpunished.

As regards the adulteration of bread, the most important of the subjects treated of, and respecting which we are anxiously looking on all sides for such further details as may tend to throw some light upon the contradictory and conflicting evidence now before the public, Mr. Scott still withholds any information and has "only to repeat his original statement without the slightest qualification," informing us that "any one of competent analytical skill" can ascertain its truth for himself, if he will take the trouble to do so. If this is the case, how are we to reconcile with it the very opposite results obtained by Dr. Odling? When we consider the immense amount of bread consumed in London and that the number of bakers is more than two thousand four hundred, I think it can scarcely be using too strong language to term the statement made by Mr. Scott that 87 per cent. of all this vast quantity is more or less adulterated, a "sweeping assertion," for I very much question whether any one person has examined a sufficient number of samples to justify the publication of so positive a statement.

It would be interesting to know whether the "87 per cent." was obtained from the examination of samples from

any particular district, whether they were of a special quality of bread or procured from the lower class of tradesmen exclusively?

Mr. Scott alleges that the necessity of condensing his paper for the Society of Arts prevented him from entering into details, but it would have scarcely increased its length perceptibly had he put us in possession of some two or three important data. A very few words would have sufficed to have told us in what proportion of the 87 per cent. the adulterant was alum, and what was the average amount of this substance found in a quartern loaf. Admitting, however, that the time would not allow of this, why does he still refuse the desired information, now that he has ample opportunity of affording it? Mr. Scott states that "the simple tests for, and characteristics of, the presence of adulterants" named in his paper "are to be regarded solely as popular tests." Whilst it is very desirable that simple tests capable of being made by every one and which may serve as indications of the presence of foreign substances in our food should be made known to the public, it is at the same time of the utmost importance that such tests should be thoroughly reliable, and have no tendency to mislead; otherwise they can only serve to create unnecessary alarm. Now, I maintain that the process of testing bread for alum as described by Mr. Scott, would, in the hands of ordinary and unscientific operators, indicate its presence in bread made from pure flour. The testing of bread for alum by searching for alumina is not a process that can be performed, even so far as to give reliable indications of its presence (unless the quantity used has been very large), by any one unaccustomed to operations of this description. In following the directions given, the ordinary potash of the shops would naturally be employed, which every chemist knows almost invariably contains alumina—the very substance the presence or absence of which it is desired to ascertain. Moreover, the method given is incomplete, even were pure potash employed and the operation performed by a skilful chemist.

I also greatly question whether "brittleness of structure" is any indication of rice, or "crumbly texture" of potatoes, inasmuch as bread made from pure wheat frequently possesses these properties, while on the other hand, bread containing rice or potatoes certainly does not always exhibit either of the indications alluded to.

The question of brewers' grains in oats, and the danger to horses arising from adulterated rape-cake, need not be further noticed, since it seems that the former is a deception that can only be practised on the blind, and the latter appears to have arisen from an unsuccessful attempt to feed a horse upon an unusual description of diet.

Mr. Scott appears to be somewhat indignant at my conceiving it possible he could have pursued a course so far beneath his "mission in the world" as to have made the authorities aware of his discovery of the common practice of poisoning meat with arsenic. Are we to suppose that Mr. Scott would quietly look on while a robbery was being committed, or that he would consider himself a "common informer" if he communicated the fact to the police? I think I shall express the opinion of most of your readers in holding it to be our duty when we discover the perpetration of any crime against society, to do all in our power to put a stop to it, and one would scarcely expect, in pursuing the most effectual and straightforward course towards this end, that we should be regarded as having acted in a manner derogatory to the character of a gentleman.

The fact that Mr. Scott has examined 1127 samples of pickles and several thousand samples of tea, shows that he must have paid great attention to his subject, and the evidence he could give would doubtless be both interesting and instructive, but his estimation that 91 per cent. of all pickles are adulterated, is of no practical value unless we know what he means by the terms "good and pure." If

he considers pickles to be adulterated, to which any substance (although perfectly harmless) has been added to improve the colour or flavour, then we can scarcely be surprised at his finding so large a proportion of adulterated samples, but I apprehend that the public only care to know what proportion contain poisonous or injurious ingredients.

However numerous may be the samples of tea he has examined, yet his results afford us no criterion of the extent of the mischief against which we wish to guard ourselves without some information as to the nature and amount of the adulterants employed. If other leaves are substituted for those of the tea-plant, what are they? Is their nature positively injurious, or is it simply that they do not possess the valuable qualities of the tea-plant?

Mr. Scott asks "What kind of authority is one of the largest bakers in Vienna upon the question of the purity of bread?" and speaks of the statements of tea-dealers as *ex parte*. Now, I think it will be admitted that the interest of respectable dealers, so far from lying in "exactly the opposite direction to that in which truth is to be found," consists in giving every information calculated to put a stop to those frauds practised by unprincipled men, who are thereby enabled to undersell them in the market. If the evidence of no person in trade is to be considered as reliable, how comes it that Mr. Scott quotes Messrs. Huntley and Palmer as to the purity of their own biscuits? Why does he consult Messrs. Phillips on the question of tea? and why does he speak of Messrs. Crosbe and Blackwell as "no mean authority" on the question of pickles? It is difficult to understand what Mr. Scott considers as a "pure and wholesome" food; but judging from his proposal to prohibit altogether the sale (retail) of the whole of the "brown sugars of commerce, except the crystalline varieties," we have every reason to believe that his ideas on this subject are not of a very practical character.

In conclusion, it is perhaps scarcely necessary to disclaim any intention on my part of making the discussion of this question in your columns a matter of personal controversy, my sole object being to prevent the publication of what I conceive to be mistaken views from passing unnoticed, and thus allowing it to be assumed that they have met with the approval and concurrence of all your readers.—I am, &c.

VERAX.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Wood's Fusible Metal.—Lipowitz has made some experiments on the cadmium-alloy, described by Dr. Wood (see CHEMICAL NEWS, vol. ii., p. 257). He found that an alloy composed of 8 parts lead, 15 parts bismuth, 4 parts tin, and 3 parts cadmium, possessed the following properties:—It is permanently silver white, and has a brilliant metallic lustre; it is not so brittle or hard but that it may be obtained in thin leaves or flexible plates; it has a fine-grained fracture, and may be filed without stopping up the file. In dry air it keeps its polish. It expands in cooling, but not so much as bismuth or antimony. Its specific gravity is from 9.4 to 9.41. It softens between 131° and 140° Fahr., and near 140° becomes perfectly fluid. No change in the condition of the metallic mass was observed on remelting after rapidly cooling the alloy. The above properties show that the alloy may be applied to some useful purposes. It may supersede all the quicksilver alloys for stopping teeth: it may be used as a solder whenever the metals soldered are not likely to be exposed to heat. Tin, lead, and Britannia-metal may be soldered together under water above 160° Fahr. Zinc, iron, copper and brass may also be soldered with the greatest ease under water, to which a little hydrochloric acid has been previously added. The alloy is so easily

fusible that it may be melted on a piece of paper over a spirit lamp. In the preparation of the alloy the author recommends the use of the purest bismuth.—*Dingler's Polytech. Journ.*, Bd. clviii. s. 271 and 376.

II. ORGANIC CHEMISTRY.

The Spontaneous Decomposition of Alloxan.—W. Heintz publishes (*Poggendorff's Annalen*, Bd. cxl. s. 436 and cxli. s. 79) a long paper on the nature of the changes which take place in the spontaneous decomposition of alloxan. Gregory first observed that some crystallised alloxan, which had been two or three years in a bottle, had become changed into a fluid and some crystals which no longer possessed the properties of alloxan. Heintz observed a similar change in some alloxan which had been prepared twelve years, but this still produced some of the reactions of alloxan. It caused a deep blue colour with sulphate of iron, when a drop or two of dilute ammonia was added, showing that alloxan was still present with some acid. With a specimen, however, which had been made fifteen or twenty years, the author could produce none of the reactions of alloxan, but found that it possessed the following properties:—It gave a yellow colour with sulphate of iron, and on the addition of ammonia only a reddish brown precipitate. It did not reduce a silver solution. The warm solution gave no purple red with ammonia. Baryta water gave a white precipitate, and the liquor smelt of ammonia. Chloride of calcium gave a precipitate which did not dissolve in acetic acid—it was, in fact, oxalate of lime, showing the presence of oxalic acid, and the absence of alloxan and alloxantin. Further researches proved that the alloxan had become converted into oxalic and oxaluric acids and urea, the latter forming the largest proportion. Baumert had found alloxantin, carbonic acid, and parabanic acid in a specimen of decomposed alloxan which he examined, and Heintz, comparing his own results with these, explains the decomposition which takes place to be as follows:—First. The crystallised alloxan breaks up into carbonic acid, alloxantin and parabanic acid, as Baumert has described. The latter acid and part of the alloxantin dissolve in the water of crystallisation of the alloxan. In this solution the alloxantin takes slowly oxygen from the air, whereby alloxan is again formed, to be again decomposed as before, the process continuing until all the alloxan and alloxantin is converted into parabanic acid. This acid then slowly takes up water, and becomes changed into oxaluric acid, which easily passes into oxalic acid and urea.

Products of the Oxidation of Fats and Sebatic Acid.—Arppe denies (*Annal. der Chem. und Pharm.* Bd. cxv. s. 57) the existence of azelaic, adipic, lipic, and leparglyc acids, described by Laurent and Wirtz as among the products of the oxidation of oleic acid. These bodies, the author says, are only mixtures. He has separated suberic and succinic acids, a new acid something like the oxypyrolic previously described by the author, and pimelic acid.

III. ANALYTICAL CHEMISTRY.

Estimation of Iodine and Bromine in Mixture.—Reiman shows (*Annal. der Chem. und Pharm.* Bd. cxv. s. 140.) that iodine and bromine may be estimated when existing together in solution by means of a solution of chlorine of known strength. If a solution containing both the iodide and bromide of potassium be shaken with a standard solution of chlorine, and a small quantity of chloroform, the chloroform becomes of a blue, or if the solution of the salts be very weak, a rose colour. On continuing to add chlorine water, the colouration disappears when six equivalents of chlorine have been added to one of the iodide. Then, if the solution contains bromide, on a further addition of chlorine a new reaction takes place,

and the chloroform becomes yellow, then orange, afterwards again yellow, and finally a yellowish white, when two equivalents of chlorine have been added to one of bromine. The bottle must be placed on a sheet of white paper to see these changes clearly. On a further addition of chlorine, the chloroform becomes colourless on the formation of the pentachloride of bromine. Founded on these reactions, the author has constructed a formula for making an approximate quantitative estimation of the iodine and bromine, but the process is evidently of but little practical use. The author explains that a bromine reaction is finished when the chloroform has the colour of a weak solution of chromate of potash. If the liquid tested contains organic matter, it must be got rid of by calcination with caustic soda.

Molybdenate of Ammonia a Test for Sulphur.—Schlossberger states (*Zeitschrift für Chem. und Pharm.* 1860. s. 423.) that a dilute solution of molybdenate of ammonia supersaturated with hydrochloric acid, gives a beautiful blue colour with a solution of sulphuretted hydrogen or the sulphide of a metal. This test, he says, is more delicate than the nitro-prusside of sodium.

IV. TECHNICAL CHEMISTRY.

Mineral Green Free from Arsenic.—Struve (*Archiv d. Pharm.* Bd. cliv. s. 42) gives an analysis of this colour which has been in use in Germany for the last three years as a substitute for Schweinfurt green. Its composition was as follows:—

Chromate of lead	13.65
Basic carbonate of copper	80.24
Oxide of iron	0.77
Carbonate of lime	2.65
Moisture	2.58

99.89

This colour, although it has not all the beauty of the Schweinfurt green, is recommended as being free from arsenic; but it is not altogether harmless, and therefore must not be used in confectionery or the like.

MISCELLANEOUS.

Shipment of Gas from America to London.

We saw yesterday at the passenger station of the Pennsylvania Railroad, at Eleventh and Market-street, a number of wrought iron cylindrical vessels, which, we were informed, contained compressed gas. Upon further inquiry we learned that Mr. Williams, who had fitted up the cars on the Pennsylvania Central Railroad for burning gas, had also taken a contract from Hathaway and Leach, of this city, for fitting out a number of cars to be used on the street railways in London. These cylinders had been sent to the railroad workshops in Altoona, to be filled with gas by means of the company's apparatus there; so that before long we may expect to hear of American street railway cars in London, lighted with gas made at the foot of the Alleghany mountains.—*Philadelphia Gazette.*

Great Salt Lake.—Away out in the Western wilds, some three hundred miles beyond the Rocky Mountains, and amidst other and loftier mountains still, there exists one of the most remarkable natural curiosities in the world—the Great Salt Lake. All the streams and rivers which run into Salt Lake have their sources in the Great Basin, and what is remarkable, none of them find their way out of it. There are no outlets to the great lake. It receives the waters of several large rivers, swollen annually by their mountain tributaries, but in no very extensive degree are the waters of the lake increased during the seasons of the most copious flowing of these rivers. Great Salt Lake, according to the United States Government

survey, which was completed in 1850, is two hundred and ninety-one miles in circumference on the shore line. The water of the lake is saltier than any other upon the face of the earth. Persons engaged in boiling salt on the shores of the lake say that three buckets of the water dipped out of the lake and boiled in an open wooden trough, with a sheet iron bottom, will yield one bucket of salt; or, in other words, that it is one-third salt. The analysis of the water made under the United States survey in 1850 says that the water contains more than twenty per cent. of pure chloride of sodium, and about two per cent. of other salts, making one of the purest and most concentrated brines known to the world. The specific gravity of the water is very great; this in the same analysis is given at 1.170, water being 1000. The water is so heavy or buoyant that a person bathing in it can sit upright, with head, shoulders, and arms out of the water, like sitting on a rocking chair; and a person can lie on the surface with head, hands, and feet out of the water. In the lake the colour of the water is a very deep dark blue, much more so than the ocean, but when taken in the hand it is transparent.

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the CHEMICAL NEWS, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 6d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part I. of the present Volume is now ready.

*. All Editorial Communications are to be addressed to Mr. CROOKES, and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

*. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Vol. II. of the CHEMICAL NEWS, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

Erratum.—It was obviously a slip of the pen on the part of the author.

Querist.—We will enquire into the matter, meantime you will be quite safe in denying it. It is a disgraceful trade puff.

Wm. Fox.—We do not think it is yet published, but will enquire.

Shop-keeper.—Heat it with water containing a bichromate of potash and sulphuric acid sufficient to colour it an orange yellow.

W. Flight.—Received. Apply at a second-hand bookseller's.

Apollo.—Try lard and cod-liver oil properly scented.

X. Y. Z.—"The Giessen Outlines of Chemical Analysis," and "Fowles' Chemistry," are the best books you can have for your purpose. Study Dr. Frankland's Lectures, which we are now publishing, and spend as much time as possible in experimenting and taking notes in the laboratory.

G. F. R.—J. A. D.—We regret that we must again decline our correspondents' communications. There are too many demands upon our space to allow of its being filled with purely theoretical speculations, unsupported by experiments.

W. L. C.—1. Apply to Messrs. Horne and Thornthwaite, Newgate Street, or to Casella, Hatton Garden. 2. Chromate of lime is a definite compound; it may be prepared by mixing monochromate of potash with chloride of calcium, when it falls down as a pale yellow precipitate.

K., Cost of Oxygen Prepared by Different Methods.—Oxygen prepared from chlorate of potash, costs per cubic metre 10 francs. Prepared from binoxide of manganese, it costs 4 francs 87 cents for the same quantity. From sulphuric acid (Deville's method), the cost is not more than 1 franc, supposing the sulphurous acid produced to be valueless. If the latter be utilised in any way, the price would be still further reduced. We presume you must get the apparatus made to order.

THE CHEMICAL NEWS.

VOL. III. No. 69.—March 30, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Existence of a New Element, probably of the Sulphur Group, by WILLIAM CROOKES.

In the year 1850 Professor Hofmann placed at my disposal upwards of ten pounds of the seleniferous deposit from the sulphuric acid manufactory at Tilkrode, in the Hartz Mountains, for the purpose of extracting from it the selenium, which was afterwards employed in an investigation upon the selenocyanides.¹ Some residues which were left in the purification of the crude selenium, and which, from their reactions, appeared to contain tellurium, were collected together and placed aside for examination at a more convenient opportunity. They remained unnoticed until the beginning of the present year, when, requiring some tellurium for experimental purposes, I attempted its extraction from these residues. Knowing that the spectra of the incandescent vapours of both selenium and tellurium were free from any strongly-marked lines which might lead to the identification of either of these elements, it was not until I had in vain tried numerous chemical methods for isolating the tellurium which I supposed to be present, that the method of spectrum analysis was used. A portion of the residue introduced into a gas-flame gave abundant evidence of selenium; but as the alternate light and dark bands due to this element became fainter, and I was expecting the appearance of the somewhat similar but closer bands of tellurium, suddenly a *bright-green line* flashed into view and quickly disappeared. An isolated green line in this portion of the spectrum was new to me. I had become intimately acquainted with the appearance of most of the artificial spectra during many years' investigation, and had never before met with a similar line to this; and as, from the chemical processes through which this residue had passed, the elements which could possibly be present were limited to a few, it became of interest to discover which of them occasioned this green line.

After numerous experiments, I have been led to the conclusion, that it is caused by the presence of a new element belonging to the sulphur group; but, unfortunately, the quantity of material upon which I have been able to experiment has been so small, that I hesitate to assert this very positively. I am, however, at work upon some of the seleniferous deposit itself, and hope shortly to be able to speak more confidently upon this point, as well as to give some account of its properties.

In the purest state that I have as yet succeeded in obtaining this substance, it communicates as definite a reaction to the flame as soda. The smallest trace introduced into the burner of the spectrum apparatus giving

rise to a brilliant green line, perfectly sharp and well defined, upon a black ground, and almost rivalling the Na line in brilliancy. It is not, however, very lasting: owing to its volatility, which is almost as great as that of selenium, a portion introduced at once into a flame merely shows the line as a brilliant flash, remaining only a fraction of a second; but if it be introduced into the flame gradually, the line continues present for a much longer time.

The properties of the substance both in solution and in the dry state, as nearly as I can make out from the small quantity at my disposal, are the following:—

1. It is completely volatile below a red heat both in the elementary state and in combination (except when united with a heavy fixed metal).
2. From its hydrochloric solution, it is readily precipitated by metallic zinc in the form of a heavy black powder, insoluble in the acid liquid.
3. Ammonia added very gradually until in slight excess to its acid solution gives no precipitate or colouration whatever, neither does the addition of carbonate or oxalate of ammonia to this alkaline solution.
4. Dry chlorine passed over it at a dull red heat unites with it, forming a readily volatile chloride soluble in water.
5. Sulphuretted hydrogen passed through its hydrochloric solution precipitates it incompletely, unless only a trace of free acid is present; but in an alkaline solution an immediate precipitation of a heavy black powder takes place.
6. Fused with nitre and carbonate of soda it becomes soluble in water, hydrochloric acid added in excess to this liquid producing a solution which answers to the above tests 2, 3, and 5.

An examination of these reactions shows that there are very few elements which could by the remotest possibility be mistaken for it.

The accompanying list includes every element, with the exception of the gases, bromine, iodine, and carbon. Opposite the name of each I have placed the number of the reaction which eliminates it from the list of possible substances, taking great care, in every case, to give the benefit of any doubt which might arise, on account of an imperfectly known or doubtful reaction, in favour of the opposite opinion which I desire to prove, and in cases where several reactions would prove the same thing only making use of the most trustworthy.

1, 5,	Aluminium.	1, 5,	Didymium.
	Antimony.	1, 5,	Erbium.
	Arsenic.	1,	Gold.
2, 3, 5,	Barium.	1,	Ilmenium.
2, 3, 5,	Beryllium.	1,	Iridium.
1,	Bismuth.	1,	Iron.
1, 2, 5,	Boron.	1, 5,	Lanthanium.
6,	Cadmium.	1,	Lead.
2, 5,	Cæsium.	2, 5,	Lithium.
2, 3, 5,	Calcium.	2, 5,	Magnesium.
1, 5,	Cerium.	1,	Manganese.
1,	Chromium.	3, 6,	Mercury.
1, †	Cobalt.	1,	Molybdenum.
1,	Copper.	1,	Nickel.

¹ Quarterly Journal of the Chemical Society, iv. 12, and "Gmelin's Handbook" (Cavendish Society's translation), viii. 122.

unexpected use to which iridium can be put; and it is likely, that ere long we shall learn other applications of it.

But there are reasons why the price of platinum is maintained so high as greatly to limit its application; these reasons are,—first, the rarity of platinum ore, and then the expense of extracting it. As to the first objection, Russia (which yields most platinum) contains a quantity of this metal, the amount of which is not yet estimated. With the increasing demand for this metal, careful researches will multiply the number of known platiniferous localities, and will cause a reduction in its price. As to the expense of washing platiniferous sands, it can be notably diminished by establishing new and less imperfect machines than those at present in operation. The chemico-metallurgic question having been resolved in so brilliant a manner by the French chemists, the Russian technologists and the members of our mining corps should aid in perfecting the mechanical appliances.

For the application of platinum for divers purposes the purely chemical study of its properties and the accompanying metals is not less important than the metallurgic question. The great Swedish chemist, Berzelius, who, at the suggestion of the Russian Government, occupied himself with this question, treated it with great ability. He succeeded, in fact, in a comparatively short time, and with rare skill, in creating a completely new science in respect of these metals, in spite of the little material available for his researches. His very exact works form the only basis of all subsequent researches. Nevertheless, the modesty of this great chemist made him add, at the end of his long paper:—"My experiments have thus elucidated this question. However, they can be regarded only as the first outline of the history of the metals associated with platinum, and also of those which may yet be met with,—a history whose development is as extended as difficult." The French chemists, besides their fortunate solution of the metallurgic question, are also occupied with the exclusively chemical one, and have set forth, in this respect, many important data.

Their intention is, in the first place, to obtain general results by comparing these metals with others; they admit that the six known platiniferous metals, from a certain point of view, may be rightly considered as forming a separate and well-defined group. According to this view osmium, so remarkable for its properties, would be the metalloid of the platinum series, a consideration which, though very suggestive, appears illusive when the question is examined more closely. On the other hand, each metal of the platinum series is allied by certain analogies with some metal of another series. Thus platinum is allied to gold and iridium, palladium to silver, ruthenium to tin. We cannot deny the existence of these analogies, nevertheless I think I am justified in attaching no great importance to them, and I am very decidedly in favour of the first part of our author's account; in fact, as I have always expressed myself on this subject, I consider the platiniferous metals as forming an isolated metallic group, inseparable, and solidly constituted. Nature itself, however, gives indications which furnish us with the means of comparing them with the other metals. In fact, in natural platinum mines, as well as in all those which generally contain platinum, osmium, and iridium, we constantly find iron, manganese, copper, and often even gold. It cannot be imagined that these metals, so constantly associated together in platinum mines,

should come there in an accidental manner, but form alloys according to certain relations of analogy. These analogical relations, of which I shall have occasion to speak by-and-by, do undoubtedly exist. It has been proved that osmide of iridium is not, as was at first believed, a chemical combination of equal parts of osmium and iridium, by equivalent, but rather a platinum ore, in which the metals generally found in soluble platinum ores exist in excess: thus, in the osmide of iridium, either osmium or iridium predominates, while it is platinum in the soluble ore. There is yet a third platinum ore, *irite*, which my friend M. Herrmann, of Moscow, has studied, and which, according to his analysis, is found to be a combination of oxidised platiniferous metals with the oxides of iron and chromium. Nevertheless, I think myself justified in doubting the existence of this ore, not only because such a combination does not appear likely to take place, but because that so complicated a mixture as the platinum residuum should, by so simple a process, eliminate a pure mineral. In short, by exactly following M. Herrmann's method, I have obtained a substance in spangled powder possessing the properties which M. Herrmann attributes to *irite*, and which, upon close examination, proves to be a very complex mixture, the greatest part of which consists of fine spangles of osmide of iridium and of chrome iron ore.

In my assays of 1854 I attempted the grouping of the platiniferous metals. Having since enlarged my experience on this subject, I again undertake this labour, with certain modifications.

(To be continued.)

On a Series of New Combinations of Ammonia, Picric Acid, and Metallic Bases,¹ by M. CAREY LEA.

IN a paper published in this Journal, vol. xxvi. p. 379, for 1858, I described two compounds of picric acid and ammonia with copper and nickel respectively, and mentioned their instability and the great difficulty of obtaining them in a state of tolerable purity. Since then I have found that many other metals form compounds of a similar nature, and have succeeded in obtaining several of them sufficiently pure for approximate analysis. The results obtained are given below. Other investigations are in progress, and I hope to communicate them hereafter.

When a metallic salt is precipitated by ammonia, and a large excess of the precipitant added, a more or less complete solution of the precipitate is frequently obtained, especially if a considerable quantity of ammoniacal salt be present. I have found that these ammoniacal solutions, when treated with an alkaline picrate, for the most part yield an immediate precipitate, containing the elements of a metallic picrate united with those of ammonia. These precipitates are often very beautiful; they are generally yellow or yellowish, somewhat soluble in the mother liquid, or in strong ammoniacal solutions, by aid of heat, nearly insoluble in water, and decomposed by it very readily, especially if it be present in quantity, or heat be applied. Thrown on a filter and washed, the picric acid, ammonia, and some portion of undecomposed salt in solution pass through, while the greater part of the metallic oxide remains on the filter, and with sufficient washing is perfectly freed from picric acid and ammonia, and presents the appearance of a pure hydrate.

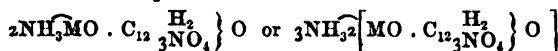
¹ American Journal of Science and Art.

The analyses of these substances were made in the following manner:—It was found desirable that before the ammonia and metallic oxide were determined, the whole of the picric acid should first be removed. This was effected by adding chlorhydric acid to a weighed quantity of the substance, carefully dried, and evaporating to dryness at a gentle heat. Ether, which must be anhydrous, or nearly so, was added in the proportion of 15 to 18 grammes to 6 or 7 grammes of the salt used, detaching the dry crust and breaking it up to powder with a glass rod, that every part might be brought thoroughly in contact with the ether. After standing ten minutes, the ether was poured off, a fresh quantity added, and a large beaker was inverted over that which contained the substance undergoing analysis. After a lapse of three or four hours, the liquid was poured off, a third, and, if necessary, a fourth quantity added in the same way. The decantation is effected with the greatest ease, as, if the ether is sufficiently strong, the chlorides remain nearly or quite dry at the bottom. It was found that by this means the picric acid could be separated with sufficient exactness to afford a tolerably correct estimation of its quantity, a careful trial with a weighed quantity of perfectly pure picric acid showed that the result fell a little under the truth by reason of a small loss arising from minute portions of the acid remaining undissolved. On the other hand, no chlorides are dissolved when the operation is properly performed; the ethereal solution evaporated to dryness, and the residue dissolved in water, was not troubled by solution of nitrate of silver.

The operation requires nice manipulation, otherwise the loss of picric acid is considerable, and if the ether be not sufficiently anhydrous, portions of the chloride may be removed by it.

In this manner the quantity of picric acid was determined, and from it was calculated the quantity of the group $C_{12}H_2N_3O_{14}$, or picric acid minus the equivalent of hydrogen, which is replaceable by a metallic oxide; in other words, the group which, by union with a base, forms a picrate, or, by union with hydrogen, forms picric acid. From the residue undissolved by the ether, the ammonia was determined, either by distillation with solution of caustic soda, or by precipitation with bichloride of platinum. From another portion the metallic oxide was found by appropriate means.

As far as examined, the ammonia picrates all contain more than one equivalent of ammonia, and are represented either by the formula:—



Ammonia-picrate of Silver.

Nitrate of silver, dissolved in water, and treated first with excess of ammonia, and the resulting clear solution mixed with cold saturated solution of picrate of ammonia, gives immediately an abundant, highly-crystalline, light yellow precipitate of ammonia-picrate of silver, which is to be thrown upon a filter, thoroughly washed with dilute ammonia, and dried *in vacuo* over sulphuric acid.

It is to be observed that in the formation of this, as well as of all the other salts here described, the metallic solution must invariably be in excess, to check as far as possible the precipitation of picrate of ammonia together with the compound sought. I have shown in a paper intended to be read at the same time as this,¹ that if an alkaline picrate be poured into an alkaline solution, a

precipitate of alkaline picrate takes place. When the alkaline solution is strong the precipitate is immediate, and in the ammoniacal solutions used in the preparation of these salts any excess of alkaline picrate would fall with the ammonia-picrate. Even with every precaution, it is difficult, and in some cases apparently impossible, to obtain the ammonia-picrate perfectly free from this impurity.

(To be continued.)

On the Incineration of Filters.

THE great difficulty with which, in many cases, the last portions of the carbon of a filter are consumed when ignited in a porcelain crucible, is a fact well known to analysts. It does not appear, however, that the following simple method of obviating the difficulty—as practised in the laboratory of Professor Scheerer, in Freiberg—has ever received the publicity which it deserves.

Whenever a filter upon which a substance capable of injuring platinum has been collected has to be incinerated, the porcelain crucible or capsule in which the process is to be conducted should be placed within a vessel of platinum of similar form, and the whole ignited in the usual way.

Whether the greatly accelerated rapidity of combustion of the carbon which ensues depends upon a more equable distribution of heat brought about by the greater conducting power of the metal—an explanation which is current for the somewhat analogous case of copper-coated glass flasks, or whether, as seems probable, the power of the porcelain vessel to absorb heat be really increased by the interposition of the platinum; whether both these causes be of influence, or the result depend upon another less apparent reason, or, finally, whether vessels of some other metal would not be preferable to those of platinum, are questions which we do not propose to discuss in this connection. We desire only to give currency to the fact to which attention has been called, feeling assured that the chemist who has once employed this method to destroy a refractory filter will ever afterwards bear it in mind.—*American Journal of Science and Art.*

TECHNICAL CHEMISTRY.

On the Relations of certain Yellow Vegetable Dyes, by JOHN GALLETT.

PROFESSOR BOLLEY has recently published a paper² to show that Hlasiwetz was right in regarding the colouring matter of Persian berries as identical with that of quercitrin bark. Hlasiwetz assumes their identity mainly from their similar elementary composition, having only examined quercitrin; while Professor Bolley, at a former period, has made an examination of quercitrin, and has now undertaken an investigation of Persian berries, with the special object in view of removing any discrepancies in the way of these colouring matters being considered identical. I wonder, then, that he has furnished no explanation of the very considerable differences which exist between the published accounts of these bodies.

¹ Read before the Chemical Society, and published in the *Quarterly Journal of the Chemical Society*, January, 1861. See also an abstract in *CHEMICAL NEWS*, December 8, 1860.

² See *American Journal of Science and Art*, p. 75.

Hlasiwetz has made many valuable observations on various glucosides, and, amongst others, the important fact that when several of them are decomposed, by boiling with a little dilute sulphuric acid, they yield, besides the glucose, a body which, on heating with potash, gives phloroglucine; and this phloroglucine has the same composition, whether obtained from phloridzin or any variety of quercitrin; and when it is known that phloridzin is obtained from several species, and quercitrin from plants very distantly related, the subject appears well worthy of attention.

Hlasiwetz regards as a variety of quercitrin a body which yields glucose and quercitrin, in whatever proportions these are obtained from it. Thus, one variety may contain only one equivalent of glucose, others two or three equivalents, combined with quercetin, the quercetin not being subject to any variation. It is, therefore, perhaps of little use to point out that xanthorhamnin contains, when dried at the same temperature, one-half per cent. more hydrogen, and one per cent. less carbon than the quercetin of which the analyses are given in Hlasiwetz's paper. It would appear also that quercitrin does not lose its water of crystallisation at temperatures corresponding with xanthorhamnin.

Professor Bolley has not remarked that Rigaud found quercitrin to be nearly insoluble in cold, and sparingly soluble in boiling water, while xanthorhamnin dissolves in cold water in large quantity, and in boiling water so abundantly that it is difficult to make a saturated solution.

The proportion of sugar which xanthorhamnin yields is very different from that of any variety of quercitrin, thus:—

	First kind. Hlasiwetz.	Second kind. Rigaud.	Third kind. Kawaller.
Glucose	27.87	44.35	56.3 per cent.
Quercetin	—	61.40	— "

Xanthorhamnin yields:—

Glucose	68 per cent.
Rhamnetin	41 "

Supposing quercetin and rhamnetin the same, it would require about four atoms of glucose to one of rhamnetin, and then the calculation for the elementary composition would not correspond with the analyses so closely as the closely-agreeing numbers obtained on analysis require, while the water of crystallisation would be fully twenty atoms,—an unusually large number for this class of substances.

Quercetin and rhamnetin agree well enough in elementary composition, but not in properties, the former dissolving readily in alcohol, giving small, lemon yellow crystals; while, on the other hand, rhamnetin is almost insoluble, and gives at most a little flocculent matter, which the microscope alone could resolve into crystals.

Altogether, the bodies are quite distinct, as far as they have yet been investigated. Elementary composition cannot be considered of much value in identifying bodies of this class, as they mostly have very closely-corresponding compositions.

Professor Bolley remarks that Hlasiwetz regards chrysorhamnin and rhamnetin as identical. I did not observe this in reading over Hlasiwetz's paper, but Professor Bolley seems to regard it as accurate. If this were the case the unripe berries would contain rhamnetin, which, as they ripened, combined with glucose, and became converted into xanthorhamnin, which can again yield the rhamnin by boiling with dilute acid. So important a conclusion has as yet no foundation what-

ever, and is very improbable. In fact, the chrysorhamnin got by Kane, which he converted, by boiling for a few minutes in water, into xanthorhamnin, and then analysed the xanthorhamnin, corresponds not in the least with the crystals obtained by Professor Bolley with crude ether. Kane could not possibly make the mistakes necessary for Bolley's substance to be identical with his. It is a pity crude ether was used by Professor Bolley, as this might be a mixture very difficult to obtain again exactly the same, and as the result differs from Kane's and my own, it is hard to say what the particular substance Professor Bolley obtained might be.

On the Action of Carbonate of Soda on Cast Iron.¹

C. TISSIER, director of the Aluminium Works, at Amfreville, has published in the *Technologiste* (p. 357), some interesting experiments on this subject, an abstract of which we now make from *Dingler's Polytechnisches Journal*, cxlvi. p. 118 (1857). Tissier's attention was first drawn to the fact that the mixture of carbonate of soda, chalk, and charcoal used in the manufacture of sodium,—notwithstanding the great excess of carbon it contains—does not affect the malleability of the iron of the retorts which are employed. He found, further, when a piece of cast iron was submitted to a high temperature, with this mixture, that it was first converted into steel, and finally into malleable iron. This led him to try the effect of the action of carbonate of soda alone on both malleable and cast iron—malleable iron thus treated remained unchanged, while the soda salt extracted the carbon and silicium from the cast iron, thereby converting it into infusible malleable iron.

If a specimen of pig-iron (that experimented on contained 6.6 per cent. of silicium and free carbon) be exposed for several hours at a red heat, in a crucible containing an excess of carbonate of soda, the following reactions may be observed. As soon as the temperature is sufficiently elevated, the mass commences to rise, and large bubbles of carbonic oxide are given off, burning with a yellow flame. If after the evolution of carbonic oxide has ceased the fire is allowed to go down, and the iron taken out with tongs and freed from adhering salts by means of a hammer and by water, the metal exhibits a completely etched surface, although the form of the mass is unaltered—it may be drawn out under the hammer and forged either hot or cold, and the granular fracture of the cast iron is replaced by a fibrous crystalline texture—the mass is porous, the little cavities being filled with white silicate of soda, formed from the silicium contained in the metal. The iron thus obtained is scarcely attacked at all by chlorhydric acid in the cold, and but slowly even when heated. Dilute nitric acid acts upon it with energy, but still not so rapidly as in the case of ordinary malleable or cast iron.

Tissier considers it conceivable that this action of carbonate of soda not only extracts the carbon and silicium, but also must remove the phosphorus and sulphur contained in the iron. It is further possible that the iron takes up a portion of sodium, which does not injure it, but on the contrary the metal acquires desirable properties, as evinced by the fact of the dealers being very willing to purchase the worn out retorts from the sodium works. If sodium be not thus taken up, we must assume that for every equivalent of carbonic oxide an equiva-

¹ *American Journal of Science and Art.*

lent amount of anhydrous soda is formed. Tissier suggests that in the process of annealing or the conversion of cast iron into malleable iron, now accomplished by packing and heating for a long period with substances rich in oxide of iron, it may be possible to substitute fusion with carbonate of soda, it possessing the advantage that the metal can be removed from time to time to watch the progress of the conversion into steel or malleable iron.

The author hoped by this method to convert large masses of cast-iron into malleable metal, such as heretofore could only be obtained by forging, but the length of time required for the conversion of a mass of any considerable thickness, and the porosity of the iron obtained, present practical difficulties requiring some modification to overcome them. With smaller castings, however, the action, even when superficial, imparts to them great toughness, so that they no longer are liable to fracture. The amount of carbonate of soda required, when properly used, is inconsiderable, but it should be pure; or, if the commercial article be used, it must first be heated with carbon in order to reduce the sulphate of soda to sulphide of sodium, as the alkaline sulphates have a powerful action on iron at a red-heat.

On a New Re-agent for Aniline, by M. CH. MÈNE.

THE only re-agent hitherto employed for the detection of aniline has been hypochlorite of lime, which instantaneously imparts to it a deep violet colour. On account of the dyes which are produced by aniline, I am led to believe that the following reaction which accurately characterises this body may be utilised. When nitrous gas is passed into aniline, either anhydrous or dissolved in cold alcohol, the aniline assumes a yellow-brown tinge. If, then, nitric, sulphuric, hydrochloric, or oxalic acid, &c., is added to it, a magnificent red colour, very soluble, is developed. A large quantity of water changes it into yellow; a drop of acid restores its colour. It dyes silk, cotton, &c., perfectly. In employing Hofmann's method for detecting benzene by the aniline and chloride of lime method, nitrous gas and an acid will also serve to find this body. As this red body crystallises very definitely, it will be thoroughly investigated in my laboratory.—*Comptes-Rendus*, vol. lii.

PHYSICAL SCIENCE.

On the Prismatic Decomposition of the Electric, Voltaic, and Electro-Magnetic Sparks, by CHARLES WHEATSTONE, F.R.S., Professor of Experimental Philosophy in King's College, London.

(Read August 12, 1835, at the Meeting of the British Association, held at Dublin.)

§ I.

FRAUNHOFER examined the ordinary electric spark with his prismatic telescope, and found that it presented a perfect spectrum crossed by numerous bright lines, as if certain definite rays were exalted in intensity, thus essentially differing from that of the solar light, which he ascertained was traversed by innumerable dark lines, indicating the absence of so many definite rays. The brilliant lines in the prismatic spectrum of the electric spark seem to have been but cursorily observed by this eminent optician; he gives no measurements of the lines, and confines himself to a simple statement of the

fact. I have examined, in a similar way, the brilliant spark produced between two charcoal points terminating the poles of a voltaic battery, and find it to resemble the ordinary electric spark, exhibiting, as might have been expected from the longer continuance of the light, a spectrum of intense brilliancy, intersected by still brighter lines. These lines did not seem of a deeper colour than the parts of the spectrum immediately adjacent to them, but had rather the appearance of brilliant white lines superposed on the spectrum.

§ II.

The electro-magnetic spark obtained between the two ends of the wire in which the current is induced from the magnet, is, in all cases which have yet been observed, very minute; but it is well known that the brilliancy of the spark is greatly increased by causing it to occur between one of the poles and a cup of mercury connected with the opposite end of the wire. This arrangement is adopted in all the electro-magnets now made, and affords, on the rapid repetition of the spark, a considerable degree of illumination. The intensity of the light thus produced has occasioned it to be generally supposed that it is in part owing to the combustion of the mercury, a supposition which I shall hereafter have occasion to consider.

On analysing by the prism the electro-magnetic spark taken from the surface of mercury, I observed with the telescope the following singular and beautiful phenomenon. Instead of the appearance just described as characterising the ordinary electric spark, and the voltaic spark obtained between two charcoal points, I saw a few definite lines of light, separated by very wide dark intervals from each other, some of great brightness. The first was a double line of orange, and the smallest interval of any was between this line and the next, which was green; the interval between this and the succeeding one, which was greenish-blue, and when attentively examined was seen to consist of two lines close to each other, was about twice as great; after an interval of four this was succeeded by a purple line; and a violet line followed at the distance of three. The order of brightness of the lines was as follows:—green, very bright; purple; orange; violet; the double line of bluish-green; the last is so faint that in small electro-magnets it is not seen. When the spark is viewed near with a common prism, the orange line appears tinged on its lower part with red; but this red is by no means so apparent when the telescope is used; this arises from the red not being a definite ray, and its parts, consequently, being differently refrangible are diffused over a considerable space when the telescope is employed.

The electro-magnet employed for this and the succeeding experiments, was the powerful one constructed by Mr. Saxton for the Gallery of Practical Science in Adelaide Street. To render it convenient for these experiments, that is, to ensure the appearance of the spark always at the same point, the following temporary alteration was made:—The place of the revolving star was supplied by a small oval disc, which while it revolved acted on one arm of a lever, beneath which a spring was placed like that to a flute key; this lever was fixed on a plate of sheet-iron, having at its extremity a small cup to hold the mercury, and the other end of the lever was bent at right angles, so that as the oval disc revolved the bent end of the lever was alternately dipped into the metal and removed, and the spark always occurred precisely at the same point. Another advantage was obtained by this arrangement,—

the mercury was not thrown out of the cup as it is in the ordinary disposition of the apparatus.

§ III.

I next proceeded to observe the prismatic analysis of the electro magnetic spark taken from different metals while in a fluid state. For this purpose I employed the following metals in the purest state I could obtain them:—Zinc, cadmium, bismuth, tin, and lead. I placed the metal intended to be the subject of experiment in the cup formed in the iron plate, and melted it by the application of a spirit-lamp placed beneath; the spark was then taken as above described. Not having at my disposal an instrument like that which Fraunhofer employed in his experiments, by which the degrees of refrangibility might be absolutely measured, I was obliged to content myself with an ordinary telescope-prism, furnished with a micrometer eye-piece, which affords only comparative results. The eye-piece was graduated with parallel lines, in one direction only, the fortieth of an inch apart. The spark was taken pre-

cisely at the same point, and the telescope remained in the same position during the whole of the experiments with the different metals; the spark was also obtained under exactly similar circumstances from carefully-distilled mercury. None of these metals gave an uninterrupted spectrum, but each presented a few bright, definite lines, widely separated from each other; the number, position, and colour of these lines differ in each of the metals employed. These differences are so obvious that any one metal may instantly be distinguished from the others by the appearance of its spark; and we have here a mode of discriminating metallic bodies more ready even than a chemical examination, and which may be hereafter employed for useful purposes. A Table accompanies this paper, in which I have laid down the positions of the bright lines observed in the six metals above mentioned. It will be seen that red lines characterise only zinc and cadmium, and these metals may be known from each other by the red ray in the cadmium being most refrangible. Mercury and lead exhibit bright violet lines, while zinc, cadmium, and tin have

Table of the Bright Lines in the Spectrum of the Magneto-Electric Spark, taken from different melted Metals, and observed with the Prismatic Telescope.

	Standard Soda Flame.	Mercury.	Zinc.	Cadmium.	Bismuth.	Tin.	Lead.
Violet		—			<u>v.f.</u> f		<u>v.b.</u>
Purple		<u>v.b.</u>				<u>b.</u>	—
Green			—	—	<u>b.</u> v.f.	<u>b.</u> <u>b.</u>	
Blue		<u>f.</u>					—
Green				—	—		<u>f.</u>
Green		—	<u>both v.f.</u>		<u>v.f.</u>	—	—
Orange	—	—				—	•
Red			—	<u>f.</u>			

ABBREVIATIONS:—b, bright; v b, very bright; f, faint; v f, very faint.

The above relative measurements were taken by means of a ruled glass micrometer, placed in the focus of the eye-piece of the telescope.

none. The homogeneous light of a spirit-lamp occupied almost exactly the place of the orange lines in the mercury spectrum. The brilliancy of the spark in different metals appears to be in proportion to their volatility; cadmium and zinc are next in brightness to mercury.

On trying experiments with various amalgams and alloys, it was found that when the metals differed much in volatility the lines appertaining to the most volatile metal only was observed; in other cases lines belonging to both were seen.

§ IV.

The voltaic spark, taken between one of the poles of the pile and mercury, or between two globules of mercury, or between amalgamated points, presented exactly the same appearances as the electro-magnetic spark from mercury. Experiments were made with batteries of different kinds with the same results; the only difference observed was that the lines were brighter as the intensity of the spark was increased.

No difference was perceived in the prismatic analysis of the spark when the positive or negative pole was brought in contact with the mercury. Neither did the nature of the conductor produce any change in the colour or situation of the lines. Copper, iron, and platina were successively employed.

§ V.

Having ascertained this proof of the identity, by prismatic analysis, of the voltaic and electro-magnetic sparks, and having it in our power to obtain a strong spark from metals in their solid state by making use of an energetic pile, I now commenced a series of experiments with the intention of extending my Table to all the metals I could procure. For this purpose I put in action the large battery of King's College, consisting of 200 pairs of plates, on the Wollaston construction, with double coppers. These experiments were in progress when I left London, and I intend to resume them, with improved instrumental aids, on my return; I shall, therefore, not dilate upon them at present. One result is, however, too curious to be passed over without notice. Bringing the end of one of the wires of the battery in contact with different parts of the opposite wire, I observed that the spark seen by the naked eye varied in colour at different times, appearing now violet, now green, now red. I have not yet been able to ascertain whether this change of colour in the spark arises from variations in the intensity of the battery, or from the condition of the surface of the wire from oxidation, or any other cause. It might naturally have been expected that the prismatic decomposition of the spark would have varied under these different circumstances, but this, to my surprise, I found not to be the case. The number and position of the lines remained invariably the same; but as my assistant called out the colour the spark appeared to him, I saw in the telescope a line of the same colour, which was already visible in the spectrum flash on the eye with intense brilliancy, while the others remained unaltered. It is, therefore, evident that no mistake in identifying the metals can arise from this cause.

§ VI.

It has been supposed by Dr. Faraday and others that the bright spark from mercury is, in part at least, owing to a true combustion of the metal occasioned by the intense heat developed by the current. To determine whether this is or is not the case, I instituted two series

of experiments: the first to ascertain whether the spark is produced and presents the same prismatic appearances in media where combustion cannot be expected to take place; and the second, to be informed whether the same prismatic decomposition occurs in the flame produced by the ordinary combustion of the metals as in the electric sparks which are drawn from them. With regard to the first point, Sir H. Davy had already proved that the voltaic light obtained by two charcoal points was evolved with equal splendour when the experiment was made in gases in which no combustion could be maintained; and it is also known that the spark occurs, though with diminished intensity, under water, oil, alcohol, ether, and other liquids. To determine whether the light from mercury was equally independent of this cause, I obtained the spark successively in the ordinary vacuum of the air-pump, in the Torricellian vacuum, in carbonic acid gas, and in oxygen, with scarcely any difference of effect, and the lines observed by the prism were the same in all cases,—a proof that the medium had no influence on the nature of the light. It will be unnecessary in this abridged account to enter into any detail of these experiments. The spark from mercury was also taken under water, alcohol, and oil. In these cases, by continuing the production of the spark for a long time, a quantity of black powder was formed, which, on examination, proved to be pure mercury, in a very finely-divided state. The same powder was observed on the receivers when the spark was obtained *in vacuo* or the gases, but it could not well be collected for examination.

These experiments leave no ground for supposing that the electric light is in any case a consequence of combustion. I may add to these proofs the observation of my friend, Professor Daniell, that what is called the combustion of gold-leaf and platinum foil is effected as well in hydrogen gas as in the air.

With respect to the second series of experiments above alluded to, I have examined with the prism the light of different metals while undergoing ordinary combustion. Iron, copper, bismuth, lead and tin were successively burned on charcoal by directing a stream of oxygen upon them. Examined by the prism they all presented bright uninterrupted spectra, in which no redundant or defective lines were visible, the same thing was observed when zinc foil was burned in the flame of a spirit lamp. There is, therefore, a marked difference in the physical properties of light obtained from the same metal by combustion and the action of electricity.

Wires ignited by the action of the voltaic pile, when observed by the prism presented spectra perfectly uninterrupted, and destitute of lines.

§ VII.

The prismatic analysis of the spark developed from different metals by ordinary electricity, brings to view many obvious differences from that excited by voltaic electricity. I have not yet made much progress in this part of the inquiry, and the only results at which I have arrived are the following:—1. Each metal presents lines differing in position and number; the metals I have tried are mercury, lead, tin, zinc, bismuth, copper, and iron. 2. The lines are less bright and more numerous than those which occur in the electro-magnetic or voltaic spark from the same metal. It appears to me that all the lines of the voltaic spark are present in the electric spark, with the addition of many others. In the spark from a small jar taken between two amalgamated balls of tin, I observed lines corresponding in situation,

colour, and relative brightness to those seen in the voltaic spark from mercury; the additional lines were—1, a red line; 2, two fine orange lines between the orange and green; 3, a faint purple line between the bluish green and purple; and 4, a violet line between the purple and violet, but nearer the purple line. Similar observations were made on sparks taken from tin, lead, &c. As I did not determine the exact positions of these lines, I cannot be certain of their perfect identity with those of the voltaic spark. The apparatus I am now constructing will enable me to ascertain this point, and to obtain accurate measurements of the lines. 4. When the spark was taken between two balls of dissimilar metals, the lines which belonged to each metal appeared together.

I intend to construct a table exhibiting the lines in the electric spark obtained from all the simple metals, and shall probably extend the experiments to include many of the imperfect conductors.

§ VIII.

I shall terminate this notice with a few observations on the bearing which the preceding experiments have on the theories which have been advanced to account for the origin of electric light. Having already proved that combustion cannot be its cause, I shall not resume the consideration of this subject here.

The electric light was for a long time considered by philosophers as a modification of electricity itself, which possessed the property of becoming luminous when accumulated to a certain degree. The experiments are decidedly against this hypothesis; were it true the character of the spark might be found to differ according to the source whence the electricity had emanated, which we, however, find not to be the case; but it would be improbable that the accumulated electricity having passed through solid conductors without any change should experience such great modifications simply from the limiting surface from which it passes into the air—the nature of this surface determining the character of the light.

Another theory was advanced by Biot, and was for some time very prevalent. This eminent philosopher having been engaged in some experiments on light produced by mechanical pressure, thought that the electric light might have a similar cause, and be merely the effect of the compression produced on the air by the explosion of the electricity. To account for the production of the spark *in vacuo*, he supposes that the space is filled with disseminated molecules, which constitute a medium offering a very sensible resistance and inertia to a velocity so great as that of electricity. According to this theory, if any change should be observable in the character of the spark, it should depend on the medium which is supposed to produce the light by its compression, and not on the conducting substances from which the electricity escapes; whereas we find the direct contrary to be the case. The experiments of Thenard are also fatal to Biot's hypothesis; this celebrated chemist has proved that light is never disengaged by the compression of gases, and that whenever light is apparently thus produced, it is owing to the presence of foreign bodies.

A different theory was formed by Ritter, which since developed by Davy, Oersted, Berzelius, and others, has very generally prevailed among chemical writers. In this theory the atoms of ponderable matter are considered as each possessing a peculiar electric state, some being positive, others negative; the action of the elec-

tricity of these particles is dissimulated externally by an atmosphere of the contrary electricity which completely envelopes it. Electricity is supposed to be communicated along a row either of positive or negative particles, by a series of decompositions and recompositions of neutral electricity, and the light and heat which accompany this phenomenon are the result of these actions. According to this hypothesis, sparks are produced between the atoms, in the same manner as in a string of metal beads, and the electric light is the result of the sparks between all the atoms. This theory affords no explanation of the modifications in electric light observed by prismatic analysis.

Seeing the insufficiency of all these theories to account for the observed phenomena of electric light, I am strongly induced to believe that it results solely from the volatilization and ignition of the ponderable matter of the conductor itself. The difference between the appearance of the prismatic spectra of the same metal electrically ignited and ignited by ordinary combustion, I conceive to consist in this,—in the first case the particles are by volatilization attenuated to the highest possible degree; while in the second, that of ordinary combustion, the light is occasioned by incandescent particles of sensible magnitude.

Fusinieri has proved by a series of beautiful and decisive experiments, too well known to be more than alluded to here, that in all cases when the electric spark traverses the air, it transports with it the ponderable matter of the conductor it has left. The views to which these experiments led him respecting the nature of electric light, are nearly coincident with those above stated; he assumes that the light arises from the ignition and combustion of the fused particles of the metallic conductor carried away in its passage; but this mode of considering the subject allows of no distinction between electric ignition and that of ordinary combustion; a distinction which my experiments so fully prove to exist.

The peculiar luminous effects produced by electrical action on different metals, depend, no doubt, on their molecular structure; and we have hence a new optical means of examining the internal mechanism of matter, in addition to those which Sir D. Brewster and other philosophers have already placed at our disposal.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE V.

Crystallization—Influence of Cohesion on Affinity—Precipitation—Normal Constituents of Water as it Occurs in Nature—Sources of Impurity in Water for Domestic Purposes.

(Continued from page 187.)

In addition to the solution of solid matter, water can also dissolve gases. We have here a table showing the solubility of different gases in water. Those which dissolve in a less proportion than 250 parts in 100 volumes of water are generally regarded as being merely dissolved in the water without any chemical effect taking place between the liquid and the gas; while those which are dissolved in larger quantities, such as ammonia, hydrochloric acid, and sulphurous acid are aided in their

solution by a simultaneous action of chemical affinity. Now here I have a vessel by which I will try to show you one of the extreme cases. This vessel contains ammonia. One hundred volumes of water can dissolve 78,000 volumes of that gas. [The vessel used for the experiment was a long glass cylinder. The mouth was held downwards, and was plunged into a vessel of water.] I will now open the mouth of the cylinder, and there you see the water rushes up as into a vacuum; and with the exception of a little air with which the ammonia was contaminated the jar is now full of water. Water, from the moment of its condensation from steam, contains gas. Here is an apparatus by which the volume of a gas dissolved by a certain quantity of water can be ascertained. The gas is driven off by heat, and collected over mercury. I will not stop to explain the arrangement, but if any feel particularly interested in the apparatus, I shall be happy to explain it to them after the lecture. In this apparatus, when the air is exhausted, and the pressure upon the surface of the water is reduced, you see those gases dissolved in the water bubble out, and the water seems to be in a state of ebullition.

Now, from what I have said regarding the solvent powers of water, both upon gaseous and solid matter, you will easily conclude that pure water is almost an impossibility; we never meet with it in nature. If we trace water from the moment when it assumes the liquid condition, to the time when it finally makes its way to the ocean, we find it gradually accumulating impurities of various kinds. At the moment of condensation it exerts its solvent power and absorbs gases from the air. When it falls to the earth it percolates through strata more or less soluble and more or less pervious to water, and dissolves, according to circumstances, various quantities of the solid matters which it there meets with in those strata. If the surface of the earth where it falls be very hard and insoluble the water becomes only very slightly contaminated with solid matter. Loch Katrine, for instance, contains only $2\frac{1}{2}$ grains of this solid matter in a gallon of the water; the Dee at Aberdeen contains 4 grains; and the Tay at Perth contains 5 grains. Frequently, also, when the strata through which the water percolates are pervious, supposing they are at the same time comparatively insoluble, very little solid matter gets into solution. Such is the case with the green sand formation where the water is very free from mineral matters of a solid kind. Generally, however, water meets with more constituents than those I have just indicated in these exceptional cases; and the quantity varies from 5 to 50 grains per gallon. When this quantity is exceeded the water generally acquires a taste, and may be regarded as *abnormal* water. The water of the ocean is in this condition. The rivers which flow into the sea carry with them matters dissolved, and leave them there, for the solid matters are not carried back by evaporation. Therefore, the sea contains a larger proportion of these substances than is contained in the water of rivers; and we get this effect on a still more exaggerated scale when in hot climates rivers empty themselves into lakes which have no outlet. This is the case with the Dead Sea. The river Jordan which is constantly flowing into it contains 75 grains of solid matter in the gallon; and none of this matter is returned again into the atmosphere, or no practical amount; therefore there is a constant accumulation of the saline matters going on there, and in this Dead Sea we have no less than 2600 grains of solid constituents to the gallon of water. The same effect takes place in a very recently-discovered lake in the north of Australia, a lake discovered a few months ago. It is highly charged with saline matters, and is known to possess no outlet. There is a similar instance in the celebrated Elton Lake in Russia which is 11 miles long, 8 miles broad, and on an average only 15 inches deep. In summer it appears to be covered with snow in consequence

of the evaporation of the water forming a crust of saline matter. No less than 200,000 tons of salt are yearly extracted from this lake.

In addition to these mineral and saline constituents, water also dissolves certain organic substances, so that we may classify the impurities contained in water as *mechanical*, or those that are merely suspended in the water mechanically, and not dissolved, *saline* impurities and *organic* impurities. We will consider these separately.

First, as regards *mechanical* impurities. They consist, in the first place, of mineral substances which are usually innocuous, and have no effect upon the drinker. Then we have vegetable or animal matters, which are generally of an exceedingly noxious character; and then we have living organisms, which generally attract the attention of the water-drinker more than the other substances contained in the water. These organisms consist of animalcules and animals of a larger size. We have in the New River water twenty-six species of these animals, in the Thames water twenty-nine species, and twenty-four species have been detected in the West Middlesex water, and so on, varying in number, in different waters. Perhaps you might be interested to make a somewhat nearer acquaintance with these beings which often come into close contact with us; and therefore I have obtained from my friend Mr. Warrington some two or three species, which I will endeavour to throw upon the screen. I have here a microscope by which I can produce a very greatly magnified image of them, I hope large enough to be seen by all of you. [The room was then darkened.] There are some of them on the screen. When they get a little more accustomed to the light I dare say they will be a little more steady in their movements, so as to give us a better opportunity of examining them. This is, I think, the *water-flea*. There is one of them, and there [pointing to a smaller] is a younger one. (Laughter.) You see they are very lively in their habits; and this is a kind of creature which is met with, I may say, in all good waters—(a laugh)—for instance, the water supplied to Manchester, which is well known to be one of the best waters in this country, is quite full of this kind of animals in summer. Here [changing the species in the microscope] I think we have got the *cyclops*, which is also a very common organism in these waters. This is an interesting organism, if we can get one tolerably still, because the beating of the heart is very perceptible. We must get one upon its side; that is the best position. The animals are not at all exposed to the heat of the lamp; it is simply the light that frightens them; the heating rays are completely cut off by a thick stratum of water. Therefore they are not subject to any pain, but there is more light than they are used to. I must pass on to another specimen. Both of the specimens which I have shown are to be met with in good waters. I am now going to show you one which occurs only in waters of a very low character indeed. It is the little blood-worm. This is one which I got out of the Thames the other day, and you may get thousands of them out of the mud of the river almost any where, but especially near Westminster-bridge. This is obtained from a locality about a mile distant from where, a few years ago, a great amount of water supplied to London was pumped; therefore, I think we may say that this is one of the organisms which may possibly occur in water used for domestic purposes. See how beautifully transparent the body of this little creature is. A streak of crimson runs through the body in a spiral form. That [pointing], is the head of the animal. However, this is a monster which I trust we do not often meet with in water, but still it is an inhabitant of the Thames water which is yet supplied to some parts of London. It has been stated that some of these little animals are very deleterious to health, but I do not think that this has been at all clearly made out. Looked at from a chemical point of view, they are exceedingly useful in the water, especially if it be at all impure. Each little animal is a

small furnace, which occupies itself in consuming the organic matter and converting it into its ultimate compounds,—carbonic acid, water, and ammonia, or nitric acid. They feed upon the organic matter in the water, and remove it far more readily than by the oxidising property of the air. From a physiological point of view, I do not know what sort of a case can be made out against them, but I do not think they have been fairly condemned. However, it is desirable to remove them before the water

is used as a beverage, and this can be done by filtration. The animals themselves cannot pass through the filter, but the ova can; and therefore this has an important bearing upon the storing up of water, to which I shall have occasion again to allude. These are, then, the mechanical impurities.

We come now to the *saline* impurities. The nature of these will be readily seen by an inspection of this large diagram.

Solid Constituents Contained in One Gallon of Various Waters Supplied to Towns.

	New River Company.	East London Company.	Kent Water Company.	Thames Ditton and Gnd. Junction Co.	Farnham.	Watford.	Loch Katrine.	Aberdeen.	Liverpool.
Carbonate of lime	7.82	10.16	7.02	11.79	.23	16.13	—	.85	.76
Sulphate of lime	3.23	2.33	11.03	3.06	1.31	—	.38	.12	1.
Nitrate of lime	.02	.72	.07	.27	Trace	.23	—	—	—
Carbonate of magnesia	1.09	1.51	3.42	1.27	.64	.75	.15	—	.11
Sulphate of magnesia ..	—	—	—	—	—	—	—	.32	—
Chloride of sodium	1.73	1.76	3.50	1.10	.93	1.01	.45	.67	1.56
Sulphate of soda	1.49	.94	—	—	.07	1.31	.28	—	—
Chloride of potassium	—	—	—	.67	—	—	—	—	—
Sulphate of potash	1.11	1.25	.70	.17	.43	.44	Trace	—	—
Iron alumina and phosphates ..	Traces	.47	Traces	.09	.88	—	Traces	.08	.24
Silica	.50	.62	.76	.62	.99	1.59	Trace	.14	.15
Organic matter	2.79	4.12	2.61	2.29	1.78	1.26	.82	1.82	1.40
	19.78	23.88	29.55	21.33	7.26	22.72	2.08	4.00	5.42

We have here the proportion of solid matter in the New River Company's water; the East London Company's; the Thames Ditton; the Kent Water Company's; the Farnham water, which it was proposed some time ago to bring to London, and which is the green sand water; the Watford; and the Loch Katrine water, which is the purest perhaps ever supplied to towns. Of these constituents you will see that some of them are what are termed *earthy salts*, and some of them are *alkaline salts*. We have carbonate of lime, sulphate of lime, and nitrate of lime, carbonate of magnesia, and occasionally chloride of calcium and chloride of magnesium. These are salts of the alkaline earths, and the others are alkaline salts.

The property that we all know as "hardness" in water is due to the first of these classes of constituents—to the alkaline earths—the lime and magnesia salts present in the water. Hard water may be defined as a water capable of decomposing soap, and forming with the soap an insoluble precipitate. These salts of lime combine with the stearic acid, and form an insoluble stearate of lime, the curdy matter which is produced by washing in such waters. The difference between washing in hard and soft water I think I shall be able to demonstrate to you by means of the electric microscope. This is the water supplied to the Institution—I don't know by what Company. I will wash my hands in this water, and we shall see what sort of result will be produced. I shall get, I have no doubt, some of this stearate of lime precipitated, and we will collect a little of it upon the glass plate and put it in the microscope. In washing you are rubbing the skin with this precipitate—this stearate of lime; and it is important to know what sort of substance it is. It is impossible with water of this kind, in which this curdy, fatty matter is produced, to clean the hands, you may get them nearly colourless; but still the pores of the skin are filled up with this greasy substance, which I will endeavour to show you under the microscope. [A portion of the precipitate formed in the hard water was collected on a glass plate, and a magnified image was thrown upon the screen. The theatre was again darkened.] That is the kind of greasy matter—the stearate of lime—which is there separated, and which we rub into the pores of our skin when washing in water of that kind. I will now show you some of the lather which is formed in

washing the hands with distilled water. Now I think we shall have here a collection of bubbles, but they will be transparent to light and will present a very different appearance from that solid greasy matter which we had formed in the other case. [An image of the lather formed by washing in the distilled water was then projected upon the screen.] You see here what a very different result we have. There is the scum formed in the distilled water. We have none of that thick greasy matter which penetrates into the pores of the skin and renders cleansing with hard waters utterly impossible. When you rub soap upon your hands in the process of washing you form a solution of soap, then you immerse your hands into the water and in washing off that solution you precipitate the stearate of lime within the pores of the skin. This is, in fact, a counterpart of the processes of the dyer and calico-printer. He first of all impregnates the fabric with one solution, and then precipitates a colouring matter from it by means of another solution which is applied afterwards; thus he fixes the colour within the fibre, and thus we fix the greasy stearate of lime within the pores of the skin, and render it incapable of removal by any amount of subsequent washing.

NOTICES OF BOOKS.

How to Work with the Microscope. By LIONEL S. BEALE. Churchill, New Burlington Street.

THERE are two instruments which man has invented both of which are calculated to excite in his mind the most ennobling and the most exalted ideas; one of these is the telescope, the other the microscope. By means of the former he is enabled to gaze into space and obtain views of the most stupendous character, by means of the latter he is enabled to examine the infinitely small. If the magnitude, grandeur, and mystery which surrounds the objects revealed by the telescope impress the imagination more powerfully, the studious examination of the wondrously beautiful, and perfect living organisations revealed to us by the microscope brings us, we say it with all reverence, in closer communion with the mighty Creator,

whose power is manifested more impressively in forming and endowing with life myriads of creatures too small to be visible to man's unassisted eye, than in the formation of globes of inanimate matter of the uses of which we can form no conception.

To derive the greatest amount of pleasure and instruction from the microscope, it is necessary that we should know how to use it in the best manner. Amusement, but very little information, would be gained by using it to inspect objects in a desultory manner during occasional half hours; a system must be adopted, and, first of all, the student must acquire a thorough knowledge of the best method of operating with the instrument. This preliminary study is rendered exceedingly easy by the lucid manner in which Dr. Beale has treated the subject in the work before us. He has not fallen into the too common error of dwelling almost exclusively on the best method of performing the more important microscopical manipulations, leaving the student to find out for himself elementary details; but has descended into these matters with a minuteness which cannot be too highly praised, seeing that it is at the very beginning, where even the most intelligent students chiefly require instruction. On this part of the subject he very justly remarks, "I feel very strongly how intimately success in microscopical, as well as chemical, inquiries, is connected with a readiness in surmounting comparatively small difficulties, and with the possession of mechanical dexterity; and I should be doing great injustice to my pupils if I did not bring these processes under their notice, and endeavour to facilitate, as far as possible, the performance of those operations which are essential to the successful demonstration of the arrangement of textures under the microscope. . . . He who is most fully conversant with elementary detail will be the most successful in the consideration of the higher and more abstruse problems, while he will feel a real love for his work, which is denied to the mere superficial inquirer." Much valuable information is given, also, with respect to the choice of an instrument, and what are the absolute requisites which one must possess to pursue researches in a satisfactory manner, together with the ordinary cost of such instruments.

Dr. Beale's work is one which we would strongly recommend to any of our readers who may desire to make microscopical investigations in a serious and systematic manner. It contains nothing to amuse the general reader, it does not describe the wonders the microscope reveals, but it tells the earnest student the best manner of making these observations for himself and of preserving the objects he may examine; in short, it instructs him fully on those points without a thorough knowledge of which he can never take a foremost place as a microscopist.

CORRESPONDENCE.

Molybdenate of Ammonia a Test for Sulphur.

To the Editor of the CHEMICAL NEWS.

SIR,—In the last Number of your excellent Journal I see a statement of Schlossberger's, "that a dilute solution of molybdenate of ammonia, supersaturated with hydrochloric acid, gives a beautiful blue colour with a solution of sulphide of hydrogen or the sulphide of a metal. This test is more delicate than the nitro-prusside of sodium." In order that others may not be led astray by the above, I have to remark, that I tried in every possible way to obtain the reaction, but in every instance failed. The result indicated a dirty-bluish liquid, which might be mistaken for various refuse menstrua, *e.g.*, the runnings from a foul indigo vat. I expected to obtain a magnificent azure tint, which would at once prove an excellent test. I should never employ Schlossberger's method in seeking

for sulphur, especially when we possess so sensitive a test as the lead one.—I am, &c.

SHERIDAN MUSPRATT, M.D.

Liverpool College of Chemistry, March 25.

Aniline Dyes.

To the Editor of the CHEMICAL NEWS.

SIR,—The patent laws really require serious modification. Constantly some one throws a sum of money uselessly away in patenting a process known and practised secretly before. Secretly, I mean, merely as regards its trade application: or else, by patenting an invention or discovery, the whole matter becomes easy to copy without actual infringement.

Last August I first succeeded in producing and dyeing a good colour by heating a salt of aniline with peroxide of mercury; the mass becomes green, froths up, and requires very careful management of the heat. I could not perfect the process till late in September, and, on the 4th of October, to avoid disputes, I communicated the entire process to Professor Calvert of Manchester.

There can be no doubt that aniline decomposes chloride of mercury—chloride of aniline being formed and oxide of mercury; and starting from this I succeeded in perfecting my process. I may state that its success depends on proper and careful purification. I have devised a ready and economical way of doing this, by which a great quantity of aniline is recovered.

In my own opinion aniline is very prone, in contact with substances containing oxygen in not very energetic combination, to form the colour, and to reduce the oxide, &c.

If sulphate of aniline be heated to a low temperature with oxide of mercury, a dark blue shade is procured; a higher heat makes a redder shade. In fact an almost pure scarlet may be got.

I have observed a curious fact. If aniline and oxide of mercury be heated together, no colour is produced; but if aniline and bichloride of mercury and ammonia be heated together, small quantities of colour are produced, and mercury is precipitated in globules. Does this point to the conclusion that the presence of a salt is useful in the reaction? If so, perhaps the reaction that takes place may be something similar to this, which is confirmed by a long and tedious series of experiments I have lately made.

The aniline decomposes the metallic salt, and forms sulphate, or chloride, &c.; then, by the heat, the aniline becomes as it were nascent in the vaporous state, and reduces the metallic oxide, the chloride of aniline forming the salt, similar to chloride of mercury. I am unable to say what becomes of the chlorine; but after continuing the action of heat on an aniline salt and the mercuric oxide for days together, large quantities of chloride of mercury are invariably found present. Perhaps the oxide of mercury parts with its oxygen to the aniline in incipient vapour, and transforms it into fuchsic acid, while the chlorine goes back to the mercury. Perhaps the reaction is due to the evolution of chlorine and the mercury salt becoming decomposed into subchloride.

I have been led thus far into an almost unknown subject, and must apologise for troubling you with so long an epistle. I look forward with joy to the day of my friend Perkin's lecture on the matter. Perhaps he will, with his superior opportunity and talents, and having only a scientific end in view, develop more fully than I can do the reaction that takes place. I am compelled to avoid mentioning more of my experience in the matter for fear of intruding upon trade secrets.—I am, &c.

JOHN S. BLOCKEY.

Heckmondwike.

Royal Institution.—The General Monthly Meeting of the above Institution will be held on April 1, at 2 o'clock.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

The Amount of Carbonic Acid which Silicic Acid expels at High Temperatures, and the Formula of Silicic Acid.—M. Scheerer finds (*Annalen der Chem. und Pharm.*, bd. cxvi. s. 129) that the amount of carbonic acid which is expelled from pure carbonate of soda depends upon—1. The temperature at which the fusion takes place. 2. The duration of the fusion. 3. The relative proportions of acid and base fused together; and 4. On a peculiar relation of the carbonic acid to the base at a high temperature. The author's researches show that carbonate of soda loses carbonic acid as the temperature rises, and takes it up again as the heat sinks. By fusing together silicic acid and carbonate of soda it appears that three different silicates may be formed—1. The primary silicate, $3\text{NaO} \cdot 2\text{SiO}_2$, when one equivalent SiO_2 is fused with one and a-half equivalents of NaO CO_2 , in which case the carbonate of soda is completely decomposed. 2. The intermediate silicate $2\text{NaO} \cdot \text{SiO}_2$, when one equivalent SiO_2 is fused with three equivalents NaO CO_2 , in which case one-third of the carbonate remains undecomposed. 3. The maximum silicate, $3\text{NaO} \cdot \text{SiO}_2$, formed when one equivalent of SiO_2 is fused with an unlimited amount of carbonate of soda. Experiments with carbonate of potash yielded similar results. The maximum amount of potash which combines with one atom of silicic acid, when this is fused with carbonate of potash, does not appear to exceed two atoms. No higher silicate of potash seems to be formed than $2\text{KO} \cdot \text{SiO}_2$. The lowest formed is $\text{KO} \cdot \text{SiO}_2$. Between these an intermediate silicate may exist. Considering the combining proportions of silicic acid with potash and soda, the author gives the preference to the formula SiO_2 for silica.

Phosphide of Hydrogen.—Landolt shows (*Annalen der Chem. und Pharm.* bd. cxvi. s. 193) that when the non-spontaneously inflammable phosphide of hydrogen prepared by Rose's method is passed into pure nitric acid, sp. gr. 1.34, contained in a small porcelain capsule, the bubbles do not ignite when they come in contact with the air; but when only a drop or two of red fuming nitric acid is added, ignition takes place. A large addition of nitrous acid prevents the ignition. It appears, in the former case, that the nitrous vapour reduces the compound H_2P and gives rise to the formation of the spontaneously inflammable gas H_2P . A small quantity of chlorine or bromine added to water or pure nitric acid, causes no ignition.

II. ORGANIC CHEMISTRY.

The Constitution of Scammony Resin.—The researches of H. Spirgatis (*Ibid.* s. 289) confirm the results obtained by Mayer, and also by Kosman (*CHEMICAL NEWS*, vol. ii. p. 167), which proved the identity of the composition of the resins of scammony and jalsap $\text{C}_{68}\text{H}_{56}\text{O}_{32}$. The author obtained pure resin of scammony by dissolving the rough in alcohol, then adding water until the solution became cloudy, afterwards decolorising by animal charcoal and then distilling off the alcohol until the resin separated. The separated resin was afterwards treated a long time with hot water, whereby a small quantity of a volatile acid, which perhaps gives the odour to the scammony, was driven off. The pure resin is amorphous, perfectly colourless, transparent, odourless, and tasteless. About 150°C . it melts into a clear, colourless fluid, and at a stronger heat begins to decompose, giving off a pungent smell. When burnt, it leaves no trace of ash. It is soluble in alcohol in all proportions, and the solution has an acid reaction. It is also easily soluble in ether, benzol, and chloroform, and, with difficulty, in naphtha and oil of turpentine.

Warm acetic acid also dissolves it without any decomposition. From eight analyses the author gives the following as the composition of the pure resin:—

Carbon	56.50
Hydrogen	7.97
Oxygen	35.53
	100.00

Scammonic Acid, $\text{C}_{68}\text{H}_{56}\text{O}_{32} + 3\text{HO}$. When the resin of scammony is dissolved in potash, soda, ammonia, or baryta water, no resin can afterwards be separated from the solution. Under such treatment the resin takes up three equivalents of water, and becomes an acid. The experiment is best made with baryta water, because the baryta is easily removed. The acid forms a shining, anorphous, transparent, brittle, yellowish coloured mass, which resembles the original resin. It is very deliquescent, has a sharp acid and then bitter taste, but no smell. It is easily soluble in water and alcohol, but hardly so in ether; the solutions react strongly acid. It expels carbonic acid from the carbonates of the alkalis. Acetate of lead gives a dense precipitate with the aqueous solution. The analysis of the acid gave as the per-centage composition $\text{C}54.59 \text{ H}8.16 \text{ O}37.25$. By treating the resin of scammony and scammonic acid with a strong acid, sulphuric or hydrochloric, they are split up into sugar, and a new acid, which the author names *scammonic acid*, the *scammonol* of Kosman, but our authors do not agree as to the composition. Pure scammonic acid— $\text{C}_{32}\text{H}_{30}\text{O}_6$ —says Spirgatis, forms shining white tufts of needles, hardly perceptible to the naked eye. It is odourless, but has a sharp acid taste, is easily soluble in alcohol and ether, and the solutions have an acid reaction. It melts at 64° or 65°C ., and makes a greasy stain on paper, but congeals again at 60° , and then forms a hard, brittle, crystalline mass. When burnt on platinum foil it gives off white fumes, which are exceedingly offensive to the eyes and nose. The mean of five analyses gave as the per-centage composition $\text{C}70.95 \text{ H}11.51 \text{ O}17.54$. The author describes several scammonolates, which we need not notice. By treating the scammonic with nitric acid the author obtained another acid, which Mayer has named *ipomic acid*, the properties of which seem greatly to resemble the scammonic.

MISCELLANEOUS.

The Houses of Parliament.—The Committee appointed by the Government to inquire into the causes and remedy of the decay in the stone used in the Palace of Westminster met on Saturday, the 23rd instant, at Richmond Terrace. There were present:—Sir R. Murchison, Messrs. Tite, M.P., G. G. Scott, Sidney Smirke, G. Godwin, C. H. Smith, G. R. Burnell; Professors Frankland, Hofmann, F. A. Abel, and Ansted; and Mr. D. Wyatt. The Committee appointed Mr. Tite Chairman, Mr. Bonham-Carter, M.P., being the Secretary. The Committee adjourned to Monday, the 8th of April, at twelve o'clock, when they propose to make a thorough examination of the building itself.

Diminution of Salt in certain Seas.—At a recent sitting of the Academy, M. Babinet read a paper on the diminution of salt in the waters of certain seas. Those inland seas, like the Euxine, which are constantly receiving fresh waters from the rivers, while their salt is being successively abstracted by some outlet, such as the Bosphorus, must, in course of time, be transformed into fresh-water lakes. This has been the case with the Lake of Baikal, in Siberia, whose waters were formerly salt, but are now, according to the learned member, as pure as distilled water. During this process, which it has taken

ages to accomplish, the fish which were indigenous before have continued so, although the water is now fresh. Among them are a kind of herring called *omoul*, and also seals of the same kind as those to be found in the Polar Seas. M. Babinet, therefore, concludes that the acclimatization of herrings and seals in rivers is possible, and strongly recommends the Society of Acclimatization to try the experiment, especially with the latter species, which might very well be reared in the lakes of the Bois de Boulogne, giving as a reason that its flesh constitutes the exclusive food of the Esquimaux and Samoyedes.

A New Canadian Dye.—Professor Lawson has prepared a new dye of great richness, in the Laboratory of Queen's College, Canada, from an insect, a species of *Coccus*, found for the first time last summer on a tree of the common black spruce (*Abies nigra*, Poir), in the neighbourhood of Kingston. This new dye closely resembles true Cochineal, a most expensive colouring matter capable of being produced in warm countries only, and which is used to give a fine and permanent dye in red, crimson, and scarlets, to wool and silks. Unlike Cochineal, the new dye, discovered at Kingston, is a native Canadian product, and capable of being produced in temperate countries. Having been but recently observed, a sufficient quantity has not yet been obtained for a complete series of experiments as to its nature and uses; but the habits of the insect, as well as the properties of the dye, seem to indicate that it may become of practical importance. In colour it closely resembles ordinary Cochineal, having rather more the scarlet hue of the flowers of *Adonis autumnalis*, and no doubt other shades will be obtained. The true Mexican Cochineal is now being cultivated in Teneriffe and other vine-growing countries of Europe and Africa, with such success as to displace the culture of the grape vine.

What to observe in Lichens.—There are no plants so variable in character as the lichens; none in which it is consequently so difficult to decide what are species and what are varieties. In order to a comprehensive knowledge of species, it is necessary to study individuals in every condition of growth and from every possible habitat. Hence, the commonest species and varieties become of value—the more so if collected in countries comparatively unexplored botanically, for lichens are no exceptions to the rule that geographical differences are attended by corresponding differences in the characters of the same plants. Every collector of lichens—however common and well-known the latter may be—many therefore consider himself as contributing towards a more scientific and philosophical, because more comprehensive, knowledge of a very Protean, but interesting, group of plants. If the collector make a point of gathering specimens of everything he meets which belongs to the lichen family, he will run a good chance of including some novelties, perhaps new species or varieties. New species are most likely to be met with among the very minute crustaceous lichens which grow on rocks or trees, and which cannot be properly studied without the aid of the microscope; among species, belonging, for instance, to such genera as *Lecidea*, *Lecanora*, *Grophia*, *Opegrapha*, *Calicium*. It is not to be expected that the tyro should make these microscopical examinations or discoveries for himself: he will probably require the assistance of some experienced microscopist or lichenologist. The applications of lichens to the arts are daily becoming more numerous and important. New dye-lichens are being discovered in India and the East. Again recently the probability has been shown, on good ground, that a lichen—the *Lecanora esculenta* of Pallas—was the Manna of the Bible. The colorific capability of a lichen, so far as regards a red or purple dye of the nature of orchill or cudbear, may be readily discovered by simply macerating the lichen—

chopped into small fragments or pulverised according to the nature of its thallus—in a weakish solution of liquor ammonia—allowing the mixture to stand a few days in a warm part of the house, and shaking it frequently, so as to expose the mass to the action of the air. The colorific capability of a lichen, so far as regards other colors—chiefly brown and yellow—may be easily ascertained by simply boiling the lichen, chopped or pulverised as before, in a small quantity of water. Whether and how much mucilage or starch a lichen contains may be ascertained by the same means as last mentioned, and allowing the mixture to cool, when it will gelatinise more or less, if it contain much mucilage. Lichens are very easily collected and transported; they require no sort of preparation; they may be simply allowed to dry in the open air and packed as convenient. Those growing on trees generally require the piece of bark on which they grow to be sliced off with a knife, and those on stones the piece of rock to be broken with a hammer. Both may be wrapped in paper like mineralogical specimens. In all cases the localities and dates of collection should be mentioned, and any further information as to uses, &c., which may be known to the collector.—Dr. W. Lauder Lindsay.

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the CHEMICAL NEWS, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 6d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part III. of the present Volume is now ready.

* * All Editorial Communications are to be addressed to Mr. CROOKER, and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

* * In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Vol. II. of the CHEMICAL NEWS, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

W. P.—a.—By applying to the patentee you could probably get some; but we do not know his address.

F. B., Son, and H.—We trust they will shortly be resumed. It, however, depends upon the author, not upon ourselves.

J. Driver.—Use a solvent in which each is soluble. Benzol, for instance.

A Workman.—See answer to Metallurgist last week.

A Constant Reader.—1. It is a simple matter of calculation, which our correspondent can do as well as ourselves. The price would be over 20s. per 1000 cubic feet: for oxygen, that is very cheap. 2. A clean plate of copper placed in the solution, will precipitate all the silver on to its surface, no special galvanic arrangement is needed.

Books Received.—“Elements of Agricultural Chemistry.” By THOS. ANDERSON, M.D. Edinburgh: Adam and Charles Black. “The Journal of the Board of Arts and Manufactures for Upper Canada” (in exchange).

THE CHEMICAL NEWS.

VOL. III. No. 70.—April 6, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Detection of Alum in Bread,
by WILLIAM CROOKES.

THIS problem is one of far more difficulty than is generally imagined, and it is, doubtless, to this fact that the discordant results obtained by different analysts are to be attributed: one stating that out of sixty-four samples of bread purchased at various shops in poor neighbourhoods at the East of London, where, if anywhere, adulteration would be practised in the most barefaced manner, not a single one was found to contain alum: whilst another analyst, with equal positiveness, mentions the name of a baker who is, in his opinion, almost the only person in a large district at the West End of London who sells unadulterated bread, and proceeds to state that more than 87 per cent. of the bread in London is adulterated. Very few of those who have published anything on this subject give details respecting the process they adopt, but in most instances it seems to be somewhat to the following effect:—The bread is first charred and burnt nearly to an ash; the latter is then boiled in diluted hydrochloric acid, with which a little nitric acid has been mixed; ammonia is then added, and the precipitate which it produces is boiled in potassa. After filtration, hydrochloric acid is to be added in excess, and then ammonia, when it is supposed that the precipitate will consist of alumina. With a pure solution of alumina to start with, doubtless this process would give accurate results; but it must be remembered that in bread the alumina would be accompanied by phosphoric acid, as well as phosphate of lime and phosphate of magnesia, each of which would make its appearance in the last precipitate with ammonia, and would, consequently, pass for alumina. But, granting that this tendency of the phosphates of lime and magnesia to simulate the reactions of alumina had been provided against, it seems to have been almost entirely overlooked by popular writers on this subject that whenever alumina and phosphoric acid meet together in solution, they adhere with the greatest pertinacity, and will infallibly appear together in the last precipitation. I should not have deemed these points worthy of mention did I not know that many analysts are habitually employing similar processes to the above, and are even estimating quantitatively the amount of adulteration in bread by weighing this precipitate of the mixed phosphates of lime, magnesia, and alumina, and calculating it as pure alumina.

My attention was first drawn to this subject by the fact that a sample of bread which was known to be entirely free from adulteration had been pronounced by a somewhat experienced analyst as being largely adulterated with alum. My assistance was asked in order

to disprove this injurious allegation, and, having accordingly submitted the subject to a somewhat lengthened examination, I am induced to lay the results before the readers of the CHEMICAL NEWS, in the hope that when the attention of chemists is drawn to the subject, it may be investigated as fully as its commercial importance deserves.

The great difficulty in my hands has been to devise a process which should not confound other things with alumina. It was easy to frame various modes of operating by which a minute trace of alumina could be detected, but I was for a long time baffled by finding that they were equally delicate in their reactions, whether alumina were present or not. In fact, I do not hesitate to say that the accurate analysis of a mixture of those phosphates which are precipitated from an acid solution by ammonia is one of the most difficult problems in inorganic chemistry that the chemist is liable to meet with in technical analysis. I do not pretend to have yet solved the difficulty, but the process which I have at last adopted has at least the merit of not showing the presence of alumina when that body is absent. It has, on the other hand, the inconvenience of being rather tedious in its manipulation, and to some may seem to be needlessly complicated. No one can be sensible of this fact more than myself; but of the numerous methods which I have tried, both with and without separating the phosphoric acid, this was the only one which invariably gave me trustworthy results.

The bread, of which at least 500 grains should be taken, is first to be incinerated in a platinum or porcelain dish, until all volatile organic matter has been expelled and a black carbonaceous ash remains. The temperature must not be raised much beyond the point necessary to effect this. Powder the coal thus obtained and add about thirty drops of oil of vitriol, and heat until vapours begin to rise; when sufficiently cool, add water and boil for ten minutes. Filter and evaporate the filtrate until the fumes of sulphuric acid begin to be evolved, when ten grains of metallic tin and an excess of nitric acid must be added, together with water, drop by drop, until action between the acid and metal commences. When all the tin is oxidised, add water and filter. Evaporate the filtrate until fumes of sulphuric acid are again visible, when more water must be added, and the liquid again filtered if necessary. To the clear solution now add tartaric acid, then ammonia in excess and sulphide of ammonium. Evaporate the liquid, containing the precipitate suspended in it, in a dish until all the smell of sulphide of ammonium has disappeared. Filter, evaporate to dryness, and ignite to get rid of the organic matter. Powder the black ash, boil it in moderately strong hydrochloric acid, filter, add a crystal of chlorate of potash, and boil for a minute. Now add chloride of ammonium and ammonia and boil for five minutes. If, at the end of that time, any precipitate is observed it will be alumina. From the filtered

solution, if oxalate of ammonia be added, the lime will be precipitated, and if to the filtrate from this, ammonia and phosphate of soda be added, the magnesia will come down.

The following is a synopsis of the process:—

Incinerate to black ash. Powder. Moisten with HO.SO_3 , add HO . Boil. Filter, and evaporate nearly to dryness. Add HO.NO_3 , Sn and HQ if necessary. When action ceases add HO and filter.

Precipitate contains all the PO_4 in conjunction with the SnO_2 . Evaporate filtrate nearly to dryness, add HO , filter if necessary, add $\frac{1}{2}\text{HO.T}$, AmO , and AmS , evaporate and filter, evaporate filtrate to dryness, ignite, boil black ash in HCl . Add K.O.ClO_3 . Boil. Add AmCl and AmO . Boil. Filter.

Precipitate contains all the Al_2O_3 .	To solution add AmO.O , boil, filter.
Precipitate CaO.O	To solutn. add AmO and $\frac{1}{2}\text{NaO.HO.PO}_3$, precipitate of $\frac{1}{2}\text{MgO.NH}_4\text{O.PO}_3$.

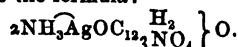
On a Series of New Combinations of Ammonia, Picric Acid, and Metallic Bases, by M. CAREY LEA.

(Continued from page 196.)

Analysis.

4245 substance gave 2632 picric acid.
5865 " " 3600 " "
4245 " " 1577 chloride of silver.
5865 " " 2177 " "
3788 " " 4932 chloroplatinate of ammonia.

This leads to the formula:—



Dried over sulphuric acid *in vacuo*:—

	Calculated.	I.	Found.	II.	Mean.
$\text{C}_{12}\text{H}_2\text{N}_3\text{O}_{14}$	61.62	61.73	61.11	61.42	
Ag	29.19	27.96	27.94	27.95	
2NH_3	9.19	9.93	—	9.93	
	100.00	99.62	—	99.30	

The silver falls below, and the ammonia exceeds the calculated amount in consequence of the impossibility of obtaining the salt quite free from admixed picrate of ammonia.

This beautiful salt appears to be one of the most permanent of this very unstable class of substances. It dissolves readily in hot water containing ammonia, sparingly in cold, and crystallizes in fine needles from the hot solution. Heated on platinum foil it detonates and leaves a brilliant spot of metallic silver.

Ammonia-picrate of copper.

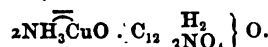
A cold saturated solution of picrate of ammonia added to an ammoniacal solution of sulphate or nitrate of copper, immediately throws down an abundant precipitate, which, after washing with strong solution of carbonate of ammonia, and afterward with dilute ammonia, exhibits a beautiful pale greenish yellow colour with a shade of red through it. It is in fine scales, and much resembles the dust of the wings of butterflies.

Analysis gave for this salt a constitution similar to that of the silver salt.

6059 gms. substance gave 4652 picric acid.
9310 " " 1279 oxide of copper.
14240 " " 1577 metallic copper.
6059 " " 2190 chlorhydrate of ammonia.

The copper was determined in the one case by precipitating the cupric solution while boiling with hyposulphite of soda, dissolving in aqua regia, evaporating to dryness, redissolving in dilute chlorhydric acid, and precipitating with distilled zinc and estimating as metallic copper; in the other by precipitating at 212° by caustic soda.

These results lead to the formula:—



Dried over sulphuric acid *in vacuo*:—

	Calculated.	I.	Found.	II.	Mean.
$\text{C}_{12}\text{H}_2\text{N}_3\text{O}_{14}$	77.63	76.44	—	76.44	
Cu	10.79	10.97	11.07	11.02	
2NH_3	11.58	11.52	—	11.52	
	100.00	98.98			

In an experiment to ascertain whether all the moisture was removed by drying *in vacuo* over SO_3 , a quantity of about two grammes by exposure to a temperature of 210° to 212° for nine hours in the exsiccator lost a little less than one and a-half milligrammes.

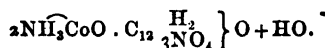
Heated on platinum foil the ammonia picrate of copper explodes with some violence and with a dazzling light.

Ammonia-picrate of Cobalt.

To a solution of protochloride of cobalt prepared from chloride of purpureo-cobalt in the manner recommended by Gibbs and Genth in their interesting and valuable paper on the ammonia-cobalt bases, a large excess of ammonia was added, and to the filtrate, cold saturated solution of picrate of ammonia. An abundant highly crystalline precipitate falls at once, which after drying is yellowish green; while moist decomposes with great facility with separation of cobaltous oxide.

6293 substance gave 4784 picric acid.
6075 " " 4594 " "
6067 " " 1641 protosulphate of cobalt.
5416 " " 1475 " "
6075 " " 2065 chlorhydrate of ammonia.

Which results lead to the formula:—



	Calculated.	I.	Found.	II.	Mean.
$\text{C}_{12}\text{H}_2\text{N}_3\text{O}_{14}$	75.87	75.69	75.29	75.49	
Co	9.82	10.13	10.29	10.21	
2NH_3	11.32	10.81	—	10.81	
HO	2.99	—	—	—	
	100.00				

This salt is much more unstable than the foregoing, and undergoes a commencement of decomposition in washing. Even when this part of the operation is performed with great care, using carbonate of ammonia and dilute liquid ammonia, a portion of picrate of ammonia is washed through, and as cobaltous oxide is not easily soluble in ammoniacal solutions, it is not easily removed by washing with such. It is also probable that more or less cobaltous oxide is thrown down with the precipitate, as the latter at the first moment crystalline becomes quickly more and more

curdy; and ammoniacal solution of cobalt by mere dilution with water throws down a bright green precipitate. In the preparation of the ammoniacal solution, it is better to drop a strong solution of cobaltous chloride into a large excess of ammonia.

Like the foregoing, this salt explodes by heat.

Ammonia-picrate of Zinc.

To a solution of pure sulphate of zinc, chlorhydric acid was added in sufficient quantity to prevent precipitation by the subsequent addition of ammonia in excess. To the clear solution, a hot, strong solution of picrate of ammonia was added. Immediately the whole became nearly solid with beautiful yellow needles and scales; the beaker was rapidly cooled by cold water, the contents thrown on a filter, and washed first with a solution of carbonate of ammonia, mixed with caustic ammonia, and, finally, with dilute caustic ammonia alone.

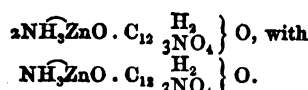
The splendid gold-yellow mass of interlaced needles and scales was more or less decomposed, even by the most careful washing, so that the brightest and purest parts only could be taken for analysis, nor were even these satisfactory.

$C_{12}H_2N_3O_{14}$	mean of 5 determinations	81.26
NH_3	3	9.06
Zn		10.08
		100.30

The formula $3NH_3 \cdot 2 \left[ZnO \cdot C_{12} \frac{H_2}{3NO_4} \right] O$ requires:—

$2C_{12}H_2N_3O_{14}$	79.692
$3NH_3$	8.913
$2Zn$	11.395
	100.000

The salt examined may be either this compound containing admixed picrate of ammonia, or may be a mixture of



The fact that when a strongly acid solution of zinc-chloride is supersaturated with ammonia, the compounds $2NH_3, ZnO, HCl$ and NH_3, ZnO, HCl successively crystallise out, speaks in favour of the latter hypothesis.

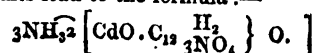
Ammonia-picrate of Cadmium.

The cadmium salt is obtained and purified in exactly the same manner as the zinc, which it nearly resembles; it is, however, not so bright in colour, but pure canary-yellow. Like the zinc salt, it re-dissolves in its mother liquid by heat, without decomposition, but, like almost all the compounds here described, when washed with pure water on a filter, the picric acid and ammonia wash through. A grayish-white residue of oxide of cadmium remains behind.

Heated on platinum foil, the salt deflagrates with a beautiful green light.

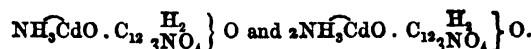
*7170	substance gave	*5247	picric acid.
*6622	"	*4877	" "
*1459	"	*8452	" "
*6496	"	*1653	sal ammoniac.
*1459	"	*2945	" "
*6045	"	*6289	chlorplatinat of ammonia.
*7321	"	*1501	cadmium oxide.

These results lead to the formula:—



	Calculated.	I.	Found.	II.	III.	Mean.
$2(C_{12}H_2N_3O_{14})$	73.67	73.44	72.86	73.31	73.20	
$3NH_3$	8.24	8.10	8.17	7.93	8.07	
$2Cd$	18.09	17.94	—	—	17.94	
	100.00				99.24	

It seems, however, not improbable that this substance may be a mixture in nearly equivalent quantities of the salts:—



for reasons similar to those mentioned in the case of the zinc salt.

Ammonia-picrate of Chromium.

This salt is easily obtained from an ammoniacal solution of chromoxide, which may be prepared in the following manner:—

Three grammes of potash chrome alum are dissolved in water by boiling, the solution removed from the fire, and 14 cubic centimètres of pure concentrated sulphuric acid and 90 cubic centimètres of liquid ammonia added. A strongly-alkaline, beautiful purple solution is thus obtained, which may be preserved in well-stopped vessels, quite filled, for a considerable time.¹

To a solution prepared in this manner, a strong, hot solution of picrate of ammonia, not in excess, is to be added. Some precipitation follows at once, which increases very much as the solution cools. The precipitate is thrown on a filter and washed with dilute liquid ammonia. It consists of splendid green, metallic-looking scales, which, if heated with a considerable quantity of water, even strongly ammoniacal, are decomposed with precipitation of chromoxide. On cooling, picrate of ammonia crystallises out. Even with very careful washing, the salt is always partly decomposed, with separation of green oxide of chromium.

The portions that were least decomposed were selected, but were not sufficiently pure to give any satisfactory result, although a number of specimens were analysed.

I.	*6949	substance gave	*5864	picric acid.
II.	*5537	"	*4735	" "
III.	*4446	"	*3709	" "
IV.	*4446	"	*0179	sesq. ox. chromium.
V.	*6949	"	*0271	" "

These numbers correspond to

I.	$C_{12}H_2N_3O_{14}$	84.02	per cent.
II.	"	85.14	" "
III.	"	83.06	" "
IV.	Cr	2.78	" "
V.	"	2.63	" "

results which do not lead to any satisfactory conclusion, but show that the quantity of ammonia salt necessary to keep the chromoxide in solution throws down picrate of ammonia simultaneously with the ammonia-picrate of chromium. This salt is very beautiful; its lustre is remarkable.

Ammonia-picrate of Manganese.

If ammonia be added to a solution of manganous

¹ While studying the chromium salt, I met with Frémy's "Investigation of the Salts of Chromium." In it the author states that the salts of chromoxide, modified by boiling (green modification), are insoluble in ammonia, except after having been boiled in acid and precipitated by ammonia. It will be observed that the above process is not in conformity with the opinion of M. Frémy, as it is there shown that the green modification, produced by boiling, is rendered soluble in ammonia by the simple addition of sulphuric acid, and without any application of heat. The solution in ammonia is always complete, and the process never fails.

sulphate, previously rendered strongly acid with chlorhydric or nitric acid, a considerable quantity of manganese escapes precipitation, an extremely unstable solution is obtained, which, in a few minutes, even before filtration is ended, becomes cloudy again. To give the solution more stability, the presence of a very large quantity of ammonia salt is necessary,—a circumstance very unfavourable for the production of a pure ammonia-picric acid, as, by a solution so strongly saline, picric acid of ammonia (as already observed) is at once precipitated. The following were the observations made:—

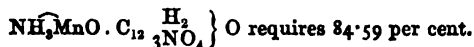
A. If a cold solution of picric acid of ammonia be poured into an ammoniacal solution of manganese, prepared by adding to a concentrated solution of manganous sulphate an equal volume of dilute chlorhydric acid, and then ammonia in large excess, there falls a precipitate of brilliant, satiny scales, mixed, however, with precipitated manganous oxide, which is readily distinguishable on the filter.

B. If the ammoniacal solution is added to a hot, moderately-strong solution of picric acid of ammonia, and the whole be rapidly filtered, the liquid passes through before complete precipitation takes place, and on cooling, granular crystals are obtained, which rapidly turn brown in the air. Heated on platinum foil, they turn brown, melt, and deflagrate sharply, with a brilliant white light.

Neither A nor B yield a salt sufficiently pure for analysis. A always contains admixed manganous oxide, and B picric acid of ammonia. A bears considerable resemblance to the chrome salt just described.

1.0115 of B gave .9115 picric acid.

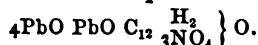
So large a per-centage of picric acid corresponding to 89.72 per cent. of the molecule $C_{12}H_2N_2O_{14}$ probably indicates a large admixture of picric acid of ammonia, which cannot be removed. The formula



Ammonia-picric acid of Iron.

An ammonia picric acid of protoxide of iron appears to exist. If solution of protosulphate of iron be mixed with one of sal ammoniac, and then with liquid ammonia, be rapidly filtered, and the filtrate be treated with picric acid of ammonia, a dark-green crystalline precipitate is obtained; thrown upon a filter, a red liquid passes through, containing a portion of the picric acid of ammonia reduced by the ferrous oxide. The precipitate, when heated on platinum foil, deflagrates with scintillations. Heated with solution of sal ammoniac and caustic ammonia, it does not appear to dissolve, but ferric oxide is thrown down, and picric acid and picramate of ammonia remain in solution. It was found impossible to obtain this salt in a condition to admit of even an approximate determination of its constitution.

If acetate of lead be treated with ammonia in excess, and if to the clear solution be added picric acid of ammonia, a curdy precipitate, at first pale yellow, gradually deepening to orange colour, falls. Heated on platinum foil it detonates with violence. Boiled with caustic alkali, it disengages no ammonia. The result of an analysis showed it to be Marchand's penta-basic picric acid of lead,



The above described salts containing ammonia are all highly crystalline. The lead compound just mentioned is amorphous.

Other ammonia-picric acids exist, which I propose to examine hereafter.

On Arsenical Paper-Hangings, by Dr. W. FRAZER, Lecturer on Medicines to the Carmichael School of Medicine.

HAVING recently been requested by some of my friends to examine paper-hangings for them, which were suspected to contain arsenical preparations, and having in every instance obtained not mere traces, but large quantities comparatively, of this substance from the paper thus tested, it occurred to me that a description of the simple means necessary to determine the fact would be interesting, and perhaps useful to many who might be desirous of having a similar question solved.

There is ample proof that the consideration of arsenical paints is at last engaging the attention of the public in a manner it has never heretofore done, and several papers have been published on the subject in the past year. The paper which was read by my friend Dr. Davy, at a recent meeting of the Royal Dublin Society, abounds in important and practical information. He detailed in it the results of his inquiries, and alluded to several striking instances in which deleterious effects have followed the use of these arsenicated papers, and also the employment of a green arsenical distemper, or water colour, applied as a paint to the walls of rooms in a public institution in this city. He further demonstrated, and I think sufficient notice has not yet been drawn to it, that these same obnoxious colours are lavishly employed in other manufactures, as wax tapers, and in the colouring of green tarlatan for ladies' dresses, to which the poison adheres so loosely that slight friction removes it; and an instance was incidentally mentioned by Dr. Barker, proving that French confectioners have not quite given up the employment of these dangerous colourings, and in which serious results might this winter have readily occurred, from children eating their attractive but deadly sweets. It is right to state, in my opinion, that every such colouring is avoided by our best Dublin houses, and that these sweets come from abroad. To these instances I might myself add the very free use of arsenical green in the painting of blinds and woodwork. I do not purpose to enter into the details of the symptoms which are now recognised as attending the continued inhalation or absorption of small quantities of arsenic. I can readily understand that some individuals are much more susceptible of their injurious influence than others; and this is what we would be led to expect from our previous knowledge of the action of other mineral substances, such as either lead or mercury. But I think that these cases which have been accumulated add another proof to the experience already acquired on the subject, of the effects of poisonous substances in small doses, and almost, I might say, proving the existence of a general law, that "minute, but far from imaginary, homœopathic, or infinitesimal quantities of mineral preparations can and do produce decided and well-marked symptoms in the human frame." Burton, Tanquerel, and others, have well worked out the details of the pernicious influence of lead preparations when absorbed in small and repeated quantities; and it is truly wonderful how small an amount of this metal may produce serious results. In connexion with this, let me detail the following case:—I was asked to see a woman some years since, labouring under obscure and unexplained *malaise*; she had wandering rheumatic pains, constipation, and colicky attacks, for which she had been under medical care for upwards of a fortnight, and as yet with little or no improvement. I chanced to look at her gums, which I always do in obscure attacks of

abdominal pain, and the well-defined blue line at once told its tale of lead as the source of the attack. I had considerable difficulty in getting any history of her exposure to its action; at last it came out that she was employed as a care-taker in an empty house; that the family had been away, and that painters were working there; and, finally, that she used to sleep in the room in which the paint-pots were kept, and that for several nights some of these pots were close to her bed. She rapidly recovered by using suitable treatment.

Copper, too, is another of those metals whose insidious action upon the system, when absorbed in small quantities for a considerable period, is undoubted; and Dr. Corrigan's purple line around the gums is as distinctive and conclusive of its presence as the blue line of Burton is well known to be of lead. In a case which I saw in Dr. Corrigan's wards, there was no doubt either as to the existence of a purple copper-poison line upon the gums of the patient, or of the presence of those symptoms which copper causes; but how was the entrance of copper into the man's body to be accounted for? His trade was that of an upholsterer's workman; he had to all appearance nothing to do with copper, and still, on strict examination into his history, it was found that for some weeks he had been chiefly, if not altogether, engaged in polishing and cleaning up old brass furniture-casters, the dust from which he stated, when closely questioned, had often affected his taste unpleasantly when working at them. Yet the symptoms caused by both copper and lead absorbed in this gradual manner were very different in character from such as attend their exhibition in full medical doses.

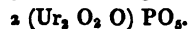
Shall I also instance mercury,—how well recognised as the result of exposure to it in continued, though minute doses, is the metallic trembling, the palsied state of a looking-glass maker, or a button gilder, and yet how distinct from salivation, griping, and other well-known symptoms of the influence of this mineral in full therapeutic doses. In fact, we are but beginning to recognise the truth of that law which I allude to, that the influence of medical agents, especially mineral substances, over the human body, differs not only in degree, but even in essential characters, according to the doses and the mode of exhibition. Palpably I would say that one special class of symptoms will be produced by exposure to the continued absorption of minute doses; and this is noticed alike, whether the substance be inhaled by a painter working in lead preparations, a person inhabiting arsenical-papered rooms, or one breathing the fumes of mercury, or from the repeated internal employment of the deleterious substance, as in drinking leaden waters. That a distinct class of symptoms will attend the use of these same substances when employed in their ordinary medical doses,—a class of symptoms best known to us from their being most frequently brought before our notice; and, lastly, that a third class of symptoms are to be traced when these preparations are exhibited in excessive, or, as they are popularly termed, poisonous quantities. I do not wish to be understood as asserting that the symptoms produced by any one of these substances, arsenic, copper, or lead, have nothing in common in these three modes of exhibition, but that they present broad and well-marked distinctions, valuable to us as practical guides, and interesting as abstract truths.

But, to return to the subject of testing arsenical paper-hangings, I wish to point out a very simple and satisfactory mode of examining these papers, which was first mentioned to me by Dr. Davy. The powder detached

by scraping from the surface of the paper, or any other substance which we desire to examine for arsenic, is intimately mixed with about twice its bulk of pulverised yellow prussiate of potash, which has been previously dried to deprive it of its water of crystallisation, and the mixture is placed in an ordinary reduction-tube, and steadily heated. I always wrap a small portion of moistened bibulous paper around the tube, about half way up, to aid the condensation of the arsenic at this part, which sublimes in the metallic state. I find in practice that the yellow prussiate of potash is much to be preferred to other reducing agents, and I think it will be found that it is so simple in its action, and so satisfactory in its results, that anyone with a moderate amount of care can at once determine for themselves the presence or absence of arsenic in paper-hangings. Let me say, in conclusion, that it is not necessarily a bright-green paper that alone contains arsenic, many very pale greens and greenish-white colours also yield it; and at present I would be disposed to look with more than suspicion on almost all the green tints employed by the paper-stainer. There is no reason that this should continue, and in some of the compounds of chrome I have no doubt satisfactory substitutes might be found, even if they were more expensive than the gaudy but deadly arsenical tints.—*Dublin Hospital Gazette.*

Note on the Estimation of Uranium and Phosphoric Acid, by M. F. PISANI.

If phosphate of soda is poured into an acetic solution of oxide of uranium, the metal is completely precipitated in the state of phosphate, having the formula,—



This precipitate, although gelatinous, deposits perfectly when hot, which allows of its being easily washed.

Estimation of Uranium.—After saturating with ammonia the liquid containing the uranium, acidify with acetic acid, then add a sufficient quantity of phosphate of soda. Allow the precipitate to deposit when hot, and wash it several times by decantation with boiling water. Care must always be taken when, after two or three decantations, it happens that the precipitate does not deposit itself readily, and the supernatant liquid becomes turbid, to add a little chloride of ammonium, which favours the settling. If this precaution is omitted, some phosphate of uranium passes through the filter, and considerably retards the filtration. Whilst the precipitate is on the filter wash it with hot water, with the addition of chloride of ammonium, and dry it artificially. Then separate it as completely as possible from the filter, calcine it in a platinum crucible, taking care to incinerate the filter separately. The calcined phosphate has generally a green tint, owing to a partial reduction, but that has very little influence on the results. The weight of phosphate of uranium, multiplied by 0.8023, gives the quantity of oxide U_2O_5 , O.

Estimation of Phosphoric Acid in Combination with Alkalies, Alkaline Earths, and Magnesia.—Although the method of estimating phosphoric acid in alkaline phosphates by sulphate of magnesia yields excellent results, yet chemists well know that a long time is required for its complete precipitation, and there is always a difficulty in detaching the portion adhering to the sides of the vessel. Even in this case the uranium method of estimation can be recommended. Moreover, it must be observed, that as the equivalent of phosphate of uranium is very high (it contains 80 per cent. of oxide

of uranium), the loss ought to be less than with magnesia.

The following is the operation for estimating phosphoric acid by this means. After dissolving the phosphate, either in water or hydrochloric acid, add a certain quantity of nitrate of uranium, supersaturate with ammonia, and acidify with acetic acid: phosphate of uranium is thus precipitated. The precipitation is known to be complete by the supernatant fluid having a yellow tint, owing to excess of uranium. If incomplete, a fresh quantity of nitrate of uranium must be added, and afterwards ammonia or acetic acid. The precipitate must be treated with the precautions above-mentioned. Its weight, multiplied by 0.1977, gives the relative quantity of phosphoric acid. This method of estimating is particularly applicable to the separation of phosphoric acid from magnesia, a separation usually attended with considerable difficulty.

I have also successfully utilised this method for analysing the uranite of Autun in the following manner:—

Dissolve the uranite in nitric acid; then, after separating the gangue, add to the solution some ammonia, and afterwards a slight excess of acetic acid. All the phosphoric acid is precipitated with the uranium, and the two bodies are thus estimated simultaneously. A little oxide of uranium usually remains in the filtered liquid, which may be separated from the lime in the ordinary way.

The estimation of phosphoric acid by uranium does not succeed well when phosphate of iron and of alumina are present, these being always precipitated in more or less amount with phosphate of uranium.—*Comptes-Rendus*, vol. lii.

On the Presence of Arsenic and Antimony in the Sources and Beds of Streams and Rivers, by DUGALD CAMPBELL.

The author has forwarded the following letter to the Editors of the *Philosophical Magazine*:—

"Since my communication upon the above subject,² published in the *Philosophical Magazine* of October last, I have repeated my experiments upon several of the sands I then reported upon, and with the like result which I then gave. I have also made experiments upon other specimens since obtained, and in all I have hitherto examined I have found arsenic, and generally, if not always, accompanied with antimony. The process followed was the same as I formerly described, only I invariably used hydrochloric acid without the slightest trace of arsenic in it, as some doubts had been cast upon my former results in a notice of my paper in the *CHEMICAL NEWS* of the 20th of last October, because in my anxiety to admit of anyone testing the accuracy of my results I had described how the process might be conducted with what is generally sold as pure acid, but which, if properly tested, is rarely free from arsenic.

"During these last experiments it occurred to me to distil the sands with a second and a third dose of acid; and in most cases I have found the yield of arsenic and antimony to be much greater—say from two to five times—in the second distillate than in the first; and in some I have found the third distillate to give more than the first, but in others less.

"These results induce me to say, that before a sand could be pronounced not to contain any arsenic or antimony it should be distilled to dryness with at least three distinct doses of acid, each distillate being tested carefully in the manner described in my former communication."

TECHNICAL CHEMISTRY.

The Society of Arts' Annual Exhibition of Inventions.

THE Thirteenth Annual Exhibition of Inventions was opened to the public on Monday at the Rooms of the Society of Arts, John Street, Adelphi. Many of the articles there exhibited are of a character to be of considerable interest to our readers, and we therefore purpose to give a short notice of those inventions which, upon careful inspection, we deem most deserving attention. The figures are the same which are attached to the articles themselves. The first in the Catalogue which attracts the notice of the chemist is No. 2. *A Specimen of Patent Fuel*, exhibited by Jabez Church. This fuel is manufactured from the breeze of gas-works, mixed with asphalt or coal-tar pitch, slaked lime, and a sufficient quantity of coal tar to render it adhesive. It is then placed in a wrought-iron scoop, which is to be put into a retort, such as is commonly used in gas-works, and then submitted to the action of heat during five or six hours, when it forms a fuel for locomotive engines, as well as for other purposes requiring the use of coke fuel. This would, in our opinion, be improved by leaving the lime out. It not only is useless in the fire, but it would remain behind as a caustic and irritating ash, which would be liable to cover everything near with fine lime-dust. Added to this, it would occupy space to no purpose,—an important consideration for steam-vessels. No. 8. *A Safety-pipe for Steam-boilers*, exhibited by Samuel Terrill. This pipe is placed in the boiler, and fastened at the bottom by rivets. Two inches above the furnace a lead ring is placed, forming part of the pipe. If the water should at any time be low the lead will melt and permit the steam to escape through the pipe at the top of the boiler. The lead being placed two inches above the furnace, will be prevented from corroding by any action upon it of sediment in the boiler. No. 17. *Sebill's Patent Water, Gas, and other Conduit Pipes*, exhibited by John Gedge and Son. These pipes are manufactured (by pressure in a cylinder) from a composition of waste slate, vegetable or mineral pitch or resin, and a small portion of animal or vegetable fibre or bristle. They offer the advantages of cheapness, comparative lightness, and facility of connection with each other, or with other pipes, jointing being effected by bringing the ends of the pipes together and passing a hot instrument over them, when the pipes will cement by running one into the other, or when softened at the joint a portion of the same composition may be used as a solder. By boring with a hot augur, any description of branch pipe may be at once introduced, or a tap. This plan of forming pipes is very ingenious, and the facility of connection or insertion of taps, &c., renders them superior to the bituminised-paper tubing, which we are sorry to find is not exhibited alongside. On the other hand, they would probably be inferior, in the matter of brittleness, to the paper tubes. No. 22. *Improved Safety-lamp for Coal-mines*, exhibited by Charles Edward Crawley. This lamp combines Crawley and Schneider's

¹ *Philosophical Magazine*, Fourth Series, Vol. xxi. p. 318.

² *CHEMICAL NEWS*, Vol. II. 218.

improvements with those of Howden and Thresh. Its advantages are stated to be,—1st. That it gives considerably more light than any other description of safety-lamp without the use of glass; this is effected by means of a tube, open at either end, fixed in the bottom of the lamp, passing through the wick, and protected at the lower end by a double gauze. 2nd. It can be immediately extinguished without trouble. 3rd. It has an insulated handle, which enables the miner to carry it for any length of time without burning his fingers, even were the rest of the lamp to become red-hot. 4th. It seldom, if ever, will become red-hot, whatever quantity of gas may be burning inside the gauze. 5th. It will, on account of its peculiar construction, consume, whilst burning with a good flame, from one to two feet of gas per minute, thereby tendering, under ordinary circumstances, to some extent to lessen the danger. 6th. The lock is rendered perfectly secure by means of a seal, consisting of a very small, thin metal disc, with any kind of device stamped upon it, fixed over the lock in such a manner as to render it absolutely impossible for the miner or any one else to open the lamp without breaking the seal, thereby forming a perfect detector. The seal would be varied from day to day, so that the miner would never be able to tell what seal would be used on any particular day. 7th. The great increase, however, in the light would of itself remove the temptation to open the lamp, added to which it gives, if anything, less light when opened. If light arising from combustion must continue to be used in coal-mines, this lamp is perhaps as safe a one as could be devised; but why is not electricity employed, in one of its many economical forms, as a means of lighting the miner at his task? No. 23. *Specimens of Machine-puddled Balls of Iron*, exhibited by William Yates. These specimens of iron were made by Tooth's patent self-acting puddling machine, which is a wrought-iron cylinder, lined with fire-bricks, and driven by steam power. The charge of pig iron is introduced at the chimney end of the barrel; in the door is a hole for inserting the rake, and which also serves as a peep-hole to watch progress. The barrel is kept stationary during the fixing of the pig, after which it is set slowly rotating and kept so moving till the charge has gone through all the usual stages of boiling, working dry, and dropping, which takes about an hour. The iron now begins to gather into a mass; the speed of the barrel is accelerated to solidify and shape the balls; the workman now, for the first time, has to put a tool to his charge, to cut off from the lump pieces suitable for his requirements; the fire-box is now shunted away and the orifice through which the flame had entered the barrel serves as the opening through which to admit the tongs, which are attached to a small crane affixed to the standing frame of the cylinder; thus the balls are extracted ready to be sent to the shingling hammer and rolls. The machine, the working of which these balls are illustrations, represents one of the most important improvements in the manufacture of iron which has been made since the days of Cort. If it answers in general practice as well as it has on the small scale it will revolutionise this branch of manufacturing industry. No. 25. *Patent Metal for Bearings, Ships' Sheathing, &c.*, exhibited by John Gedge and Son. This metal is a composition of copper, zinc, and iron. From its toughness, it is stated to be a suitable metal for bearings of railway rolling stock or fixed machinery, and being cheaper than copper, and equally unaffected by sea water, can be used for sheathing ships' bottoms. No. 84. *Rostain's Compounds of Gutta Percha with Mineral*

and Vegetable Colours and Substances for Dental and other Purposes, exhibited by John Gedge and Son. For dental purposes the crude gutta percha is first purified from earthy and other matters soluble in boiling water, and then from oily and odorous parts soluble in alkalies; it is then heated to 110° or 120° Réaumur, to soften the substance; oxide of zinc, dense white or coloured, and a preparation of tannin are added, and the whole carefully mixed, and when it is desirable to give an agreeable odour, oil of peppermint or lavender, or an essential oil, mixed with a solution of gutta percha in chloroform, is added. Some of the preparations exhibited are composed of gutta percha and a mineral combination of blende, kaolin, and calamine, varied by the addition of a little catechu. The patent mineral colours used in combination with the gutta percha are unalterable and non-poisonous. We think that the employment of gutta percha which has been purified by Cattell's valuable process would be a great improvement upon the above. Its pure white colour would enable it to be tinted of any desired shade. No. 88. *Specimens of Aluminium and of Aluminium Bronze*, exhibited by Bell Brothers. The specific gravity of aluminium is 2.5, or about one-fourth that of silver, and thus, weight for weight, the bulk of aluminium is four times the bulk of silver. It does not tarnish by exposure to the air, has no perceptible odour or taste in the mouth, is malleable, can be forged, either hot or cold, equally well with gold and silver, and rolled into thin sheets or leaves; is ductile, so as to be capable of being drawn into fine wire. It further resembles silver in elasticity and tenacity, and, when cast, in hardness. When hammered, it takes the character of wrought iron, with elasticity and considerable rigidity, sounding like steel when let fall on a hard body. It is extremely sonorous, and M. Lissajous has made tuning-forks of it, which act extremely well. It melts at a temperature a little above that of zinc, and considerably below that of silver. Aluminium may be readily run into moulds, and when heated to a high temperature in the crucible loses none of its weight. From experiments made by Deville, he deduces its power of conducting electricity to be eight times that of iron; and as a conductor of heat it stands high amongst metals. According to this chemist, water, whether hot or cold, has no action upon the metal, even at a red heat near the point of fusion. It is, however, slowly oxidised when steam is passed over it at a white heat. Sulphuretted hydrogen has no effect upon it, nor has sulphur itself, so long as the metal is not heated higher than a red heat, though at a higher temperature they combine, forming sulphide of aluminium. Sulphuric acid, so diluted as to attack metals which ordinarily decompose water, has no action upon it whatever; and, according to De la Rive, the contact of a different metal, as in the case of pure zinc, does not help to dissolve the metal. Nitric acid, weak or strong, at the ordinary temperature, does not act on it, but when boiling it slowly dissolves it. Hydrochloric acid, whether weak or strong, is the true solvent for aluminium. Alkaline solutions have an energetic action on it, but caustic alkalies have no effect upon it, even when in a state of fusion. Ammonia exercises a feeble action. The organic acids, such as vinegar, tartaric acid, &c., have little or no action on it. The effect, however, of a mixture of vinegar and salt is different; for in this instance a small amount of hydrochloric acid is set free, which acts on the metal, but even this action is extremely slow, much slower than on tin. The salts of tin, too, have a strong flavour, whilst the salts of aluminium are less in quantity, and have little

or no flavour. Deville considers that the action of sea water on aluminium is decidedly less than on copper. It can be gilt or plated by galvanic agency, but acid instead of alkaline solutions must be used. A coating of copper may in like manner be given. The effect produced on it by saliva is very slight, scarcely perceptible, even when the metal is kept for a long time in the mouth. Up to the present time no solder for joining it has been found which is satisfactory, though it is stated that M. Mourey has succeeded in this object, but his process is not known. M. Hulot has proposed to effect this object by covering the surface with a deposit of copper, and then employing the ordinary solders. The solution of this problem would tend much to bring the metal into general use. The alloys of aluminium and copper, forming what are termed "aluminium bronzes," are remarkable. That composed of 10 per cent. of aluminium and 90 per cent. of copper is probably the most remarkable. It is a perfect chemical combination, and has no tendency, as is the case with ordinary alloys, to separate under the influence of heat. These proportions represent an exact number of chemical equivalents of the two metals. Aluminium bronzes are of a yellow or orange colour, closely resembling gold, and take a fine polish, equal to that of steel. The chemical properties are the same as those of other copper alloys. In tenacity they fully equal steel. Drawn into wire, No. 16 gauge, the breaking strain of copper, according to Mr. Gordon, was 190, of iron 280, of aluminium bronze 434; showing a strain of 84 kilogrammes to the square millimetre. Good French iron, in Deville's experiments, broke at a strain of 60 kilogrammes the square millimetre, and steel wire at a strain of from 90 to 100 kilogrammes. It thus appears that steel, and that of a fine quality, only can stand a comparison with aluminium bronze in respect of tenacity. As regards hardness, a comparison was made between a steel and a bronze groove for the guide-blocks of a locomotive engine, and, after six months' use, no trace of wear was perceptible; the bronze gave a result equally good with the steel. It was also tried for the journals of the front wheel of a locomotive, with excellent results, its great malleability, combined with hardness and tenacity, rendering it well adapted for this purpose, where ordinarily a very brittle alloy is used. The bronze containing 10 per cent. of aluminium can be rolled at all temperatures, from cold up to a bright cherry-red. It rolls well at a bright red heat, breaks less and elongates more than pure red copper. It is difficult to roll cold, and after a number of passes through the rolls, it elongates no further; it is then necessary frequently to re-heat it, as it hardens rapidly under the rolls. It is desirable to roll it at as high a temperature as possible, short of fusion. Re-heating and plunging in water to cool renders the alloy more tractable than simply reheating without dipping. If re-heated to a bright red heat, and not dipped in water until it has been left to cool in the air down to a low red heat, it is sufficiently malleable and ductile, when cold, to bear without breaking the ordinary manipulations in working it, except some descriptions of stamping.

(To be continued.)

Natural Oxide of Silver.

THE following letter appeared in the *Times* last week:—

"Sir,—Up to within a very recent period the existence of such a thing in nature as oxide of silver has been denied by the most eminent chemists.

"Science, however, has now demonstrated not only that it does exist, but also in enormous quantities in the neighbourhood of the ordinary metallic silver deposits.

"The result is simply this, as proved by recent experiments:—

"First, That in a ton of ore in its natural state, where the normal average of yield of the metal would be 13 ounces, now, by the new process, an average of 113 ounces of silver would be obtained, the overplus being derived from that which has hitherto been rejected as useless; and, secondly, That the cost of this new method of reduction is relatively so small as to render the expense but little more than nominal.

"The importance of this discovery to our own mines where silver is often found in close approximation to copper and other metals will be very great, but to some of the foreign ones, where the deposit is large and unmixed with other metals, the result in the shape of returns will be almost fabulous.

"Yours obediently, "CHEMICUS."

In corroboration of this, "R. G." writes to the same paper as follows:—

"Sir,—In your impression of the 29th of March you publish a letter from 'Chemicus,' on the important subject of 'Natural Oxide of Silver.' The period up to within which the existence of silver in the form of a natural oxide has been denied by scientific men of the highest chemical attainment has hardly yet passed away, there being even at this moment some whose opinions or prejudices are so strong that they have not given themselves the trouble to investigate the subject, and either still wholly deny its existence, or assert that, if it does exist, it can only be extracted in such small laboratory experiments as would render the discovery commercially valueless. The existence of silver in gozzans in some form hitherto unsuspected and unknown was first discovered two years since by Mr. F. Squire, who conducted a series of successful experiments in public with a view to the utilisation of the discovery, the result of all which goes to prove the accuracy of the figures stated by 'Chemicus,' as the proportion of yield in the ores before and after treatment,—ores which in their natural state gave, upon assay, from 5 to 13 ounces of silver per ton, yielding from 54 to 216 ounces. These experiments were mainly conducted on a small scale, but the discoverer of the presence of silver in this form, and of the method of concentrating, or, as it may be perhaps more properly called, globulating the same, asserted that operations on a large scale would be even more successful, and it may be interesting to 'Chemicus' and to the public to know that this position he has been enabled to prove as incontestably as the former.

"The importance to this country generally, and to the county of Cornwall in particular, of this most interesting discovery it is impossible to over-estimate. There are millions of tons of gozzan in Cornwall, all known to contain silver, but believed to contain it in such small quantities as to be worthless, which may be worked under this discovery to an enormous profit. In Russia silver ores containing four ounces of silver per ton are worked profitably. In Mexico, where labour is dearer, the lowest yield is five ounces. Taking a comparative statement for the higher wages of England, it would not pay to work ores containing silver only in less quantity than 10 ounces to 12 ounces. The Cornish gozzans are known to contain from 5 ounces to 13 ounces on the average in their natural state, but it is proved that in the form of oxide they contain from 54 ounces to 216 ounces, according to the average of the trials yet made,

while some have been found to be fabulously richer. It follows that if an average yield of 12 ounces could be worked without loss,—an average the lowest yet obtained by the process of reduction now adopted, 54 ounces would yield an immense profit,—the expense of raising, reducing, and crushing these neglected ores could not exceed 3*l.* per ton, while the value of 54 ounces of silver only would be over 13*l.*, and that of the average yet obtained over 35*l.* per ton.

"In every point of view, then, this fresh discovery of science may be looked upon as likely to confer great and lasting benefit upon the country. It will open up a vast field for the employment of labour as well as capital, and will turn into wealth thousands and tens of thousands of tons of mineral hitherto looked upon as waste and worthless."

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE VI.

Normal Constituents of Water as it occurs in Nature—Sources of Impurity in Water used for Domestic Purposes.

At the close of the last Lecture I directed your attention for a moment to a particular class of the constituents present in water as it occurs in nature, these constituents are generally regarded as insoluble, and yet we find them in water in very considerable quantities. I allude to the carbonates of lime and magnesia which are present, for instance, in the New River Company's water, to the extent of nearly 8 grains per gallon, in the East London, to the amount of 10·16 grains. These numbers refer to the carbonate of lime only. Then we have also carbonate of magnesia present—in small quantity, it is true, but still in an appreciable degree. The Thames Ditton water contains 1·27 grains in the gallon. Now comes the question, how are these substances, which are generally regarded as insoluble, dissolved in the water? Chalk or carbonate of lime is so nearly insoluble in water that one gallon of pure water will only take up 2·5 grains of this compound. Carbonate of magnesia is perhaps scarcely so soluble. Now, the cause of the solution of these two comparatively insoluble salts is the presence of carbonic acid among the gaseous impurities of water. I have already alluded to it as one of the gases dissolved in normal water, and I think I shall be able to show this power of carbonic acid in dissolving carbonate of lime. If we take some lime-water, which is quite clear and transparent, to begin with, and pass through it a stream of carbonic acid, which I shall get from this apparatus, we shall, I think, almost immediately begin to see a turbidity produced. [A stream of carbonic acid gas was passed through a vessel of lime-water.] There you see we have carbonate of lime thrown down. We have a "precipitate," as we term these insoluble matters that are thus separated from solution, and that precipitate is carbonate of lime; so that you see carbonate of lime is obviously insoluble in the water in which the lime itself had previously been dissolved. Now, I think we have perhaps thrown down as much as is necessary for our purpose, and I want to show you now that on dividing this turbid liquid into two portions we shall be able to dissolve one of them by means of water containing carbonic acid in solution, while distilled water will entirely fail to dissolve the other. We will try distilled water first. You see, of course, the solution is less turbid, but we certainly have no effectual

solution of the carbonate of lime, or very little indeed. Distilled water generally contains a little carbonic acid, but not enough to dissolve the carbonate of lime. Now let us try the effect of the carbonic acid water. I think we shall not require to add so much of this, as of the other, and yet we shall in the course of a few seconds see a very marked difference between the two waters. Here you see we have perfect transparency produced. The carbonate of lime which was at first thrown down is dissolved by this carbonic acid water, while the distilled water had not the power of dissolving it. The precipitate of carbonate of lime was quite readily soluble in water containing carbonic acid, and hence the solution of this substance as it occurs in nature. The same remark applies to the carbonate of magnesia.

We alluded in the last Lecture to the property, possessed by most of these natural waters, of curdling soap by producing a precipitate in a solution of soap, and of possessing, consequently, the property which we generally term *hardness*. To which of the constituents of water is this hardness due? Is it due to the whole of the constituents? If not, to which? There is a very convenient mode of testing this. What is termed the "soap test" is used for this purpose. Here I have some of it which consists of a solution of pure white soap in dilute alcohol. The alcohol is made use of because the mixture of alcohol and water dissolves more soap than water alone. Now, let me add some of this soap-test, as it is termed, to the distilled water, and I will add it from a graduated vessel, in order that we may notice the quantity we take. Here is about a pint of distilled water, and I will add a very few drops of this solution of soap in the dilute alcohol. On shaking the vessel, you see we at once get a lather which is quite permanent, or very nearly so. It will break over the surface presently, but if I add a few more drops of the solution the lather, you see, is now practically quite permanent: we have, in fact, an excess of soap there over what is decomposed by the water itself. You see we have produced very readily, by this very minute quantity of our soap-test, this lather in the distilled water. Now, let us try the effect of the same test upon a solution containing salts of sodium—containing the sulphate of soda and the chloride of sodium, which are two of the principal alkaline salts contained in those natural waters. Here is some of this water; we have a larger quantity than in the former experiment, but I am not very particular about quantity. I will add about the same quantity of soap-test. You see we get a lather there, but not a permanent one yet. We must add a little more. We have now a very evident lather but still it is not quite permanent. We will make a third addition of this test. We have still added a very small amount. I have about twice as much water as in the former vessel of distilled water; and we have added about double the quantity of soap-test. Now, we will add in the same way some of this soap-test to the water supplied to this Institution. I will, in the first place, add a little more than was added to the distilled water in the first instance. You see we get not the slightest lathering effect there. We will add a much larger quantity. [A larger quantity of the soap-test was poured in.] But we have still no approach to a lather. We will add a very large quantity. Well, we are just approaching now the point at which a permanent lather would be produced. By the addition of a little more soap-test we shall get a permanent lather: but you see what a quantity of soap is lost in this hard water—what a large quantity of soap is consumed in the formation of this curd which I showed you in the microscope in the last Lecture, and what a very small quantity over and above this is necessary to produce this effect of lather in the water, and to give the water a detergent property. There, you see a very little more of this soap-test will suffice; at last, we have a lather with some pretensions to permanency. In the first place we had distilled

water which produces a lather with a very small quantity of soap; then we had this solution containing alkaline salts which did not take more, bulk for bulk, than the distilled water; therefore we see that that water which contained ten grains of sulphate of soda, and ten grains of chloride of sodium is, notwithstanding practically as soft as distilled water. But where we have these salts of the alkaline earths, lime and magnesia, as in the waters supplied by the London companies, we require to add large quantities of the soap-test in order to produce a lather.

In order that you may see the hardening effects produced by the lime and magnesia salts we will add a little more of our soap test—an excess of soap to this water. We have, you see, a lather there. Now I will add to this water, with the lather upon it, one of the constituents which is generally present in hard water—namely, sulphate of lime which is present in the New River Company's water in the proportion of 3.23 grains to the gallon. This sulphate of lime is not very soluble in water; it requires about 500 times its weight of water to dissolve it, still I need add but a very small quantity of its solution to the water in which we have the lather. [A small quantity of sulphate of lime was then added to the water.] You see our lather has disappeared; we have no longer a permanent lather, we have no longer an excess of soap there. That very small amount of sulphate of lime added to that large quantity of water killed the excess of soap we had at first. The hardening constituents of water are the carbonates of lime and magnesia, sulphate of lime, nitrate of lime, and sulphate of magnesia. All the other constituents present in that analytical table do not practically harden the water at all.

Now, having this hard water to deal with very frequently, especially in such chalky localities as this where the water charged with carbonic acid percolates through the soil and readily takes up some of these salts which are otherwise nearly insoluble, it is a very important point to ascertain how this hardness can be removed—whether it can be removed by any process at all, and in the next place what is the best method of doing it?

The hardness resulting from each of the different hardening constituents is not equally capable of removal. The hardness due to the carbonates of lime and magnesia is comparatively very easy of removal. The hardness caused by the nitrates, sulphates, and other salts is very difficult to remove. The best means to remove, upon a small scale, the hardness, when it is due to the presence of the carbonates, is to boil the water. You see the condition upon which the solution of these carbonates depends is the presence of carbonic acid in the water; but water at the boiling temperature cannot dissolve carbonic acid, or retain it dissolved if it has already dissolved it beforehand. Consequently, in heating such water for some time every trace of carbonic acid is expelled, and the carbonates of lime and magnesia are precipitated. Here, for instance, is some of the water supplied to this Institution, and which has thus been boiled. At the bottom of the vessel is a white sediment of the carbonates of lime and magnesia, which has been deposited on boiling. The hardness of such water as this may be reduced to three and a-half or four degrees by boiling. What are these degrees of hardness? By this term the chemist understands an amount which is equal to one grain of carbonate of lime, so that you can see at a glance from the analysis of a water what will be the hardening effect of the carbonate of lime in it. For instance, this New River Company's water will have 7.82 degrees of hardness due to the carbonate of lime. But then the other hardening constituents—namely, sulphate of lime, carbonate of magnesia, and sulphate of magnesia—have hardening powers somewhat different from those of the carbonate of lime. We require somewhat more than an equal quantity of the sulphates of lime and magnesia to produce the same degree of hardness. We require rather less of the carbonate of magnesia to produce

the same effect. But, practically, we do not mind to which of these ingredients the hardness may be due, as our soap-test affords a ready means of determining its exact degree. The soap-test is so made that, if one of these measures is required to produce a lather with a thousand grains of the water you are investigating, that water possesses a hardness of one degree, and contains either one grain of carbonate of lime to the gallon, or a quantity of the other constituents equivalent in hardening effect to one grain of carbonate of lime. Thus the quantity of the soap-test required will enable you at once to ascertain what is the comparative hardness of any particular water, and consequently the comparative amount of soap which will be expended in producing a lather, and the comparative amount of the greasy matter which will be produced before the detergent process can take place.

Another process, and a very beautiful one, for softening water, consists in the removing of the carbonic acid by the addition of caustic-lime, to the water. Caustic-lime has a strong affinity for carbonic acid. We passed a stream of carbonic acid gas into a solution of caustic lime, and we at once had the lime thrown down in the form of carbonate of lime, so that the lime had fixed the carbonic acid, which was in a gaseous state, and produced this white precipitate. If we add a further quantity of lime to take away the carbonic acid which has already dissolved the carbonate of lime, we shall effect a precipitation of the carbonate of lime, and we shall at the same time remove in the same form the lime we add to the water. Let me see if I can illustrate this by an experiment. Here is a vessel more than half filled with the water supplied to the Institution. This water [pointing to a vessel of lime-water] contains lime in solution. This water [referring to the vessel containing the water supplied to the Institution] contains carbonate of lime. The lime in the first vessel wants to get at carbonic acid and combine with it; and I dare say we shall find when we add this lime-water to the water in the other vessel, that we shall get a very copious precipitate of the carbonate of lime and also of the carbonate of magnesia; for I scarcely need to explain that the same thing which holds the carbonate of lime in solution holds also the carbonate of magnesia, and they are thrown down as a precipitate by the same means. Both are thrown down together. Let me try to illustrate this a little more clearly to you by our atomic cubes. Here we have the carbonate of lime contained in the water from the pipe. It is composed of one equivalent of lime and two equivalents of carbonic acid. To this we added in the larger vessel pure lime or caustic lime, which will be represented by this cube which we will add to the others. Now, the effect of the addition of lime was to cause an equal distribution of the carbonic acid between the two equivalents of lime, so that this atom we added ran away with half the carbonic acid of the bicarbonate of lime originally contained in the water, and left it with only one equivalent of lime; but this carbonate of lime, which contains only one equivalent of lime to one of the carbonic acid, is practically insoluble, and goes to the bottom. The carbonate formed by the union of the lime added in solution with the carbonic acid which was taken from the bicarbonate of lime was also insoluble, and was, therefore, precipitated. The carbonated hardening constituents of water may be thus removed, the subsidence being complete in the course of a few hours. Here is some water which has been thus operated upon, with the carbonates below, settled to the bottom of the vessel. Both these vessels contain water which has been operated upon in the same way. This [pointing to the water contained in one of the vessels] is the clearer. It was operated upon some six or eight hours ago. The other one was operated upon only two hours since. Now, this is really practicable upon a large scale. At the present

moment water at Woolwich and Plumstead—which is very hard—is softened in this way. It is pumped out of the chalk strata and mixed with some milk of lime, and allowed some twelve or eighteen hours to subside before being used. It is thus softened from sixteen or eighteen to about six or eight degrees of hardness, which makes a considerable difference when you come to operate upon water with soap. This process, however, is one which is not adapted for being carried out by individuals. It is a process which can only be carried out upon a large scale, and involves a certain amount of trouble. I know a gentleman in London who softens the water supplied to him in this way; but it is not a convenient process for general adoption. There is, however, another way in which this softening can be performed so as to avoid the curdling of the soap, which I showed you in the last lecture, and which can be applied in a moment to a basin of water when it is about to be employed for washing. In order to show you the effect, I am about to employ hot water, because we get the effect far more rapidly and visibly with hot than with cold water, and it is a very visible effect which I must produce in order to render it evident to you that there is any effect at all. The method I am now going to explain to you removes the whole of the hardening matters, therefore it has an advantage over the lime, because the lime only removes the carbonates; but as these are generally the most abundant it reduces the hardness down to five, or six, or eight degrees. What I am going to do now is to add to this water a few grains of carbonate of soda—of the soda, in fact, which is used by laundresses for softening water. Although we do not mind rubbing the greasy matter into the skin, it is found that when such a substance is produced the water will not do for washing linen. Two or three grains of carbonate of soda to a pint of water are sufficient. You see in this case an effect is at once produced. Instead of having clear water as before we have turbid water, which is due to the precipitation of the hardening substances. It is quite easy to soften two or three gallons of water in this way when required. It is, however, advisable not to add a further amount than this, otherwise the alkaline carbonate of soda will produce an unpleasant effect upon the skin, which would not make its use popular. If, however, you guard against using too much you will find this use of carbonate of soda very agreeable and advantageous.

This method, then, as I have said, removes the whole of the hardness, and we have another process by which the hardening constituents; and, in fact, nearly all the impurities of water may be effectually removed, and that is distillation. But here again we have a process which is not practicable under ordinary circumstances. Water thus prepared would necessarily be a comparatively expensive article. The process is, however, carried out in the Navy, and the water is subsequently aerated by an ingenious process invented by Dr. Normandy. But this is an exceptional case. We could not expect this process to be adopted on land, where there are far more available processes capable of being applied.

I have now to notice very shortly the organic impurities contained in water. These are partly of animal and partly of vegetable origin. They are both of them decidedly objectionable in water, but the animal impurities are by far the most objectionable. I allude now to both classes of impurities when in solution, not in mechanical suspension, because we have seen the latter are capable of being removed by the simple process of filtration. These organic impurities consist of materials generally formed of the elements carbon, hydrogen, nitrogen, and sulphur, and the peculiarity of them is that, as they become dissolved, they are constantly undergoing chemical change, and it is during that process of chemical change that danger is to be apprehended. Substances of this kind containing nitrogen—and this is peculiarly the

character of the animal impurities contained in water—constitute a class of substances which chemists call “ferments,” of which yeast is a well known example. Now, these ferments have the property of causing chemical change in substances with which they are brought into contact, without actually entering into combination with the new compounds. Thus, in the case of yeast, sugar is resolved into alcohol and carbonic acid. Some of these ferments, when they gain access to the system, produce disease. We have, for instance, small-pox virus, and there are others of these ferments which produce a very definite action on the human system. Now, the mode in which ferments act cannot be said to be very clearly made out, but still it has been generally ascertained that the use of water containing any of this class of organic impurities in a state of chemical change—and they must be, if dissolved, in a state of chemical change at the ordinary temperature,—I say it has been ascertained that these are highly dangerous to health, and therefore the use of water containing these organic impurities, of animal origin, ought to be very stringently avoided. It is not possible, however, to give you any test by which these can be infallibly detected. The distinction between *animal* and *vegetable* impurities belongs to one of the nicest points of chemistry, but still there is a test which will assist us in the matter, and that is the permanganate of potash. This salt produces a solution of a very beautiful pink tinge. If this liquid be added to water containing no animal matter whatever, the colour will simply be diminished in intensity by being diluted with the water. In twenty or thirty minutes, if the water contains organic impurities, the colour will disappear. It disappears from vegetable impurities in time, and from animal impurities more rapidly. Let us now apply our test to see which of these two waters would be the best to be used as a beverage. We will add a drop of this test first to this water. I do not want to add too much. You may perhaps already be able to see a difference in the tinge of these two waters, even from that distance; the difference to me is very perceptible indeed here, and I think at least it ought to be seen at that distance. I do not want to add too much of this solution to it, because I want to show you that this very small quantity which I have added is sufficient to produce this very decided change in the tinge. Let us add about the same quantity of this solution to the other water. Now I think, if you compare them, you will find that there is a very decided difference, and that this last water is not altered in colour. You see in one case we have a water which is free from organic matter; in the other we have a water which not only contains organic matter, but contains organic matter of a dangerous kind; it is, in fact, water that has been intentionally mixed with sewage. It resembles some of the water which is sometimes pumped up from the wells in the streets. We will add a further quantity of the test, and you see with this large addition of pink solution we have not the slightest tinge of colour. Now we will add a still further quantity. You see with this large addition it is still practically colourless,—the tinge of colour is very imperceptible. I could go on adding until I had completely oxidised all the organic matter in this water, and as soon as that was accomplished, we should get a tinge produced by the first few drops of the solution in excess added to the water. You must not expect, if you apply this test, that you will find absolute freedom from organic matter; but still the colour ought to be only very slightly discharged if the water be fitted for use as a beverage.

The next form of contamination which is met with in water is that of lead, which results from the solution of the pipes and vessels through which we transmit, and in which we store up, the water that we make use of. Although we have the misfortune, here in London, to be supplied with very hard water, there is a little alleviation of this

distress in the fact that this hard water is almost incapable of dissolving lead, and becoming contaminated with this form of poison. It is only when it is kept for a long time in lead vessels, and under circumstances favourable for the solution of the lead, that such contamination need be feared. Seeing that this is the case it is, perhaps, very unwise to adopt any method for preventing contamination which is not very certain in its action. Such a method is in use here and elsewhere. It consists in coating with a thin layer of tin the inside of the pipe; and no doubt, so long as the coating remains intact, you cannot have a trace of lead dissolved. But, in the first place, this tinning process may never be so perfect as to prevent some small portion of the lead from being uncovered; and, in any case, where joints are made, you will there certainly have some of the bare lead exposed. Then comes the question, Have we no voltaic action set up? There must be. Now if that action be in one direction, our pipe will be perfectly free from any contaminating influence; but if the action be in the other direction, the use of the tin will greatly increase the contamination instead of preventing it. The difference consists simply in the direction of the electric current which is always set up when two metals of different kinds are put in contact with water, if one of the metals is at all oxidisable. If the electric current pass in this particular case from the tin to the lead then the water will be effectually protected from being contaminated with lead; if, however, the current proceed from the lead to the tin, then if any trace of lead be exposed in the pipe, a voltaic action will be set up, which will cause rapid corrosion of the lead and its rapid solution in the water, and then the contamination from the lead will be produced perhaps a hundred-fold.

Let us try whether this tinning does effect the protection of lead or not. It is a question very easily settled. We have here some of the galvanometer which will indicate the direction of the current. Here I have a sheet of tin and here I have some lead. If the red end of the needle moves towards you when I bring the two ends of the metals into the water, the question as to whether the tin protects the lead will be answered in the negative. In that case the current passes from the tin to the lead, and the tin will have the effect of causing a more rapid solution of the lead, if the latter be in any part uncovered. Now we will immerse the two plates without letting them come in contact. [No effect was produced upon the galvanometer.] There is some want of contact here. The wire is broken off here from one of the metals, so that there is not the slightest possibility of a current being communicated to the galvanometer. [The detached end of the wire was refixed to the plate of metal, and the experiment was repeated with success.] There you see the red end of the needle moves towards you, and the question is answered in the negative—the tin does *not* protect the lead, but on the contrary, it facilitates the solution of the lead in the water.

We all know that spring-water is a very refreshing and delicious beverage—very different from the kind of water we usually meet with in large towns. What is the reason of this? The reason is that in spring-water we have a well aerated water—a water which has dissolved carbonic acid and oxygen from the atmosphere in descending through it, and in percolating through porous strata where, it is true, it comes in contact with organic matters, but they are present in a very small amount. It goes down below the surface and acquires a mean temperature of somewhere about 50°, while the water we drink in London during the summer varies from 65° to 75°. Further, spring-water (if it be carefully selected at least) is free from any of the organic impurities of which I have spoken, and being used immediately it is brought out of the stratum where it has accumulated, it possesses those refreshing qualities to which I have just alluded. Now these condi-

tions are quite within the reach of most, if not all, of the Companies supplying water to this metropolis. If you look at this list [referring to the table printed in the last lecture] you will find that we have not a very large quantity of organic matter, except in the case of the East London Company where the quantity of organic matter is more than four grains to the gallon. Many of these organic substances are, probably, entirely vegetable. The quantity you see in no other case is by any means excessive. We may say, in fact, that the London Companies have it in their power to supply us with water possessing very nearly an equal degree of purity and equally refreshing qualities with that which we obtain directly from the spring. But it cannot be done on the present system of supply. It is quite impossible, in the way in which it is supplied at present to have it in any other way than that in which it now reaches our lips, and these conditions are superinduced entirely through the mode in which it is supplied. Let us just glance at this mode. We collect the water in a vessel of this construction [pointing to a diagram of a square water-cistern, and supplied with water by means of a ball-cock]: the water is turned on for some twenty minutes during the twenty-four hours. Here is a ball-cock which rises with the water, and when the water rises to a certain height this cock prevents a further quantity of water from flowing in. There is also a waste-pipe which prevents the cistern from overflowing. The waste-pipe is fixed to carry off the water in case the ball-cock should get out of order, and the pipe communicates with the street sewer. It is, however, sometimes, perhaps generally, trapped. The lower end of the pipe is re-curved, and the water collects there, and prevents gases going back again into the cistern. So long as the water continues to fill the bend of the pipe, such an escape of gas from the sewer into the cistern is impossible, but then, if water be first poured down into this pipe, it will evaporate in the course of three or four weeks, and leave the space quite free for the passage of gas, and this ball-cock is so arranged that it does not let the overflow run down the waste-pipe, unless it gets out of order, which is, perhaps, once in six or seven years. When the water has evaporated from the bend in the waste-pipe, the sewer gases are free to pass into the cistern. You all know, when you open a window, the air rushes into the room; and in the same way the air from the sewer rushes up the waste-pipe into the cistern, and we have seen what a powerful absorbent of gaseous matter water is. The water becomes contaminated to a very considerable extent, and this contamination goes on until the water is almost saturated, because probably not one-third of this water is used in the day. The cistern is again filled up from the ball-cock, and the fresh stratum of water is again saturated in the course of a few hours with these noxious gases, and thus the water reaches our lips in this infected, unwholesome condition. The supply of pure and refreshing water is utterly impossible upon this intermittent system. You see, if the water were laid on so that, by opening a cock, we could at once obtain it without the necessity of storing it in a cistern, we should have at any moment water free from contamination, and perfectly free from these sewer emanations. Almost all the large towns except London are now supplied upon this constant system. Manchester is thus supplied, and the benefit which results is incalculable. We have now only one source of pure water in London, and that is our drinking-fountains. We cannot, however, have the water supplied to us in this state in our own houses, and the only plan to obtain it free from contamination is to send for it to the drinking-fountains.

I see, however, that our time is very nearly gone, and I have one or two other matters still to mention. The first is ozone. I have already stated that hydrogen is capable of uniting with one equivalent of oxygen, forming water; with two equivalents of oxygen to form binoxide of hydrogen; and

with three equivalents to form "ozone," or, at all events, to form tetroxide of hydrogen, the existence of which is theoretically indicated, if ozone be not this very compound. What is this ozone? Some chemists suppose it not to contain hydrogen; others think it does contain hydrogen; and a series of analyses certainly did seem to prove that hydrogen in the proportion I have stated was a constituent of ozone. Now, ozone is produced in two or three different ways. One of the most peculiar is by means of the electrical machine. There is a peculiar odour in the neighbourhood of an electrical machine when worked, and that odour is usually supposed to be due to ozone. It is caused by electric sparks passing through the air. We can collect this ozone in a variety of ways; but before doing so I must refer you to the liquid contained in this glass vessel, which furnishes a very delicate test for the presence of ozone, and of other matters too, as we shall presently see. It consists of a solution of iodide of potassium and starch. Ozone, containing as it does three equivalents of oxygen, readily gives up oxygen, and it is capable in this way of oxidizing the iodide of potassium and converting the potassium into potash. The iodine thus liberated forms an intense blue solution with the starch. Here I have a Ruhmkorff's coil, which will give us a series of sparks. You see by the working of this air-pump the so-called ozonised air passes through the liquid, which becomes blue. You see we have now got a very decided blue colouration here. That is one mode by which ozone is produced; but we shall see that the blue colour is not entirely due to the presence of ozone. Now, another mode for the production of ozone is by placing moist phosphorus in a close air-jar. Here is some filter paper which has been imbued with the solution of iodide of potassium and starch, and which will become blue when it is plunged into this vessel containing ozone produced by this process. This constitutes what is generally known as the usual test for ozone in the atmosphere. Then we have another mode of producing ozone, and that is by electrolysis. When water is mixed with some highly oxidizing substance, such as chromic acid, we have this ozone produced when the water is decomposed by a current of electricity. It was from this source that ozone was produced for analysis, and it was from this that the formula I have here used was formed—namely, HO_3 , three equivalents of oxygen and one of hydrogen.

Ozone, as I have said, is supposed to be present in the air, and many very careful observers are at the present time making observations upon the relative quantities of ozone present in the air. Papers of this kind [exhibiting ozone papers] are exposed to the atmosphere for certain fixed and definite lengths of time, the amount of blueness which they manifest in that time is carefully noted, and the intensity of this blue colour is supposed to represent the comparative quantity of ozone present in the air. Now, it is greatly to be regretted that such an amount of labour should be expended upon a matter which is utterly and entirely valueless, because this so-called "test" for ozone is really only a test for a great number of things which may—nay, do—exist in the air, and from which ozone may be really absent. We may say that there is not up to the present time a single experiment which demonstrates that ozone is present in the atmosphere; and certainly these tests, so far from proving its existence, do not even infallibly demonstrate the presence of an oxidising influence in the air. It is quite possible that this blueing of the paper may be produced in a condition of air very different from that in which ozone is present. Ozone being a highly oxidising substance, is supposed to decompose organic impurities in the air, and therefore the air which contains the largest quantity of this ozone is supposed to be the most wholesome. You have only to have present, in the air, some acid gas,—you have only to go into the neighbourhood of some chemical works, for instance, where hydrochloric acid gas is evolved,—and you will

there have plenty of these manifestations of the presence of ozone. Here we have the test I have just applied and described. Here we have some solution of iodide of potassium and starch, and I acidify it with acetic acid. You see we have abundance of ozone indicated, but it is said that ozone is never present in this part of London. The liquid has become of a dark blue colour. The reason is, that the iodide of potassium which is contained in this liquid, and in all these test-papers, is decomposed by acids, and hydriodic acid, a compound of iodine and hydrogen, is immediately formed. It is only necessary to bring this hydriodic acid in contact with free oxygen; when the hydrogen is oxidised, and the iodine is liberated; so you see this so-called "test" for ozone is perfectly unreliable. But to what are the effects due which are produced upon the test solution by the air through which the electric sparks passed? They are due, as I will show you in the next Lecture, to the combustion of nitrogen, which forms nitrous acid when electric sparks pass through the atmosphere; and I think I shall be able to show you that this gas produces a very intense colouring effect upon the ozone test-papers.

SOCIETY OF ARTS.

Wednesday, March 20, 1861.

Professor H. W. WILLIAMSON in the Chair.

At this meeting Mr. CHARLES TOMLINSON read a paper on *The Economic History of Paraffine*, a portion of which we will extract, as our readers can hardly fail to regard the subject as an interesting one. After the usual introductory remarks, Mr. Tomlinson said:—

"There is scarcely a better known natural product than common coal, and yet how marvellous are the properties of that familiar substance. Its history, if fairly written, would be of the bulkiest; it would take us into many sciences, it would draw upon many arts, and when written—when all that is known respecting it had been set down—the last words of the last page must still be, 'To be continued.'"

"Such must ever be the case when the finite attempts to take measure of the Infinite—when the human seeks to scan the purposes of the Divine mind in the varied relations of an individual specimen of any one of the great kingdoms of nature.

"I trust the Society will not deem these reflections impertinent to my subject, which, though including only one of the applications of coal, is indeed so large, that in attempting to give anything like a complete and logical account of it, I am bewildered with its richness, and the multiplicity of details, all so important, and so suggestive, that my task of selection would be one of great difficulty, were I not guided by a principle which this Society I know will appreciate, namely, that of insisting on its technological, rather than on its scientific bearings. The botanist, the chemist, the geologist, the paleontologist, have abundant information to give us respecting the various kinds of coal, bitumen, petroleum, &c. My business is to gather together a few details chiefly respecting one substance that has been associated with coal and other carbonaceous bodies, and to point out some of its applications to the arts of life.

"Paraffine, however, is so mixed up with other compounds of carbon and hydrogen that it will not be easy to disentangle it without some reference to them. I must, however, be permitted once more to remind the Society that my object is industrial, not chemical. Some of the most illustrious chemists of this and of other countries have been, and are devoting their high powers to the solution of questions of great importance and still unsettled, respecting the products of the different varieties of coals and bitumens, so that it would be presumptuous

in me, a simple technologist, to pretend to do more than invite the attention of the Society to a few practical applications of the labours of those distinguished men.

"In common with all other branches of industry, the distillation of coal has been of slow growth. Sources of inflammable gas had long been known to exist in the coal districts of this country before the year 1659, when Mr. Shirley communicated to the Royal Society of London a notice of some experiments on an inflammable gas issuing from a well near Wigan, in Lancashire. Some years later Dr. Clayton made some direct experiments on the distillates of coal, but they were not communicated to the Royal Society, or indeed made public, until the year 1739. He says:—'First there came over a flegm, then a black oil, and then likewise a spirit arose, which I could in no wise condense.' This spirit (as the inflammable non-condensable gas was called) excited so much curiosity that the black oil was disregarded. Yet Dr. Hales, in 1726, had published a notice of some experiments on the distillation of coal, and had mentioned the condensation of a volatile oil in the vessel attached to his still. Dr. Watson also noticed the production of oils, when coal is heated to redness in close vessels.

"This scanty information expressed all that was known for many years on the subject of the liquid products of the distillation of coal.

"In 1781, Lord Dundonald obtained a patent for 'a method of extracting or making tar, pitch, essential oils, volatile alkali, mineral acids, salts, and cinders from pit-coal'—'which coals, being kindled, are enabled by their own heat, and without the assistance of any other fire, to throw off in distillation or vapour, the tar, oil, &c., they contain into receivers or condensing vessels,'—the art depending on the management of the air admitted into the kilns, which can only be acquired by experience.'

"In 1797, a writer in the *Encyclopædia Britannica* refers to Lord Dundonald's process with applause, 'He turns to a very considerable profit the mines of coals in his and other estates, building ovens of a proper construction for burning pit-coal into coke, and at the same time collecting in separate receptacles the volatile alkali, oil, tar, and pitch, which are generally lost by the usual method.' The writer goes on to say, that this method, compared with the usual method of coking coal, 'affords a very remarkable instance of the great losses to mankind, for want of carefully attending to every result from great processes of art when made on a large scale. These ovens are so contrived as to admit an under supply of air, and the coals, after being kindled, decompose themselves by a slow but incomplete combustion, which does not destroy the ingredients. The residuum left in the oven proves to be most excellent cinders or coaks.'

"It appears that about this time, M. Faujas de St. Fond introduced into France a similar process, from information which he had obtained at Lord Dundonald's works, forgetting, however, as Frenchmen sometimes do, to acknowledge the source of his information.

"The article above referred to states that, 'on subjecting pit-coal of any kind to distillation in close vessels, it first yields phlegm or watery liquor, then an ethereal or volatile oil, afterwards a volatile alkali, and lastly a thick and greasy oil; but it is remarkable, that by rectifying this last oil, a transparent, thin, and light oil of a straw colour is produced, which being exposed to the air, becomes black like animal oils.'

"Mr. Northern, of Leeds, in the *Monthly Magazine* for April, 1805, directed attention to the use of coal gas. He says, 'I distilled in a retort 50 oz. of picked coal in a red heat, which gave 6 oz. of a liquid matter covered with oil, more or less fluid, as the heat was increased or diminished; about 26 oz. of cinder remained in the retort; the rest came over in the form of air. . . . In the

receiver I found a fluid of an acid taste, with a great quantity of oil, and at the bottom a substance resembling tar.' In this, and in Lord Dundonald's previous process, we have the germ or basis of the manufacture of volatile oils from coal. The germ, however, was destined to remain nearly half a century without expanding, and what at last gave it vitality was the demand for rapid solvents of caoutchouc, which led to the distillation of tar, as a source of benzole, when other valuable luminiferous agents were found to be present, or separable from the tar.

"The subject, however, had not altogether remained neglected. Substances were obtained from different sources, and by different inquirers, in various parts of the world, and at length becoming assembled and made known to each other, certain relationships were recognised where least expected. In 1812, Mr. Le Witte patented an apparatus for extracting tar from coal, for the manufacture of varnishes, and for protecting surfaces. It is stated that 10 per cent. of tar was thus obtained, while the residual coke was a valuable marketable product. In 1819, Theodore de Saussure obtained from the asphaltic limestone, near Neuchâtel, in Switzerland, an oil which he found to be identical with that from the petroleum of Amiano. In 1824, Chervau took out a patent for a method of distilling bitumen from the rocks in the Department of Saône-et-Loire. The manufacturers stated that they extracted 40 parts of volatile oils from 100 parts of rock, and they recommended their bitumen or naphtha as a fuel for alcohol lamps. The year 1829 introduces us to Reichenbach, the proprietor of chemical works in Moravia, whose name occupies a high place in connection with our subject. In that year, while examining the tar obtained from the dry distillation of beech-wood, he discovered *paraffine*, a wax-like substance, which earned its name from its want of affinity for other bodies (*parum affinis*, or 'little affinity') for it would neither unite with acids nor alkalies; it was insoluble in water and alcohol, and had no action on metals. An oil, which possesses some of these properties, has received the name of paraffine oil, and has been extensively used as a lubricator. Reichenbach, also, in pursuing this inquiry recognised paraffine in the tarry matters distilled from other species of wood, and also in the tars arising from the distillations of the bitumens, and, at a later period, in the tar obtained from coal. He also obtained from the tar of wood naphthalin and creosote, and also bodies named piccamar and pittacal, which have been but imperfectly examined. In 1833 and 1834, he distilled coal in close vessels in contact with water, but from 220 lbs. of coal he obtained only 9 ounces of volatile oil. In 1833, Dr. Bley obtained a small quantity of volatile oil, and some ammoniacal products, by distilling brown coal. These failures, as they must be called, so far as respects coal, arose from misapprehension respecting the required temperature, the manipulation of the gas-house naturally suggesting a high degree of heat, and inquirers did not recur to the earlier methods of trying the effects of a comparatively low one. As early, however, as 1841, improved forms of apparatus for distilling coal were introduced by the Count de Hompesch and others, and the problem how to obtain liquid products instead of permanent gases began to assume a clearer form.

"Hompesch's patent is 'for improvements in obtaining oils and other products from bituminous matters, and in purifying and rectifying oils obtained from such matters.' These are 'bituminous schists, shales, or slates, or other rocks or minerals containing bitumen or bituminous substances.' He describes the oil of schist or clay slate as 'essential oil, intermediary fat oil, and thick oil.' His method of distillation is in a long iron close retort, arched on the top, and flat at the bottom, and part of his apparatus is an Archimedean screw, for filling the retort and turning out the coke at the other end.

"There were three tubes leading from the retort, one at the part farthest from the furnace, another in the middle, and another at the end nearest the furnace, and Hompesch says that he obtains the three separate oils by the gradual increase of the heat, thus effecting distillation without decomposition of the substance. His mode of rectifying the fat oil, is by distilling it with sulphuric acid, and treating it with caustic potash lees.

"In 1845, M. Du Buisson patented a process for 'new and improved methods for the distillation of bituminous schistus, and other bituminous substances, as well as for the purification, rectification, and preparation necessary for the employment of the productions obtained by such distillation for various useful purposes.' His works were at Autun, in France, and one feature in his process is the employment of steam. He applies a low red-heat, and 'when the oil begins to be disengaged, steam is introduced through the cock into the cast-iron pipes which are now heated to redness.' His object in introducing the steam, is to abridge the process of distillation. 'The effect,' he says, 'of introducing steam at a high temperature, is to shorten the time necessary for distillation, six hours for every sixteen, which is a very great advantage, and nearly double the quantity of oil is produced in a given time.' By the introduction of steam also, 'the retort does not require to be heated to so high a temperature, which prevents decomposition of the oils, and saves wear and tear of the apparatus.' The practical effect of the steam was thus to carry off the vapours of the schist as fast as they were formed, and to prevent the retort from being raised above red-heat by the external application of the fire. The products of the distillation were a liquid bitumen, named raw oil of schistus, ammoniacal water, and a carbonaceous residuum. From this raw oil of schistus are obtained two volatile oils, the first of which may be used in lamps with the greatest advantage with a reservoir below, and with a double current of air in which the oil rises a distance of about five inches by the capillary attraction of the wick. The light is described as being 'superior to that of gas, without any unpleasant smell or smoke; the said lamps have a glass or chimney, with a diaphragm of the same diameter as the wick, and placed a little above the top of the burner.' Another oil is also described, capable of being 'burnt in the said lamps without any alteration, by mixing with it a corresponding oil obtained from petroleum and other bituminous mineral substances.' In the third place, he gets a fatty mineral oil, which he says, 'contains paraffine in large quantity. It is particularly applicable for lubricating, for machinery;' and, fourthly, he obtains paraffine 'by crystallisation from fat and thick oils; it is thus obtained very pure, and requires but little treatment to make excellent candles.' He also gets a black and siccative tar. His method of purifying the raw oil is by distillation with sulphuric acid, drawing off the supernatant oil, neutralising the sulphuric acid by means of a caustic solution of soda, and by alcohol, the latter carrying off a pure red colouring matter. He also distills again, and collects the different oils of different densities into separate receivers. He has also a method of depriving the first refined product of its bad odour by means of sulphuric acid, caustic soda, hydrate of protoxide of iron, sulphate of iron, and caustic solution of soda, and he cautions against putting water upon the oil before the caustic solution of soda, since the effect would be to clog the wick and to make the paraffine oil give out a bad smell while burning. With respect to the paraffine, he states that 'it may be separated from the oil merely by pressure, in the same manner that stearine is separated from oleine.' The paraffine is then melted and filtered on a small quantity of animal black in a double funnel, heated by steam or boiling water, after which it is fit for making candles.

(To be continued.)

NOTICES OF PATENTS.

Fixing Pigments and other Colouring Matters on Textile Fabrics and Yarns. J. LIGHTFOOT, Accrington.

THE patentee prints on with glue, and, after steaming, passes the goods through a salt of mercury or lead. He also claims glue as a thickener, and fixing agent for murexide colours. According to this patent, colours may be fixed by printing with glue and nitrate of lead, and, subsequently, passing through ammonia and a salt of mercury. Printing on with glue and salts of alkaline metals is also claimed. The colours derived from aniline, &c., may be fixed, according to the patentee, by means of tannin or tanno-gelatine.

Artificial Fuel. J. H. JOHNSON, Lincoln's Inn Fields, London.

THE patentee takes coal-dust, peat, &c., and causes it to cohere by mixing it with glutinous paste or size. The mixture is then subjected to great pressure in moulds, and afterwards dried. It is evident that this method is, in every way, vastly inferior to the plan adopted on such an immense scale near Swansea, and in other places. We allude to mixing the coal-dust with gas-tar previous to forming it into blocks. The tar which remains in the blocks, when subjected to the high temperature of the drying ovens, yields a highly combustible fuel.

A Liquid, or Novel Preparation, to be Applied to Wools. W. E. GEDGE, Wellington Street South, Strand.

THIS patent is for a new fluid for oiling woollen yarns. It is, in fact, a fluid soap made by treating colza, olive, or other suitable oils with ammonia. As soon as the combination between the oil and the volatile alkali is complete, water is added, to render the whole sufficiently fluid to enable the fibres of the wool to separate properly.

Manufacture of Hydrated Oxides and Salts of Iron and Salts of the Fixed Alkalies. T. RICHARDSON, Newcastle-on-Tyne, and M. PRENTICE, Stowmarket.

THE patentees treat the oxides of iron with sulphurous acid; they then expose the solution to the atmosphere, by which means they obtain hydrated oxides precipitated, and sulphate of iron in solution. By treatment of a solution of iron obtained as above, with a solution of chloride of sodium or potassium they obtain, by double decomposition, sulphate of the alkali and chloride of iron. The latter they convert into hydrated oxides by treatment with lime, magnesia, baryta, or the carbonates of those earths. They thus get a precipitate of the desired hydrate and chloride of calcium, &c. in solution.

Treating Organic and Other Substances Containing Phosphate of Lime. T. RICHARDSON, Newcastle-on-Tyne.

THE patentee obtains soluble phosphates by treating phosphate of lime with sulphurous acid. The patent includes mixing porous substances with guano previous to decomposing by means of acids.

Substitute for Butter. B. HARBEN and R. J. HATHAWAY, Leadenhall Street, London.

THE patentees prepare a delicious compound as a substitute for butter, by mixing eight parts of fat, four of butter, and one of oil! The fat being first divested of strings or ligatures, is mixed with the butter and oil in a cold state, and, if desirable, salt may be added. Provisional protection only. We submit that this is an attempt at patenting a process for a disgusting adulteration of butter. The reason it was not proceeded with was,

probably, that the patentees became aware of the extreme want of originality in the very nasty idea for which they obtained protection. Most cheesemongers of middling honesty have been perfectly and practically familiar for many years with a mode of manufacturing "best fresh" from "stale salt" by a process not very different from the above.

Tanning. W. CLARK, Chancery Lane, London.
THE raw hides, prepared as usual, are deprived of hair by lime, and then cleaned. The skins are then steeped in a solution of an alkaline chromate for a period varying from six to twelve hours according to their thickness. They are afterwards washed in running water until quite clean. The patentee states that, by undergoing this process, the greasy matters of the skins undergo a transformation which enables the hides to absorb the tanning ingredients with greater readiness.

CORRESPONDENCE.

Silvering Glass.

To the Editor of the CHEMICAL NEWS.

SIR,—Having been for a considerable length of time engaged in a series of experiments for the purpose of depositing a thin layer of silver on the polished surface of a glass speculum, which I have prepared, without success, I should feel extremely obliged if any of your correspondents would direct me how to accomplish that object, or inform me where I could obtain the requisite information.

I have tried Drayton's method of silvering mirrors, viz.:—

NO ₃ AgO . . .	Two parts	} By weight,
HO	Three parts	
NH ₃ O	One part	
C ₁₂ H ₂₂ O ₁₁ . . .	Three parts	

reduced by grape-sugar, or oil of cloves; but the result was always a mottled surface of black matter and silver.

Another method was to dissolve gun-cotton in caustic potash, and drop into the solution crystallised nitrate of silver; redissolve by means of ammonia, and heat the mixture while on the surface of the glass. This produced a whiter metal, but the black residue spoiled the surface as before.

I have also tried a number of experiments with the silver salt dissolved in saccharic acid, glycerine, &c., but always without a satisfactory result.—I am, &c.

J. J. W.

The Adulteration of Food.

To the Editor of the CHEMICAL NEWS.

SIR,—I will once more trouble you with a few lines in reply to the more prominent points of "Verax's" letter on the above subject, published in No. 68 of the CHEMICAL NEWS, promising, however, that it is the last time I can accord any notice to critical communications where a mere *nom de plume* appears in place of a genuine signature; for, as another of your correspondents very truly wrote,—"truthful statements need no fictitious names."

I differ with "Verax" entirely as to the value of anonymous communications, for the general reader, at all events, will always look to the signature appended to any assertions, before he decides upon what amount of credence may be attached to the same. I cannot, either, permit another dictatorial point to pass without comment. "Verax" constantly associates himself and his opinions with those of the public at large, instead of bringing them forward in a simple, straightforward manner for whatever

they may be worth as his own well-founded convictions. It is not a little remarkable that of the large number of communications upon the adulteration question I have received within the last two months, not one has been in accordance with the notions of your correspondent.

If "Verax" chooses to pervert very obvious and common-sense statements to his own mysterious purposes, I will not be answerable for any deductions from my writings thus peculiarly contorted; as the fairest way of replying to his remarks on colouring matters, I will quote from my own paper (CHEMICAL NEWS, No. 62, page 94):—

"In the second table accompanying this paper, I have condensed some general information relating to the chief colouring matters used for purposes of (what might be called) ornamental adulteration, amounting to upwards of sixty in number, of which twelve or thirteen are active and powerful poisons—a like number are harmless, except in special cases of disease or morbid debility,—while the remainder are either more or less injurious to health, or we know too little about their effects upon the animal economy to venture a decided opinion either way."

I think it hardly possible to imagine a more definite or explicit statement than the preceding; and I cannot conceive what observation of mine can be considered to imply that I "regard all colours and essences employed to please the eye or taste, as poisonous."

With regard to the adulteration of bread, I may inform "Verax" that, as might have been supposed, my results are not founded upon the examination of samples from any one district, or class of vendor, or consisting of any particular quality; had it been otherwise, those results would never have been published by myself.

I have seldom heard a more useless query than that of "Verax," who requires to know the "average amount" of alum "found in a quartern?" and I will candidly own my utter inability to reply to the interrogation. As well might one inquire what is the average force or direction of the wind on the entire surface of the globe all the year round! Where the extremes vary so greatly, an arithmetical mean might of course be arrived at; but would such be of the smallest practical account?

I have found as little as nineteen grains of alum to the "quartern loaf"—i.e., *real* "quartern" loaf, weighing 4lbs., not the *theoretical* loaf, which is constantly several ounces short—and as much as 182 grains; and, of course, between these terminal points, every imaginable proportion. Perhaps, as a *very rough average* for the district of Bayswater, in the winter of 1859-60, I may give 72 grains of alum to 4lbs. of bread as a general proportion, and for St. Giles's 108 grains to the quartern.

If "Verax" ever possesses any samples of "bread made from pure wheat," characterized either by "brittleness of structure," or "crumbly texture," or any other bread samples without either of these properties, but containing respectively rice and potatoes in any quantity, perhaps he will be good enough to send me portions of the same; for, as a humble worker in the cause of science and truth, I am pleased to have the opportunity of investigating anything unusual.

I have throughout taken the word "adulteration" in its popular sense rather than in its more restricted meaning, as, for general purposes, I consider the former plan the most convenient, and one appealing best to the public mind.

At a future day the objections of "Verax," and others of a more important bearing, will probably be answered at length; now, however, I am content to reserve the discussion for a time, as I think a lengthened discourse on the special points brought forward would be wholly uninteresting to your readers. In reply to various other questions of "Verax," I must simply refer him to my paper and the tables relating thereto, in the hope that he will peruse the same a little more carefully than he seems to have done hitherto.

Analytical points I must altogether decline to discuss

with unknown critics, in accordance with my usual custom. In the present instance I retain the views formerly expressed by me.

With regard to the last point I deem it necessary to notice; I may inform "Verax" that I referred to various manufacturers, importers, and others, including among them Messrs. Huntley and Palmer, Messrs. Phillips, Messrs. Crosbie and Blackwell, &c., in my paper, for the reasons given therein, as I have examined the food products vended by these and various other firms in many ways for a long time past, and entirely without their knowledge. Only within the last few months have I had any direct communication with the parties in question for the purpose of acquiring certain information of a technical or statistical kind. Still, whatever assertion relating to his own works I may accept from a manufacturer—if borne out by my own researches—I should never dream of believing him if he told me that certain adulterations I had frequently detected were never used in his trade and city. Why, it is not so long ago that "one of the largest bakers" in London assured me that alum was "seldom, if ever, used now," while he was using it himself in the proportion of about 35 grains to the "quartern"!

The quotations of Dr. Odling's 64 samples from one district, which were said to contain no alum, must be taken for whatever they may happen to be worth. As something *per contra*, I may refer to Dr. Normandy's speech at the Society of Arts,¹ when he observed that he had found alum in every one of a series of 500 samples of bread he had examined not very long ago.

Thus much in support of the views "Verax" "conceives to be mistaken" ones, but which I hold are those of the great bulk of the public at large, however crudely and imperfectly they have been enunciated by me.

I am, &c. WENTWORTH E. SCOTT.
Baywater, London, W.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Some Compounds of Molybdic Acid.—Gentele describes several of these (*Journal für pract. Chemie*, Bd. lxxxi. s. 411). *Molybdate of Soda and Alumina*, $\text{Al}_2\text{O}_3 + 3\text{NaO} + 12\text{MoO}_3 + 22\text{HO}$, is formed when a solution of potash-alum is precipitated by an excess of molybdate of soda, and the precipitate is dissolved in hydrochloric acid. This double salt crystallises in needles, which, when dried over sulphuric acid, lose 7.9 per cent. of water, and when heated to redness, lose 16.5. It contains alumina 4.25, molybdic acid 71.63, and soda 7.51 in a hundred.

Molybdate of Alumina $5\text{Al}_2\text{O}_3 + 2\text{MoO}_3 + 33\text{HO}$ is precipitated when molybdate of soda is dropped into a solution of alum. When dried in the air it forms a white horny mass, which contains 3.43 of water. The ignited precipitate is composed of Al_2O_3 65.24 and MoO_3 34.76. After ignition the precipitate is only partially soluble in hydrochloric acid, and from a solution of the unignited ammonia only precipitates one-third of the alumina. When the unignited precipitate is dissolved in hydrochloric acid, and an excess of ammonia is added, the precipitate which is produced slowly dissolves again, which makes it probable that a soluble basic molybdate of alumina and ammonia exists.

Molybdate of Iron.—An excess of neutral molybdate of ammonia dropped into a solution of sulphate of iron, or potash, or ammonia-iron-alum, produces at first a cloudiness; but a precipitate falls on standing, and continues to increase for some time. It is of a pale lemon yellow colour, which on ignition turns green, and again yellow. It fuses at a great heat, molybdic acid escaping, and a greenish glass remaining behind. The precipitates formed with the

above iron compounds contained from 14.7 to 21.01 of iron oxide, and gave approximately the formulae $\text{Fe}_2\text{O}_3, 4\text{MoO}_3$; $\text{Fe}_2\text{O}_3, 5\text{MoO}_3$; $\text{Fe}_2\text{O}_3, 7\text{MoO}_3$. When a solution of either of the above-mentioned iron compounds is dropped into the solution of molybdate of ammonia, the latter remaining in excess, a thick gelatinous precipitate is formed. The solutions must be very dilute, or a stiff jelly will be formed, which it will be difficult to wash free from the molybdate of ammonia. The moist jelly shrinks so much in drying, that a quart when dried weighs scarcely 500 milligrammes, and then forms a yellow, horny, half-transparent mass. The analysis of one specimen gave 26.32, and another 25.37 per cent. of oxide, and 73.60, and 74.61 of molybdic acid.

Molybdate of Chrome Oxide.—A solution of molybdate of soda added by degrees to potash-chrome-alum produces at first no change; but, after a time, a cloudiness appears, which goes again on shaking. It is only after adding a quantity of the molybdate that the cloudiness is permanent and a precipitate falls. If all the chrome-oxide is not thrown down from the solution, the new compound, when dried, is of a clear malachite green colour. It has the composition $\text{Cr}_2\text{O}_3 + 3\text{MoO}_3 + 7\text{HO}$. When an excess of the molybdate of soda is added, the precipitate is of a darker colour, and has the composition $\text{Cr}_2\text{O}_3 + 2\text{MoO}_3 + 8\text{HO}$. Another compound $3\text{Cr}_2\text{O}_3, \text{MoO}_3 + 4\text{HO}$ is produced when a large excess of molybdate of soda and ammonia are added to the chrome alum at the same time.

Molybdate of Copper is precipitated when neutral molybdate of soda is added in excess to sulphate of copper. The precipitate is of a very pale green colour, and at first appears flocculent, but after washing it has a crystalline appearance. It has the composition $4\text{CuO}, 3\text{MoO}_3 + 4\text{HO}$. The author also describes some ammoniacal molybdates of copper.

On Benzile.—Zinin has before shown (see *Gmelin's Hand-book*, Vol. xii., pp. 174–184), that when benzoin is heated with chlorine or nitric acid, it loses hydrogen, and is converted into benzile. He now shows (*Bulletin de St. Petersb.* Tome iii. p. 58) how the benzile may be reconverted into benzoin without producing any by-products. Benzile is tolerably soluble in strong acetic acid, and the change is effected in this solution by means of metallic iron. The author dissolves one part of benzile in six parts acetic acid, sp. gr. 1.065, adds from one to two parts of iron, and boils until the fluid becomes a mass of slender white needles of benzoin, he then pours the pappy mass from the deposit of iron, after cooling, washes with water, and then re-crystallises from alcohol. In this way he obtains perfectly pure benzoin. It can also be obtained by acting on an alcoholic solution of benzile with zinc, and hydrochloric acid. On treating benzile with pentachloride of phosphorus, the author found that no hydrogen was displaced; but a new body which he calls chloro-benzyl, was formed $\text{C}_{25}\text{H}_{10}\text{O}_4 + \text{P Cl}_5 = \text{C}_{25}\text{H}_{10}\text{O}_4\text{Cl}_2 + \text{P Cl}_3\text{O}_2$.

Chloro-Benzyl (we adopt in this instance the author's orthography of the final syllable, to distinguish this body from Cahour's *chloro-benzile*) is very soluble in ether, and crystallises from the etherial solution, when evaporated at the ordinary temperature in large, colourless, transparent crystals two or three lines in thickness, but usually is obtained in short four or six-sided prisms. It is also soluble in alcohol. It melts under water or alcohol at 71°, and remains fluid after it has sunk to the ordinary temperature. It is decomposed by distillation, yielding a fluid, which among other things, contains some unaltered chloro-benzyl. The elementary analysis of chloro-benzyl led to the formula $\text{C}_{25}\text{H}_{10}\text{O}_4\text{Cl}_2$. Chloro-benzyl dissolves in strong nitric acid with the evolution of red vapours, and when the solution is mixed with water, pure benzile is precipitated. The reaction which takes place is explained by the following equation:— $\text{C}_{25}\text{H}_{10}\text{O}_4\text{Cl}_2 + \text{HNO}_3 = \text{C}_{25}\text{H}_{10}\text{O}_4 + \text{NHOO Cl}_2$. The action of caustic alkali on chloro-benzyl is remarkable. If hot alcoholic

¹ Not reported quite correctly in the *Journal of the Society and in these pages*.

solutions of both bodies are mixed together and allowed to cool, benzoate of potash crystallises out; but if the solutions are mixed with water, an oily body separates, which has all the properties of bitter almond oil. Aqueous ammonia has no action on chloro-benzyl; the reactions of alcoholic ammonia are very complex, and offer considerable difficulties.

III. ANALYTICAL CHEMISTRY.

Test for Grape Sugar.—Mulder gives the following (*Scheik. Onderz.* Deel iii. p. 25) as the best way of using his test for the discovery of grape sugar:—A little of the sulphate of indigo solution is placed in a test tube, the fluid to be tested is added, and the mixture is boiled. A solution of carbonate of potash or soda is then added until the liquid is alkaline. If grape sugar is present the mixture is decolorised, if not it remains blue.

MISCELLANEOUS.

Oxide of Chromium, when reduced to fine powder is one of the best reducing and polishing substances for metals known, and which we think is even superior to the finest emery for polishing steel. The best iridium-pointed gold pens become useless when used for writing signatures for a few hours over the green chrome ink that is printed on bank bills.—*Mining Chronicle.*

International Exhibition of 1862.—The following is the classification intended to be adopted by her Majesty's Commissioners for the Great Exhibition of 1862:—*Section I. Raw Materials.*—Mining, Quarrying, Metallurgy, and Mineral Products; Chemical Substances and Products, and Pharmaceutical Processes; Substances used for Food, including Wines; Animal and Vegetable Substances used in Manufactures. *Section II. Machinery and Engineering.*—Railway plant, including Locomotive Engines and Carriages; Carriages not connected with rail or tram-roads, Manufacturing Machines and Tools; Machinery in general, as applied to industry; Agricultural and Horticultural Machines and Implements, Civil Engineering, Architectural and Building Contrivances; Military Engineering, Armour, and Accoutrements, Ordnance and Small Arms; Naval Architecture and Ships' Tackle; Philosophical Instruments, and Processes depending on their use; Photography, and Photographic Apparatus; Horological Instruments; Musical Instruments; Surgical Instruments and Appliances. *Section III. Manufactures.*—Cotton; Flax and Hemp; Silk and Velvet; Woollen and Worsted, including Mixed Fabrics generally, Carpets; Woven, Spun, Felted, and Lace Fabrics, when shown as specimens of Printing or Dyeing; Tapestry, Lace, and Embroidery; Skins, Furs, Feathers, and Hair; Leather, including Saddlery and Harness; Articles of Clothing; Paper, Stationery, Printing, and Bookbinding; Educational Works and Appliances; Furniture and Upholstery, including Paper-hangings and Papier Maché; Iron and General Hardware, Steel, and Cutlery; Works in Precious Metals and their imitations; Jewellery; Glass; Pottery; Manufactures (not included in previous Classes. *Section IV. Fine Arts (Modern).*—Architecture; Paintings in Oil and Water Colours and Drawings; Sculpture, Models, Die-sinking, and Intaglios; Etchings and Engravings. In the Exhibition of 1851 there were only 35 Classes. In the proposed one, therefore, there are five additional.

New Colour—Indian Red.—Painters use very few colours of an organic nature. Those which they employ are usually in the state of lake, which are a combination of the colouring principle, with a metallic oxide. For some time past I have been occupied with chemical researches

on the colouring principles of organic origin, and have obtained one from sandal wood, which, by its beauty and its brightness, is nearly equal to carmine, and is of great interest to painters. This principle is a pure red solid, melts a little below 212° , and is afterward decomposed. It is insoluble in water and fixed oils, but very soluble in alcohol, ether, acetic acid and volatile oils. Dry chlorine has no action on it; wet chlorine destroys it. All acids—except nitric, chromic, and others which are rich in oxygen—have no action on it. Hydrosulphuric acid, which of all the gases is the most redoubtable test for such colours, has no action upon it; neither have light and air. Painters have found it to be a very solid colour. It was used to paint the carriages of the Emperor Napoleon about nine years ago, and the color is still as bright as when it was put on. Its preparation is easy, as follows:—Take the sandal wood in powder, and exhaust it completely by alcohol. In the alcoholic solution thus obtained, pour hydrated oxide of lead in excess. Collect the precipitate on a filter. Now, wash the precipitate with alcohol and dry it; dissolve it in acetic acid, and to this acetic solution add an excess of water. The colouring matter which is insoluble in water is precipitated, and the acetate of lead stays in the solution; and it may be used to make new oxide of lead. Now, wash the precipitate well and dry it at a low temperature. Researches on this colour—too long to relate here—have shown me that this colour is a peculiar principle, and is pure santalin. Its cost will be about 4s. a pound. I intend to make a new compound for dyers and calico-printers, extracted from santalin, capable of dissolving in water—a thing which has never been done before.—*Professor H. Dumas.*

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the CHEMICAL NEWS, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 6d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part III. of the present Volume is now ready.

* * All Editorial Communications are to be addressed to Mr. CROOKER, and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the CHEMICAL NEWS, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

J. Lym.—We regret that we cannot assist you.

F.G.S.—1. We believe not. 2. The report was published and may be obtained on application to a bookseller. 3. Not to our knowledge.

A.B.C.—The value of commercial may be estimated near enough for practical purposes by distilling a known quantity, and collecting apart all that comes over below 212° F. To estimate it with accuracy it would have to be repeatedly submitted to fractional distillation, which is a most tedious operation.

J.M.W.—Snowdon diamonds are transparent crystals of quartz. The cubical crystals you speak of as occurring in the trap rocks are most likely iron pyrites; if so, they will be of a golden colour, and very brittle.

Manufacture of Varnish.—A correspondent asks if Gum Copal or Gum Anime is first melted and run into water according to Mr. F. F. Tingley's method, and then again melted and mixed with boiling Linseed Oil to make a copal varnish? Or can the Gum Copal and Gum Anime be melted in a gum-pot, and mixed direct with boiling Linseed Oil, according to Mr. J. W. Neil, who obtained the Gold Isis Medal of the Society of Arts in 1851? And is there a flux that can be used with the gum resins so as to render those gums more easy to mix with Linseed?

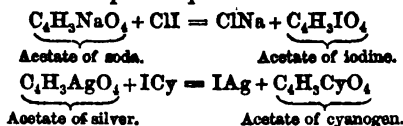
THE CHEMICAL NEWS.

VOL. III. No. 71.—April 13, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Substitution of Electro-Negative Bodies (Chlorine, Bromine, Iodine, Cyanogen, Sulphur, &c.) for the Metals in Oxygenised Salts: Production of a New Class of Salts in which the Electro-Negative Bodies replace the Basic Hydrogen, by M. F. SCHUTZENBERGER.

THE idea struck me that by making certain compounds, such as the protochlorides of iodine, bromine, or sulphur, iodide of cyanogen, &c., react on oxygenised salts, I should succeed in substituting the radical bromine, iodine, sulphur, or cyanogen (which, in these compounds, is electro-positive) for the metal of the salt by simple double decomposition, which may be represented by easily generalised special equations:—



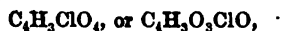
In these analogous products, the first of which is isomeric with iodic acid, the iodine, instead of being substituted for the equivalent of hydrogen of the radical acetyl takes the place of the basic hydrogen.

It is easy to foresee that bodies of this class, if they can exist, should be characterised by quite special properties, and, above all, great instability. Experiment has not failed to verify my surmises, and if I have not yet had time to give to my reactions the desirable degree of generality, I have carefully studied them on a number of compounds sufficient to enable me to predict with certainty that it is possible to prepare the oxygenised salts of chlorine, bromine, iodine, cyanogen, &c., in the same manner as salts of potassium, lead, mercury, &c. are obtained.

Here is a condensation of the facts on which I support these general conclusions,—facts described in detail in the account sent to the Academy:—

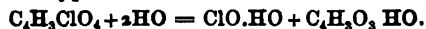
Anhydrous acetic and hypochlorous acids united at a very low temperature, in the proportions of $\text{C}_4\text{H}_3\text{O}_3$ to ClO , mix together and turn red. In a quarter of an hour the mixture decolorises spontaneously without any other apparent reaction, and without altering in weight. An excess of hypochlorous acid imparts a fixed red tint to it, which disappears when this excess is removed by gentle heat (30°).

The analysis of this liquid gives numbers corresponding to the formula,

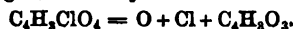


which is that of monochloroacetic acid; but the characteristics of these products do not allow of its being confounded with the latter. By its mode of formation and properties, I consider it an acetate of chlorine.

Acetate of chlorine is a liquid, colourless, or of a faint, pale yellow. It dissolves immediately, and in any proportion, in water, and turns into a mixture of hydrated acetic and hypochlorous acids,



Towards 100° C. it detonates violently, and yields chlorine, oxygen, and anhydrous acetic acid,



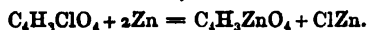
It may be preserved in ice and darkness without undergoing any alteration; but at the ordinary temperature and in the sun it decomposes, little by little, and the bottle-stoppers are forced out violently. It can be distilled *in vacuo* at a low temperature.

Mercury attacks it when cold with a rustling noise. It disengages much chlorine, and forms acetate of mercury and a little calomel,—



Here we have the singular phenomenon of the displacement of chlorine by a metal.

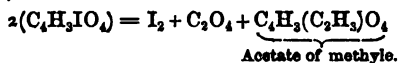
Zinc attacks it very slowly, and produces a mixture of acetate and chloride of zinc,—



Iodine instantly dissolves in it, decolorising itself and disengaging chlorine, without producing chloride of iodine. It forms solid, white, crystalline acetate of iodine, isomeric with iodic acid, but the characteristics of which are entirely different to those of the latter acid,—



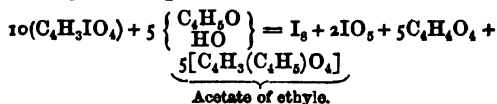
Acetate of iodine decomposes by heat above 100° C., almost always with an explosion, according to the equation,—



Water destroys it immediately, according to the equation,—



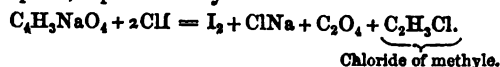
Absolute alcohol also decomposes it when cold, according to the equation,—



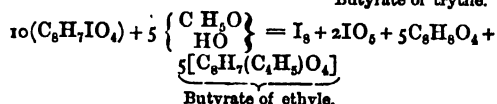
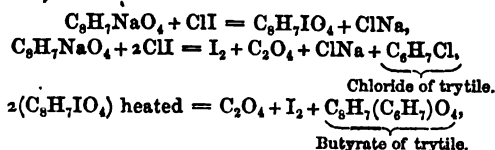
which corresponds to the preceding one.

In mixing equal equivalents of protochloride of iodine and dry acetate of soda, the mass becomes slightly heated, the strong odour of the chloride of iodine almost entirely disappears, and a mixture of marine salt and acetate of iodine is obtained. This mixture, from which it is impossible to separate the acetate of iodine, presents, in fact, under the influence of heat, water, and absolute alcohol, all the reactions formulated above. If there is

an excess of chloride of iodine, a secondary reaction takes place, represented by



The action of chloride of iodine on butyrate of soda is exactly parallel, and can be explained by a similar equation,—



When cold, bromine dissolves in acetate of chlorine; the chlorine being disengaged little by little; the mixture, at first red, becomes at last perfectly colourless,—

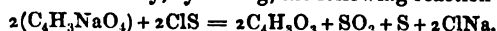


The product thus obtained is liquid, and, after some time (one or two hours), decomposes spontaneously, with an explosion. Iodine dissolves in it, freeing the bromine and forming solid acetate of iodine.

Sulphur dissolves with a rustling noise in acetate of chlorine, disengaging chlorine, but it is separated from sulphurous acid, and anhydrous acetic acid and sulphur remain,—



According to this, acetate of sulphur appears less stable than the other negative acetates; in fact, by the action of chloride of sulphur (ClS) on dry acetate of soda, we obtain immediately, by cooling, the following reaction:—

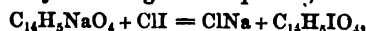


which, being so pure, could be utilised with advantage for the preparation of anhydrous acetic acid.

Benzoate of soda and chloride of iodine heat slightly when mixed; the odour of the chloride of iodine disappears. If heated further it disengages much carbonic acid; iodine distills, and a liquid insoluble in water and potash, which can be separated by fractional distillation.

1. Into a colourless iodised liquid, boiling at 200°C ., the composition of which, according to the analysis already made, appears to be that of iodide of phenyle ($\text{C}_{12}\text{H}_9\text{I}$).

2. Into a solid body very like naphthaline. According to this the first production of benzoate of iodine takes place probably according to the equation,—



and it remains for me to study more closely its products of decomposition. I shall endeavour equally to produce benzoate of chlorine, and with this pure benzoate of iodine.

By slightly heating a mixture of equal equivalents of acetate of silver and iodide of cyanogen, the mass melts, producing iodide of silver, disengaging nothing; but, by raising the temperature, the product decomposes, often with an explosion. These facts, of which a more detailed account will be given in another paper, are easily intelligible, by admitting the production of acetate of cyanogen,—



which would be decomposed at a higher temperature.

Anhydrous sulphuric acid absorbs anhydrous hypochlorous acid, yielding a stable, dark-red liquid; the excess of anhydrous acid can be separated from it by distillation without its disengaging the faintest trace of chlorine; but at a higher temperature sulphate of chlorine decomposes energetically.

These facts, I think, suffice to support my ideas, but I own that my task is far from being accomplished. The complete Memoir contains, moreover, some new researches on protochloride of iodine, which, for want of space, it is impossible to reproduce here.—*Comptes-Rendus*, vol. lii.

Observations on the Presence of Certain Elements, ordinarily very rare, in Common Substances, by M. H. SAINTE-CLAIRE DEVILLE.

I HAVE previously shown that vanadium is found in those aluminous matters so abundant in nature, and the researches of M. Beauvallet and M. Terreil have proved that the commonest clay contains this hitherto rare metal. The method I have employed for the demonstration of this unexpected fact has enabled me to detect in certain relatively abundant minerals the existence of several substances rarely found in mineral formations. I will now detail successively certain substances which I have studied from this point of view.

1. *Rutile*.—The rutile of Saint Yrieix, which is now found ordinarily in commerce, has a very curious composition, which I am about to indicate whilst exhibiting the analytical method which I have applied to it, for the purpose of resolving the special problem I have imposed on myself. Heat pulverised rutile, with three or four times its weight of hydrate of potash mixed with a little nitre. This should be effected at a red heat, and, when large quantities are operated upon, in an iron vessel.

Treat the cooled pulverised mass with water, when there remains insoluble pertitanate of potash, oxide of iron, and silico-aluminate of potash, &c. A dark green alkaline solution is also obtained, from which the colouring principle (manganese) can be separated by a few drops of alcohol. The liquid, filtered and treated with excess of sulphuretted hydrogen, assumes a beautiful red purple. A little titanous acid, and sometimes coloured sulphides, are precipitated. Filter the liquid, and exactly saturate it with hydrochloric acid; heat slightly, and when all the sulphuretted hydrogen has disappeared a heavy brown powder is separated on filtering, which roasts in a platinum vessel. A fusible and deeply coloured substance is then obtained, and a crystalline white sublimate.

The fusible matter is vanadic acid, recognisable by all its most marked characteristics: coloration of phosphorus-salt, green in the reducing flame, and yellow in the oxidising flame, then by nitre into a red-orange; easy formation of a deep yellow chloride, which is produced with disengagement of chlorine in contact with hydrochloric acid, and which assumes an azure blue tint, when the excess of hydrochloric acid is driven off. The volatile matter is molybdic acid, which in the reducing flame colours phosphorus salt blue-black, and with phosphoric acid and ammonia yields an insoluble yellow phospho-molybdate, &c.

100 grammes of rutile yields by this method 1.030 grammes of roasted sulphides, or else a mixture of acid, having the following composition:—

	Grammes.
Titanic and stannic acid, with platinum from the vessels	0.211
Vanadic acid	0.323
Molybdic acid	0.486
	1.020

It will be perceived that rutile is a substance from which vanadium may be extracted with great advantage.

2. Cerite of Batnas.—When 100 grammes of cerite are treated with potash and sulphuretted hydrogen in excess, by the process just described, we obtain by the precipitation of a sulpho-salt by means of hydrochloric acid and roasting in close vessels at a low temperature, an acid mixture of the following composition:—

	Grammes.
Acid	0.050
Acid and traces of vanadium	0.510
	0.560

This proportion of telluric acid is furnished by the 100 grammes of cerite. For the cerite I am indebted to M. Bahr, the learned Professor of Chemistry in the Stockholm Mining School, who had the kindness to send me considerable quantities. It is with these valuable materials that M. Damour and myself have prepared the elements for a work, now in course of execution, on the metals so numerous and so difficult to separate, discovered by the illustrious Danish chemist, M. Mosander.

3. Wolfram.—I have ascertained the presence of small quantities of tantalic acid in the Wolfram of St. Leonard, but in such small proportion that I should not have mentioned it here if this fact did not confirm the discovery of M. Damour of tantalic acid in the Wolfram of Chanteloube in the Limousin.

4. Cryolite.—In cryolite, also, I have found very small quantities of a substance possessing all the characteristics of hyponiobic acid. I have, however, not obtained sufficient to examine it in all its reactions. But my opinion on this subject is corroborated by the fact that niobite has been discovered in Greenland cryolite. I have sent a beautiful specimen of the mineral, found in large fragments, to the *Ecole des Mines*.—*Annales de Chimie et de Physique*.

On the Saccharine Matter contained in Acid Fruits: its Origin, Nature, and Transformations, by H. BUIGNET.

THE following is a *résumé* of a long paper on this subject which appears in the February and March Numbers of the *Annales de Chimie et de Physique*:—

From the experiments performed during my investigations, the following conclusions may be drawn touching the nature, origin, and transformations of saccharine matter in fruits:—

1. The sugar which forms originally in acid fruits is cane sugar, $C_{12}H_{22}O_{11}$, and in its properties and rotatory power is identical with that obtained from the cane or beetroot.

2. During the maturation of fruits this sugar undergoes a peculiar influence, and is gradually converted into inverted sugar, $C_{12}H_{22}O_{11}$, identical in its qualities and rotatory power with that obtained by the action of acids or glucose ferment on cane sugar.

3. If, when the fruit is perfectly mature, we examine the saccharine matter, we shall find it differently consti-

tuted in various fruits. Sometimes it is composed of pure and simple inverted sugar, as in the grape, gooseberry, and fig; sometimes it contains a mingling, in various proportions, of cane sugar and inverted sugar, as is the case with the pineapple, apricot, peach, plum, &c.

4. The cause of this variety is not, as we may well believe, the acidity of the fruits. Experience shows that organic acids, by reason of their relative proportion, their diluted state, and the feeble temperature in which they act, have little power to change the cane sugar with which they are associated. Moreover, there is no connection between the acidity of fruits and the changes exhibited in their saccharine matters. In the lemon, which is excessively acid, more than a quarter of its saccharine matter exists in the state of cane sugar: whilst in the fig, which has scarcely any acidity, the whole consists of inverted sugar. In the apricot, peach, and Mirabelle plum there is about 70 per cent. of cane sugar; while no trace of it is found in the grape and cherry, which, upon analysis, are found to contain a much smaller proportion of acid.

5. The differences presented in the relative proportions of the two sugars seem to be due to a nitrogenised matter, acting as a glucose ferment, analogous to that recently extracted by M. Berthelot from brewers' yeast. By treating crushed gooseberry seeds with cold water, a liquid is obtained which, when cold, changes to inverted sugar the cane sugar contained in the juice of the fruit.

6. The comparative influence of the acid and ferment is exemplified by two parallel experiments made on the juice of the same fruit,—one in which the ferment is precipitated by alcohol; the other in which the acid is neutralised by carbonate of lime. In the first case, the sugar remains a very long time without sensible modification. In the second, on the contrary, it is completely transformed, even in twenty-four hours. The same result is obtained by experiments made on the fruit of the banana. No trace of free acid is to be found in its juice, examined at any period of its vegetation, and yet, if artificially ripened, about two-thirds of the saccharine matter is in the state of inverted sugar.

7. There is an affinity so close between cane and inverted sugar that it is with difficulty that they can be separated. Thus, cane sugar loses its crystallising property when associated with even a very small proportion of inverted sugar. Further, it is thus that protoxide of lead, which acts so differently on the two sugars separately, yet exercises the same action on them when they are mixed together.

8. The best method for separating cane sugar from the fruits which contain it is that indicated by M. Peligot for the analysis of molasses, and consists in forming a saccharate of lime, separating it by boiling, and decomposing it by a current of carbonic acid. It is impossible to obtain any sensible quantity of this sugar in a crystallised state without repeating the treatment with lime, and rendering alcoholic the syrupy solution from which it ought to separate. Observing this twofold condition, I have been able to obtain crystallisable sugar from the peach, apricot, Mirabelle plum, apple, &c.

9. The abundance of starch in the Vegetable Kingdom has led to the supposition that this substance is the true source of the saccharine matter of fruits. Nevertheless, it is impossible to discover its presence in green fruits, either by the microscope or by iodised water. On the other hand, the sugar to which the starch gives place in the artificial transformations that we can make it undergo is a right-handed glucose, with a rotatory power

equal to $+53^{\circ}$; whilst it results from the experiments detailed in this Memoir that the sugar found in acid fruits is cane sugar, either wholly or partially inverted.

10. In green fruits a peculiar principle exists, possessing the property of absorbing iodine with even more energy than starch does, and of forming with this metalloïd a perfectly colourless compound. This principle is of an astringent nature, and appears to be allied to tannin in many of its properties. It can be estimated with as much facility as the saccharine matter itself. We know, by experimenting upon it at different stages of maturity that its proportion diminishes as that of the saccharine matter augments.

11. By adding to the juice of green fruit as much iodine as it is capable of absorbing, a precipitate is soon formed by the combination of the iodine and the astringent matter. If we collect this precipitate and carefully wash it, in order to disembarass it of anything soluble it may have retained, it will form sugar under the influence of weak acids and a suitable temperature.

12. The sugar extracted from the tannin of the gall-nut, by the action of sulphuric acid, of medium concentration and of suitable temperature, is a right-handed glucose, having exactly the same rotatory power as starch glucose. The sugar furnished by the tannin of green fruits, under the same conditions, is also a right-handed glucose, identical with sugar of starch. In this respect tannin affords no better help than starch to a satisfactory theory of the origin of sugar in fruits.

13. Much starch and tannin are found in green bananas, and the two principles diminish progressively and simultaneously, so that no trace of either of them is to be discovered in the ripe fruit. The sugar found in their stead is cane sugar.

14. There exists, then, an essential difference between the products of Art and of Nature with respect to the transformation of either tannin or starch into sugar. There is also a marked difference between the saccharine matter of fruits, according to whether it is produced by the action of vegetative force or irrespective of its influence. Experiments prove that the sugar which continues to be formed in bananas after they are gathered is not cane, but inverted sugar.

TECHNICAL CHEMISTRY.

On Copper Amalgam, by H. DUSSANCE.

I have read in many scientific and medical Journals of the discovery of a new alloy by Mr. Oersheim, which alloy has plastic properties, and is an amalgam of copper. This alloy is not new; for in 1853 Professor Peligot, in his lectures at the Conservatoire Imper. des Arts et Metiers, at Paris, spoke of it. At that time I was his assistant, and prepared great quantities of it, and analysed it.

This alloy presents the remarkable property of being soft when recently prepared, and after a short time it becomes so hard that it is difficult to break it to analyse it. Dentists used it to fill teeth, but I believe its use was abandoned. On account of its property of being soft at first, its greatest use is for the moulding of medals. The best way to prepare it is the following:—

1. Take sulphate of copper and dissolve it in water acidulated by a few drops of sulphuric acid; put in this bath a sheet of iron, perfectly clean; the copper is precipitated in the form of a reddish powder.

2. Wash that powder quickly with hot water, and, after mixing it well in a mortar with a few drops of nitrate of mercury, wash it well again with hot water.

3. Add mercury to it in a mortar, and mix it well, until it has the form of a paste that can be worked with the fingers.

4. Wash well with hot water till the washing liquid is perfectly clear.

5. Press the alloy in a deer-skin till all the excess of mercury is out, and expose to the air. In less than an hour it becomes very hard.

A singular property I must notice is the action of zinc on this alloy. If the mercury contains a little zinc the alloy becomes very hard, but in breaks in pieces by the least knock.

Here is the analysis of it:—

<i>Analysis in the Ordinary State.</i>			
Copper	.	25'75	25'81
Mercury	.	74'25	74'19
		100'00	100'00
Density	.	11'48	

<i>Analysis when Soft.</i>			
Copper	.	25'43	
Mercury	.	74'57	
		100'00	
Density	.	11'033	

Analysis after it has been Soft and Hard again.

Copper	.	25'48	
Mercury	.	74'52	
		100'00	
Density	.	11'036	

Such is the composition of this alloy, which is given for a new discovery, and which was described seven or eight years ago, and was called *alloy of the dentists*.—*American Journal of Pharmacy.*

The Society of Arts' Annual Exhibition of Inventions.

(Continued from page 214.)

No. 92. *Method of Coating Electric Telegraph Wires*, exhibited by Joseph Rogers. The conducting wires are coated with a suitable insulating material. Strips of woven fibre, tapes, or felt, previously saturated with a compound that furthers the insulation, are then bound round. Cords or wires, which have been previously covered with yarn, and prepared by a heated cement, are laid round in one or more series; if more than one series, then each alternate series is laid in an opposite direction, and bound round with wire, yarn, or woven fibrous material, or braided over, the binding wires being used to regulate the gravity and protect the cable from abrasion. No. 93. *Submarine Telegraph Cables*, by the same Exhibitor. The conducting wire, or strand of conducting wires, is first insulated with india-rubber, or gutta-percha, or both; they are then surrounded with a strengthening of flat plaited bands or cords, which have been previously prepared by immersion in a cauldron of heated cement, and are bound round and compressed in one or more series previous to receiving an external braiding of yarn, which is again saturated with a compound which renders the cable impervious to water or the attacks of animalculæ. If more than one conductor is used, they are laid together and bound round with strips of woven fibre, tape and thread,

but braiding over is preferable; any number of conducting wires can be enclosed. No. 133. *Patent Mica Garden Labels*, exhibited by Frederick Leiss. These labels are manufactured of the mineral mica, in transparent laminae. The inscriptions being between sheets of this substance, and so protected, stand all degrees of heat, cold, or humidity. No. 134. *Patent Gravity Potato Selector*, exhibited by James Anderson. The finest, mealiest, and most nutritious potatoes are always denser and heavier than the soft and waxy. By taking advantage of this difference in their specific gravity, the light and inferior potatoes are made to swim on the surface of a solution of salt, while the heavy and good sink to the bottom. By this contrivance the dry and mealy potatoes are separated. In order to classify potatoes into three qualities, good, medium, and inferior, let a solution having a specific gravity of 1.100 be put into one jar and a solution having a specific gravity of 1.080 into another. Very good potatoes only will sink in the first jar; the medium and inferior will remain floating on the surface. Let these be removed to the second jar, containing the solution of the lowest density; the medium quality will sink in it while the inferior will remain floating on the surface. This instrument is based upon a curious fact, which may perhaps be sometimes made use of in private houses. We doubt, however, if it would "pay" the greengrocer to sort out his goods in this manner. No. 130. *Hancock's Patent Butter Machine*, exhibited by Andrew M'Laren and Co. This machine is for purifying butter from acid and buttermilk, and also for cooling it in warm weather, without touching it with the hand. The butter is taken from the churn and placed in a cylinder, and pressed with a screw piston through a perforated plate at the bottom of the cylinder, with a tub of cold water. This operation may be repeated, and the butter will be thereby chilled and purified in the hottest weather. Butter having been thus treated will keep good much longer than when it is worked by the hand, as the heat of the hand imparts a greasy character to the butter, which militates much against its keeping quality, even if the hand could extract the buttermilk from the butter as well as this process. No. 140. *Artificial Stone, Building Bricks, &c.*, exhibited by C. Paine, Dippenhall Silica Works. These imitations of Bath stone, bricks, and brackets, are all made of the building material "Soluble Silica," which is stated to be more durable than natural stone, and less costly. It can be made of any colour. No. 148. *Carbonic Aerial Water and Air Purifier and Cooler*, exhibited by Joshua Jackson. This apparatus consists of a series of disks, fitted with pure wood charcoal, and arranged one above another (either suspended on chains or fixed on a stand) to the number of six, or more or less according to the requirement of the case. Above these disks is fixed a reservoir to hold the impure water. This reservoir is usually a bell-shaped vessel, with a valve or tap at its convex end. Below the disks is a receiver, which is either a jug, pan, or tub, or else a vessel similar to the reservoir. Above the purifiers is a disk, called a filter, which is either placed between the purifiers and the reservoir, or inside the latter. When the valve or tap is opened in the reservoir, the impure water flows through the filter, which retains its mechanical impurities, and then through the whole series of purifiers (which absorbs its gaseous impurities) until it falls into the receiver. While passing through the purifiers, the water is exposed to the cooling and refreshing action of the air, which gives the apparatus an advantage over an earthen or glass filter, which excludes

the air. When this apparatus is suspended over a burning lamp, it absorbs the noxious gases arising from the combustion of the oil, gas, or spirits, and also any such gases that may be floating in the air, thus keeping the air in the apartment sweet and fresh. When the purifiers are saturated with impurities, they must be boiled in water for half-an-hour, which must be repeated, if necessary, until they become as pure and efficient as when new. This apparatus is peculiarly applicable to rain-water. No. 158. *Patent Heat Regulator*, exhibited by C. Varley. The object of this apparatus is to ensure any desired uniform temperature in ferneries, forcing-houses, greenhouses, or any chamber heated by gas. There is a tube to the upper end of which is attached a chamber, composed of a series of tubes to increase its surface. The other end of the tube is bent into a U, and has a small chamber, with a stuffing-box attached to it; the lower portion of this tube is filled with mercury; the remaining portion of the longer limb of the tube and chamber is filled generally with spirits of wine; there is a smaller tube sliding through the stuffing-box into the larger tube, with a fluid joint, to allow of the motion of the smaller tube. The gas flows from the main through the small tube into the larger one, and out between the two tubes, and thence on to the burners which heat the chamber. As the temperature of the chamber rises, the fluid contained in the larger tube and in the chamber expands, raising the mercury; and when it has raised it to a certain degree, the mercury closes the mouth of the smaller tube, and cuts off the gas. On the temperature falling, the mouth of the smaller tube is unclosed, and the gas turned on. A small burner, in direct connection with the main, is kept constantly burning, to re-light the gas when turned on, or it can be arranged that the flow of gas is only lessened, and never turned off. There is a graduated scale, over which an index attached to the small tube slides, and by sliding this tube up or down the temperature is determined at which the gas shall be turned on and cut off. There is a modification of this apparatus, where, instead of sliding the small tube nearer or further from the surface of the mercury, the adjustment to the desired temperature is obtained by raising or lowering the mercury which forms the valve nearer or further from the mouth of this tube, by means of a plunger worked with a rack and pinion. By this arrangement all stuffing-boxes and moveable joints are avoided. The apparatus is made entirely of iron, and as there is nothing to get out of order, there is no possibility of derangement. Modifications of this apparatus are used to control the temperature of chambers heated by other means, and they are made of various sizes, according to the purpose for which they are applied. No. 173. *Improved Manufacture of Cork Stoppers*, exhibited by A. B. Seithen. The novelty consists in that the corks, instead of being cut, are ground, which gives them any desirable shape, a much smoother surface, with less waste in the material. No. 174. *Improved Artificial Corks*, by the same Exhibitor. The corkwood is cut into fine veneers, and rolled up to the desired size, the end being cemented, thereby producing, from thin wood, large and stout stoppers, of an even elasticity and porous substance, while those cut by hand have a good and a bad side, according to the different growth of the material employed. These are each very useful contrivances, and will be found of great value to the chemist. No. 184. *Steane and Palling's Patent Apparatus for Preventing Candles from Drooping and Gutting*, exhibited by John Gedge and Son. This apparatus consists of a small metal tube with projecting

arms, and a metal or porcelain cap. The tube is first passed over the wick of a candle, and afterwards the cap, which rests upon the arms of the tube supported by the candle; as the candle burns the tube becomes heated, and forms a well of fluid tallow or composition near the wick, and the weight of the cap resting upon the arms of the tube, it descends with it as the candle burns. The result is, that the outer circumference of the candle forms a border, over which the tallow will not run, even if the candle be moved rapidly from place to place, and the cap prevents the flame overlapping on to and guttering the candle. No. 213. *Patent Plastic Leather*, exhibited by R. Seager and Co. This is a compound of leather, india-rubber, and gutta-percha. Leather shreds are submitted to an alkaline solution under gentle heat, until reduced to a fibrous condition; they are then rubbed through a wire cylinder; the fibre so obtained is then placed in a masticator with gutta-percha, india-rubber, or a mixture of both, the proportions being according to the texture of the material required; if pliable, more india-rubber; if hard, more leather. The pliable material is adapted for shoe bottoms, being damp-proof, and stated to be more durable and less expensive than common leather. That which is hard and rigid is adapted for picture frames and other ornamental designs, either plain, bronzed, or gilt, as shown by the specimens exhibited; its colour may be so blended as to resemble wood carvings. No. 219. *Safety Medicine Bottles*, exhibited by William Toogood. These bottles are made with contracted necks, so as to allow the fluid to flow in drops; some of them have their necks ground in and moveable for filling; others have an opening at the bottom, for filling, which is closed by a cork, while others are made of dark coloured glass, with fluted sides.

PHARMACY, TOXICOLOGY, &c.

Analysis of Commercial Glacial Phosphoric Acid, by JOHN M. MAISCH.

WITHIN a few years past, phosphoric acid and its compounds have attracted the attention of medical practitioners, and are now extensively used, frequently in the form of solutions and syrups, in which free phosphoric acid is employed for keeping the earthy and metallic phosphates in solution. In many of the formulas of these preparations, the glacial phosphoric acid, as it occurs in commerce, is assumed to be the monohydrated HO, PO_3 ; but those who have made them will have frequently noticed that sometimes the phosphoric acid employed is unable to dissolve the same quantity of phosphates which at other times is taken up by it. As a reason for this, we generally assign the presence of variable proportions of fixed earthy matters, which cannot be volatilised by the fusion of the acid; and it is a fact that almost all of it, the way we meet with it in commerce, yields a white precipitate on being over-saturated with ammonia.

At the suggestion of Professor Procter, I have taken up the subject with the view of ascertaining the nature and the amount of these impurities, and the state in which the acid exists ordinarily.

In the first place, my inquiries were directed to obtain glacial phosphoric acid from different manufacturers; in this I was unsuccessful. By inquiring of a number of friends, I ascertained that they are all using an article made by Merck, of Darmstadt, a number of whose

chemicals enjoy a well-merited reputation among us. This induced me to select at random from a larger lot of this acid, three specimens, which were washed with cold water, and dried by means of bibulous paper.

On neutralising an aqueous solution of glacial phosphoric acid, to effect which the generation of heat has been carefully avoided by placing the dish in cold water, ammonia will sometimes throw down no, or only a slight, precipitate, which is increased by the subsequent application of heat. The precipitates thus collected and heated amounted to 3.18, 4.16, 0.80 per cent., and in a fourth instance no precipitate at all was obtained. In the first case I had collected from 550 grains of acid 45 grains of precipitate, and intended to use this for a quantitative analysis; but meeting with an accidental loss of a portion of it, I am only able to give the qualities of these impurities.

The precipitate was dissolved in diluted muriatic acid, and the solution filtered; a residue of silicic acid remained on the filter amounting to 1.5 grains = 0.273 per cent. of the phosphoric acid, or 3.333 per cent. of the weight of the precipitate. The filtrate was mixed with strong alcohol, and sulphuric acid dropped in as long as a precipitate was produced, which was separated by a filter and well washed with alcohol. The acid filtrate, from which the alcohol had been evaporated, yielded a granular white precipitate on being over-saturated with ammonia, which was insoluble in chloride of ammonium; carbonate of potassa produced no change in the cold, but, on boiling, a white precipitate, which was soluble in acetic acid, and this solution was not precipitated by oxalic acid, but yielded a white granular precipitate on the addition of chloride of ammonium, ammonia, and phosphate of ammonia in succession. The filtrate, therefore, contained phosphate of magnesia.

The residue on the filter was wholly soluble in a large quantity of water, and this solution was affected by reagents in the following manner:—Phosphate of soda, turbid; after a while, white precipitate; oxalate of ammonia, immediately a white precipitate, insoluble in acetic acid; ammonia, ferro- and ferrid-cyanide of potassium did not disturb it. These reactions prove the presence of lime, and the absence of alumina, and of iron, and other heavy metals.

The whole precipitate was therefore composed of silicic acid, phosphate of lime, and phosphate of magnesia. I must mention here that a portion of the same crystals was tested with chloride of barium and caustic potassa, but neither sulphuric acid nor ammonia was found in any of the specimens examined.

When the glacial phosphoric acid, dissolved in cold water, is immediately over-saturated with ammonia, filtered from the precipitate, and mixed with a solution of chloride of ammonium and magnesium, a precipitate of ammonio-phosphate of magnesia occurs only of that portion of the phosphoric acid which has, by the action of a moist atmosphere and the consequent gradual absorption of water, been converted into the terhydrate or the common phosphoric acid; an excess of the double chloride produces in the filtrate no further precipitate, until it is heated to boiling. By setting the solution aside, the meta- or pyro-phosphoric acid is gradually converted into the common tribasic acid, but this change is effected in a short time on the application of heat. To determine whether all the acid has been changed in this manner, it will suffice to add more of the double chloride to the ammoniacal solution and boil again; this must be continued until a precipitate ceases to be separated.

But inasmuch as meta- and pyro-phosphate of lime and magnesia are kept in solution by the corresponding phosphate of ammonia, it will be necessary to boil the original solution of the acid for some time, allow it to cool, and supersaturate it with ammonia. The filtrate must then be repeatedly boiled, and some ammonia must be added to the cooled liquor until no further precipitate occurs. We may then be sure that all the earthy phosphates have been separated. The combined precipitates are now exposed to heat until they cease to lose weight; their weight is then deducted from the original weight of the phosphoric acid, and the remainder may be looked upon as the weight of pure hydrated phosphoric acid, provided that in the beginning we have satisfied ourselves of the absence of sulphuric acid and ammonia. Though many works on chemistry recommend to test for nitric and hydrochloric acids, I have never, either on any former or the present occasion, found either of these two; but sulphuric acid I have in one or two cases met with, not however in Merck's.

The ammoniacal solution of phosphoric acid is then precipitated with a solution of chloride of ammonium and magnesium, and after standing for several hours, filtered. The precipitate is washed with a diluted solution of ammonia, until the washings, when oversaturated with pure nitric acid, produce no precipitate in nitrate of silver.

To determine from the precipitate the quantity of anhydrous phosphoric acid, I have followed two ways: if its quantity was large, I have dried it upon a brick tile at ordinary temperature, ascertained its precise weight, ignited a convenient portion of it, and then calculated the weight which would have resulted from the ignition of the whole. If the quantity of the precipitate was small, the whole of it was ignited, together with the filter, the amount of ashes of which was ascertained by incinerating a portion of the same sheet of paper, and deducting the weight of ashes from the whole weight obtained. The result in either case is pyrophosphate of magnesia, 2MgO , PO_5 , which contains 63.33 per cent. of PO_5 .

The following Table contains in the first column the amount of phosphoric acid employed; in the second, the weight of impurities; third, the per-centage of the same; fourth, the weight of 2MgO , pPO_5 ; fifth, the weight of PO_5 ; sixth, its per-centage of the pure hydrate; and in the seventh column the per-centage of the free PO_5 in the specimen:—

I.	II.	III.	IV.	V.	VI.	VII.
Grs.	Grs.		Grs.	Grs.		
550	45.0	8.1818	609.86	386.12	76.46	70.20
100.75	0.8	.794	122.77	77.75	77.79	77.19
48.20	none	—	63.22	40.04	83.48	83.48
		4.16				

Pure metaphosphoric acid, HO , PO_5 , contains 88.806 per cent. PO_5 , and the best specimen which I have examined contains 5.32 per cent. less, while between the highest and lowest there is a difference of 13.28 per cent. in the available acid, and after the deduction of the earthy phosphates, the difference is still 7.02 per cent. It cannot be doubted that the large amount of impurities found in the first specimen is accidental, and probably derived from the vessel in which the phosphoric acid was fused; the purity of the other two specimens, taken out of the same jar, speak in favour of this supposition, and it appears to me, on account of this great difference, as if most likely different portions of the acid, fused at the same time in one vessel, may contain a variable proportion of impurities.

From the amount of water alone no conclusion can be arrived at as to the proportion of the three phosphoric acids present at the same time. To ascertain this, an acid, known beforehand not to contain any impurities, after its solution in cold water and over-saturation with ammonia, may be precipitated with chloride of ammonium and magnesium; this precipitate will contain all the common phosphoric acid. The acid of the filtrate is then to be converted into the ordinary variety, and precipitated as before; this precipitate will now contain all the meta and pyrophosphoric acids, the amount of each of which can be easily calculated from the remaining weight after the deduction of the terhydrate.—*American Journal of Pharmacy.*

PROCEEDINGS OF SOCIETIES.

SOCIETY OF ARTS.

Wednesday, March 20, 1861.

Professor H. W. WILLIAMSON in the Chair.

(Continued from page 221.)

In France, M. Selligie had obtained tars by distilling the bituminous schists of Autun, and the paper coal and bituminous slate of the coal formation. He also purified the oils, and produced burning fluids, which had a considerable sale in France. In 1847, Mansfield obtained a patent for the separation and purification of volatile liquids from tar, and his benzole (which he was the first to obtain, nearly pure, on a large scale) met with a ready sale as a solvent of caoutchouc. Mr. Lowe had, a few years before, applied the lighter portions of coal naphtha to increase the luminosity of coal gases; and naphthalized gas was for a time a familiar term.

In 1849-50, works were erected at Weymouth, in Dorsetshire, for carrying out Du Buisson's process. Works were also carried on near Wareham, in the same county, by the Bituminous Shale Company, and they advertised the sale of oils from shale as—"Oil No. 1, or volatile; Oil No. 2, or rough oil, which contains paraffine, grease, and varnish; Oil No. 3, or machine oil, which contains paraffine, grease, and varnish, greasy liquid, alkaline ditto manufactured, asphalt or varnish." It appears, also, that Mr. John Thomas Cooper, the Consulting Chemist, in 1847, prepared paraffine oil, lubricating oil from paraffine, and paraffine itself, from the distillation of coal. In 1849, the Boghead Cannel became known, and was used as a source of paraffine and paraffine oils. Mr. James Young's patent bears date 1850, and its origin may be referred to about the year 1847, when Dr. Lyon Playfair called Mr. Young's attentions to a petroleum spring in Derbyshire, which he thought might produce some oils useful to the manufacturer. On examining this spring, it was found to be in an old coal mine. The coal had been worked out, and from the roof, which consisted of sandstone, there exuded a thick, dirty-looking oil, known as petroleum or rock oil. Mr. Young obtained from it some useful lubricating oils, and as the supply was but limited, he invented a process for obtaining rock oil from coal, the varieties best suited to his purpose being parrot-coal, cannel-coal, gas-coal, and, lastly, Boghead mineral. Mr. Young is entitled to the merit of having invented a number of useful processes for the separation and purification of these oils, without apparently being aware of what had already been accomplished in the same branch of industry. Mr. Young, with his partner, Mr. Meldrum, erected works on the Boghead estate, 15 miles from the Forth, for the advantage of being within reach of the Boghead cannel, or, as it is now called, the Torbanehill Mineral. Their process is as

follows:—The retort is placed vertically, with a hopper on the top; the fire is applied by a kiln below; the retort is cased with fire-brick for about three-fourths of its height, so that the heat passes through this to the retort. As the coals come down, they are distilled by the heat, and the products are passed through a worm pipe into a refrigerator, where they are condensed. The first product is described as a crude oil, containing paraffine. It is distinguished from tar in not drying by exposure to the air, and in being lighter than water. The rectification is by the application of sulphuric acid and soda, with two or three distillations. About two-thirds of the oil is run off as oil, part for burning and part for lubricating. The solid remainder contains paraffine, from which it may be extracted by the ordinary methods of purification.

In the Great Exhibition of 1851, Mr. Young obtained credit, and justly, for the production of paraffine in considerable quantities. The former processes, although containing all that was necessary for its production, were not commercially successful. Either they do not produce the article in sufficient quantity, or at a sufficiently low cost to be profitable. Mr. Young has the great merit of first drawing public attention to the commercial value of paraffine, and of pointing out the sources from which it could be profitably extracted.

In Germany, paraffine works were started in 1855, at Beul (near Bonn), Ludwigshafen, and Töplitz, and paraffine was soon applied to many uses, and met with a large demand. Manufactories were also established in France and Austria, and most extensively in several parts of the United States of America.

In the last-named country the first attempts were made on the bituminous shales of Dorchester, New Brunswick. Extensive works were erected at Brooklyn, near New York, Pittsburgh, Baltimore, and at several places along the Ohio valley and river. The oils produced are in great demand, exceeding till lately the supply, as solvents, lubricators, and for illumination. They give a whiter and more brilliant light than any fixed or fat oil, and are much cheaper. Hence they limit the demand, and keep down the price of fish, lard, and sperm oils; but complaints have been made that the oil is sent out in a crude and impure state, retaining much of the tar and creosote impurities.

The term Photogen has been applied to the oils or naphthas obtained from shales, brown coals, cannel, and especially the Boghead variety, and used for illuminating purposes. It is also known as paraffine oil. It differs in specific gravity from ordinary coal-oils of the same boiling point, varying from .820 to .830, while common coal naphtha has a density of .850. When photogen has a higher density than that indicated, and a very high boiling point, it probably contains paraffine. The photogen may also be obtained from ordinary bituminous coals, by distilling them at a temperature of about 700° C. Coal is broken small, heated in iron retorts, and the tar is received through a very wide worm into tanks. In some cases the coals are distilled by the heat of their own combustion, or by a modification of Lord Dundonald's plan. The tar thus obtained is distilled, and the lighter portions, after purification, form what is called Boghead naphtha, or as, in Germany and elsewhere, the distillate is divided into two portions, the more volatile being photogen, and the less volatile solar oil. Both oils are purified by concentrated sulphuric acid, which removes the highly coloured and odorous constituents of the distillate, and also by an alkali, which removes carbolic acid and its congeners, together with the remains of the sulphuric acid and some sulphurous acid. In this country the heavy and light oils are mixed together, so as to produce a fluid of medium density and volatility. The more volatile hydro-carbons give great inflammability and fluidity, and they are also more odorous than the less volatile portion of the distillate, which is the true paraffine oil.

At the establishment near Bonn, the lignite of the neighbourhood, known as leaf or paper-coal, is distilled at a low red heat in iron retorts. A blackish tar and ammoniacal liquid are the product; the former is said to yield 90 per cent. of oils, 50 per cent. of which are thin enough to burn in lamps. They are purified by means of sulphuric acid and alkaline leys.

Wagenmann's process is with bituminous coal, which is broken into small lumps, sprinkled with milk of lime, to get rid of sulphur, dried in a desiccating furnace, and distilled in retorts. The liquid products are received into a large reservoir, kept at a constant temperature of 30° Centigrade, where the tar separates from the ammoniacal liquor, and the latter being mixed with the residue of the large retorts, furnishes a good manure. The tar is pumped up into the purifying apparatus, mixed with sulphate of iron, and distilled in vessels of the capacity of 350 gallons, by means of super heated steam. The products are condensed in a leaden coil, and consist of volatile liquids of the specific gravity of .700 to .865, heavy oils for lubrication, .865 to .900 and paraffine, .900 to .950. These three substances are treated each with 4, 6, and 8 per cent. of sulphuric acid, 1½th and 2 per cent. of hydrochloric acid, and 1 per cent. of acid chromate of potash, with which they are agitated for half an hour. They are then left for three hours, poured off the dregs, and mixed with 2, 3, and 4 per cent. of a ley of caustic potash (50° B.). Lastly, each of the products thus purified is placed in a still, and distilled by superheated steam. No. 1 mixed with No. 2, so as to obtain the specific gravity of .820, produces the mineral oil or photogen, which is burnt in lamps fitted for the purpose. Part of the product distilled from No. 2 having a specific gravity .860 to .700, forms the solar oil, which may be burnt in Argand or Carcel lamps; the rest of No. 2, mixed with some of the product of No. 3, furnishes the lubricating oil for machines. The remainder of No. 3 is introduced into a vat, where the temperature is lowered till it crystallises. In the course of three or four weeks, the paraffine crystallises in large tablets, and is separated from the adhering oil by means of centrifugal machines, making 2000 revolutions per minute. The paraffine is melted and rolled into squares, and subjected to a strong hydrostatic pressure. It is then again melted, and treated with 50 per cent. of strong sulphuric acid, at a temperature of 180° C. In two hours the paraffine separates from the acid, and is washed with water; it is then run into cakes, and pressed in the hydrostatic press while hot between two layers of hair cloth. It is once more melted, mixed with 5 per cent. of stearine, and maintained for some hours at a temperature of 150° C. in a leaden apparatus. It is lastly mixed with one per cent. of a solution of caustic potash (40° B.), and at the end of two hours all the impurities will be separated, and the paraffine may be drawn off as limpid as water.

In a valuable article on photogen, by Mr. Greville Williams, in the last edition of "Ure's Dictionary," is a table showing the materials employed for distilling these oils, with the per-centage of tar of the oils and of paraffine. The raw material consists of Trinidad pitch, the Boghead cannel, or Torbene mineral of Scotland, Dorset shale, Belmar turf from Ireland, George's bitumen from Neuwied, paper coal and brown coal from various parts of Germany, and Rangoon tar. But of all the substances hitherto employed none are so rich in results as the last.

This brings us to speak of some of Nature's distillations. Various bituminiferous productions are formed in vegetable matter during its conversion into coal. Among these are mineral oil, an inflammable fluid, sometimes forming powerful springs, and frequently occurring in carboniferous deposits. Likewise naphtha, a transparent and nearly colourless liquid, burning with a copious flame, and strong odour, and leaving no residuum. Springs of naphtha may burst forth during mining operations, as in the coal-shale

of Derbyshire, where, in driving a level, a large quantity of this liquid poured forth and covered the surface of the water in the level, and being accidentally set on fire, it formed a burning spring which lasted several weeks. There is also petroleum, a dark coloured substance, thicker than common tar, which rises in immense quantities from some of our own coal beds, and from the carboniferous strata of some parts of Asia and elsewhere. Petroleum springs do not seem to depend on combustion, as has been supposed, but to be simply the effect of subterranean heat. And it is not necessary that the depth should be very great beneath the surface to give a temperature equal to the boiling point of water, or of mineral oil. In such a position the oil may be supposed to suffer a slow distillation, and so find its way to the surface, or it so impregnates the earth as to form springs and wells, as in Persia and India. Petroleum springs are those whose waters contain a mixture of petroleum and the various minerals allied to it, such as bitumen, naphtha, asphaltum, and pitch. They are very numerous, and in many cases undoubtedly connected with subterranean fires which raise or sublime the most subtle parts of the bituminous matters contained in rocks. Petroleum springs occur in abundance in Modena and Parma, in Italy, but the most powerful of which we have any account are those on the Irrawadi, in the Burman empire. In one locality there are said to be 250 wells, which yield 400,000 hogsheads of petroleum annually. The heat by which these chemical changes and transformations produce from vegetable matter such enormous quantities of petroleum, may also cause this substance to be forced up to the surface, in its native state, where by exposure to the air it becomes inspissated, and forms the different varieties of pure and earthy pitch or asphaltum, as in the case of the vast accumulations in the island of Trinidad. Bitumen is an inspissated mineral oil, of a dark brown colour, with a strong odour of tar. It sometimes occurs of the consistence of jelly, bearing some resemblance to soft india-rubber, and as it will remove the traces of a pencil, it has been named "mineral caoutchouc." Native bitumen and the substance of that name obtained from certain varieties of coal, when heated, if not identical, are closely allied, or yield products which are so. The varying proportions of bitumen which different varieties of coal are capable of yielding, depend on the amount of change which the vegetation has undergone since its deposition. In some cases the internal heat, accompanied by moisture and great pressure, has expelled all the volatile matter, and reduced the coal to the state of anthracite. The amount of volatile matter in coal and bituminous minerals may vary from 10 to 63 per cent., forming the different varieties of dry and fat coal. The bitumen of coal resembles the bitumen of nature, known as asphalt and mineral tar, in its sensible qualities and general appearance; but it does not contain the same proximate principles; nor does it by dry distillation yield the same fluids. "They belong, however, to the same natural group or series, and tend to strengthen the opinion generally held that bitumen, petroleum, and asphalt, arise from the decomposition of fossil vegetation. The natural bitumens always contain some volatile oil ready formed, and their varieties depend on the greater or less proportion of this volatile oil present in them."

Petroleum, the Lecturer said, is the most abundant source of paraffine and photogen. The wells of Pennsylvania afford from ten to fifty barrels of oil per diem, or even more. Last year oil was discovered about fifty miles from Cleveland, Ohio, when some fifty wells were at once sunk, and afterwards several hundreds, yielding from ten to sixty barrels daily.

The Burmese naphtha, or Rangoon tar, was next noticed. Here we find the following description of the separating processes:—

"The Rangoon tar obtained from peat, coal, &c., have been formed by their decomposition.

"In the first place, the Rangoon tar is distilled with steam at 212° Fahr. The distillate contains a mixture of a number of volatile hydro-carbons, which it is difficult to separate on account of the diffusible nature of the vapours, however different their boiling points. In practice a second or third distillation is adopted, and the products are arranged according to their boiling points or densities, which vary from 627 to 860, and although they all originally came over with steam at 212° Fahr., their boiling points range from 80° to upwards of 400° Fahr. All these liquids are colourless, and solvents of caoutchouc. Those of low specific gravity are known as Sherwoodole and Belmontine. They have considerable detergent power, removing stains from silk without impairing delicate colours. The distillate of higher specific gravity burns with a brilliant white flame, and as it cannot be ignited without a wick, forms a useful lamp fuel. Sometimes these lamp fuels may not be sufficiently fluid at ordinary temperatures to ascend the wick. Messrs. Price and Co. have introduced a modification of the old cocoa-nut oil lamp for overcoming this difficulty. A piece of metal is continued from the metal wick-holder down into the reservoir, and by its conducting power gently raises the temperature of the fuel.

"A small per-centage of hydro-carbons of the benzole series comes over in the first operation. By treating it with nitric acid, nitro-benzole, and other substances valuable in perfumery, are eliminated.

"After the first distillation, about three-fourths of the original material remains. This is fused and purified from extraneous ingredients by means of sulphuric acid. A black precipitate is thrown down, which, after being washed, resembles native asphaltum. The purified fluid is removed to a still, and heat is applied by steam through heated iron tubes. The distillates are arranged according to their distilling points, from 300° to 600° Fahr. Those obtained at 430° Fahr. and upwards contain paraffine.

"It is stated in the Memoir above referred to that at 212°, 11 per cent. of fluid hydro-carbons distil over, entirely free from paraffine; that between 230° and 293°, 10 per cent. more fluid distils over, containing only a very small quantity of paraffine; that between 293° and 320° the distillate is very small in quantity; but from that to the fusing point of lead, 20 per cent. more is obtained, and although containing an appreciable amount of paraffine, it remains fluid at 32°. At this point of the distillation the products begin to solidify on cooling, and there is obtained 31 per cent. of a substance of a sufficient consistency to be submitted to pressure. On raising the heat considerably, 21 per cent. of fluids and paraffine distil over. In the last stage of the operation, 3 per cent. of pitch-like matters are obtained, the residue in the still, consisting of coke containing a little earthy matter, amounts to 4 per cent.

"The results of this distillation are given in the following table:—

At 212°, free from paraffine	11.0
230 to 293°, } a little paraffine	10.0
293 to 320°, }	
320 to fusing point of lead, containing paraffine, but still fluid at 320°	20.0
At about the fusing point of lead, sufficiently solid to be submitted to pressure	31.0
Beyond fusing point of lead, quantity of paraffine diminishes	21.0
Last distilled, pitchy matters	3.0
Residue in still, coke containing a little earthy impurity	4.0
	100.0

"The above distillates are all lighter than water. Most

of the paraffine may be separated from them by exposing them to a freezing mixture, so that no less than between 10 and 11 per cent. of this valuable solid hydro-carbon may be obtained from the Burmese naphtha."

Our last quotation shall be the concluding paragraph of Mr. Tomlinson's paper, which terminated somewhat abruptly:—

"Bolley states that most of the commercial paraffines contain stearic acid, that when paraffine is melted, it is readily acted on by chlorine, when it gives off bubbles of hydrochloric acid gas, and retains some of the acid tenaciously. In the compound thus formed, some of the hydrogen is replaced by chlorine; it is tolerably soluble in benzine, and the solution may be readily spread on paper, wood, &c. The name of Chloroaffine has been proposed for this substance. Bolley says that he has met with paraffine with as high a melting point as 149.9°, and Laurent has found it as low as 91.4°."

"Leaving the question of the composition of paraffine to be settled by competent authorities, there can be no doubt as to the value of this beautiful substance as a candle-making material. The illuminating power is considerable. It is stated that a paraffine candle, weighing one-eighth of a pound, will give as much light as a spermaceti or stearine candle weighing one-sixth of a pound. The low temperature at which it fuses, and the high temperature to which it may be raised without decomposition, namely, upwards of 600° F., point it out as a valuable material for a bath for chemical purposes. The lubricating properties of the fluids obtained in the second distillation have already been referred to. Fixed oils are decomposable into acids which corrode metals; the paraffine oils have no such property, so that they do not corrode the metal bearings of machinery, nor is the brass work of lamps injured by the hydro-carbons burnt in them. As paraffine is not acted on by acids or alkalis, it has been recommended to guard, by means of paraffine, the stoppers of bottles holding those substances. A paraffine paper for holding caustic alkali has also been suggested. And, lastly, we may remark, that paraffine realises the desideratum pointed out by Liebig many years ago, of a solid olefant gas, although it is somewhat singular, that in the last edition of his 'Chemical Letters,' that great chemist still appeals to science to produce his favourite solid carburet of hydrogen."

The discussion upon the preceding paper would present but few special points of interest to our readers, even if the space at our command permitted us to report it, which, however, is not the case. The proceedings terminated with the usual vote of thanks.

CHEMICAL SOCIETY.

Thursday, March 21, 1861.

Professor BRODIE, F.R.S., President, in the Chair.

Messrs. H. O. Huskisson, A. C. McLean, W. V. Simons, and E. B. Brown, M.D., were elected Fellows.

Dr. WILLIAMSON read the following paper, "*On Thermo-Dynamics in Relation to Chemical Affinity.*" The subject which I have the honour of bringing before the Society is one which does not belong exclusively to the domain of chemistry: it involves of necessity considerations derived from other departments of science, which are not always taken notice of by chemists. I may, perhaps, be permitted, before entering upon the subject itself, to premise a few words, merely in order to describe the position of the previous question upon which I found my remarks, for the purpose of ascertaining whether my understanding of it agrees with that of the other members of the Society. I allude especially to those admirable results which have attracted the attention of the whole scientific world, in England and abroad, for many years past, as being amongst the most important of modern

times. I mean the relation between what are usually called the "forces;" the first fact being, that when one of the forces disappears, some other force is—to use the ordinary term—"produced" in its place; that, in fact, there is no such thing in nature as *destruction* of force, any more than there is such a thing as *destruction* of matter. Of course that is well known to all the members of the Society, but the part of that subject to which I have especially to allude, and the results which, more especially, I shall have the honour of laying before you, are those which have been elicited by the research of Mr. Joule and other distinguished gentlemen as to the relation between mechanical force—the motion of masses—and heat. I shall take for granted, in the remarks I shall make, that you all believe that heat is nothing else than the motion of small particles of matter. I am not aware of any serious doubt having been raised, by the investigators of that subject, in regard to that proposition. I shall, therefore, assume that to be the case in my remarks; and that a certain definite amount of mechanical force is always produced where heat disappears, provided there be nothing else produced at the same time but mechanical force. Of course the members are all aware that Mr. Joule first determined with accuracy the weight falling through a certain distance,—the number of foot-pounds, as he designates them, which correspond to a certain quantity of heat. He determined that the unit of heat, which is one pint of water heated one degree F. (the standard usually adopted in this country), if transformed completely into force, would give rise to 772 foot-pounds of force; that is, that a pound would be raised 772 feet; or that 772 pounds would be raised one foot, which is the same thing. As a matter of fact, he found that when a pound weight fell from 772 feet towards the earth, it consumed that power in friction; and when he measured the amount of heat evolved, it corresponded to one degree of heat; calorimetrically designated.

Now, the term "latent" which is familiar to everybody, as applied to heat, has for some time been chiefly applied to certain cases of the disappearance of heat, and the production of something else; but I think it would be desirable, on account of the resemblance of other cases of the disappearance of heat and the production of other forces, to extend the term, which is used only in certain cases, to those other cases in which a similar process occurs. I will not give here the particular instances in which I should suggest the extension of the term; I will only mention a couple by way of illustration. For instance, not only would I apply it to the fusion of ice and the evaporation of water, inasmuch as we can regain heat when we solidify water or condense steam; but I would similarly apply the term to the expansion of the permanent gases. If we had a certain quantity of air in a cylinder, and if I were to raise the piston of that cylinder so as to expand the air, I should have a certain fall of temperature in that enclosed air, and the heat contained thus in that air has to be distributed equally in a greater space. I should consider that the heat in that case became latent. But more especially would I consider it important chemically to apply the term to those cases of the disappearance of heat in which the chemical force is overcome whilst heat disappears. Thus if we decompose water by a galvanic current, and if we determine the quantity of heat which makes its appearance during five minutes of the passage of the current; and if we then remove this liquid and put in a thin wire of such length that it should have the same resistance as the liquid to the passage of the current, and if we then measure the amount of heat in the wire, it is well known, from the researches of Mr. Joule, Mr. Wood, and others, that in the first experiment, where we decompose the water, we get considerably less heat evolved than in the second. We do not get the same quantity of heat as we do in the second experiment where we cause this force to act upon the resisting wire through which the current is forced to pass. It is stored up, in

fact, in the oxygen and hydrogen gases which are evolved during the former process. Now, I would submit that cases of that kind ought as much to be called cases of the absorption of latent heat, as those transformations to which we are accustomed to apply the term. I will give you one more instance before I leave that point, because it is one of extensive occurrence; and that is, the ordinary process of the growth of plants in which it is well known that by an action, unfortunately unknown to us at present, upon the surfaces of what I was going to call the "lungs" of plants—the leaves of plants, the rays of the sun cause a process which is the opposite of combustion, a process by which oxygen is evolved, and combustible bodies—wood and so forth—made; and there is no doubt that heat is absorbed as much as in the other processes to which we are accustomed to apply the term "latent heat."

Now, in chemistry, the physical basis which we all adopt for our own reasonings upon the more difficult transformations is that of the atomic hypothesis. We do admit that there is a limit to the divisibility of matter, and it is to be assumed that masses of matter such as we can deal with are built up of certain small particles which we call "atoms." I have no intention of entering into the evidences of this theory, because, of course, they are quite as well known to you as to myself; I merely allude to it in order to admit frankly that I use that theory, and that it is a necessary condition of the truth of the arguments which I shall have the honour of laying before you. If that theory be wrong, of course what I shall say will fall to the ground; but I believe I am not over-stating it when I describe it as the only physical theory at present given for the transformations of matter.

Now, from many points of view, we chemists are bound in all our reasonings to take at the commencement facts which have been established in the study of other phenomena; because, after all, the matters we have to deal with—solids, liquids, and gases—are the same as natural philosophers have to deal with; and, if any one truth has been ascertained by natural philosophers, or "physicists," as we call them, regarding matter, that truth is equally true for chemists; and I say, therefore, that if it be proved by physicists that certain forces are acting upon the small particles of matter by which displacement of them must occur, and if it be proved by physicists that certain small particles must and do exist, I say that chemists have no right to shut their eyes to the fact, and to reason upon the constitution of matter as if it were built up of little particles which are absolutely at rest. However, without drawing *a priori* such a conclusion, I would rather beg leave to admit what I conceive most members of the Society must be prepared to consider reasonable, that these small particles of matter—gaseous, liquid, and solid matter—are, under all circumstances in which we have to deal with them, relatively to one another in a state of vibration. That is admitted, I believe, by everybody—of gases; it is also admitted, usually, of liquids, but with regard to solids I believe that many persons experience some reluctance to admit that the constituent atoms can be in a state of motion relative to one another. It is not natural for some persons to conclude that a solid which retains, for a certain time, any particular form should have its particles in motion, for one would think that in such a case it would alter its form or that it would yield to pressure; yet a number of cases, all the results of such motion, are known to everybody; and I submit that the belief of the constituent atoms of any solids being at rest relatively to one another is inconsistent with well-established facts derived from natural philosophy, and even from chemical considerations; and I shall therefore extend this hypothesis to the three different states of matter, and not confine it to the liquid and gaseous alone.

Now, some years ago I had occasion to propose a theory

by which the decompositions of two salts which are dissolved or liquified by any process can be represented in accordance with these considerations. I will briefly state an outline of them; and what I shall endeavour to show to the Society will be not only the convenience of this mode of representing the constitution of matter in itself, and its accordance with what we know of chemical affinity; but also the confirmation which it receives from the results of our observations in other departments of knowledge.

Now, in order to represent the movements of atoms by which we obtain the results which we designate by the general name of "chemical affinity," it is necessary to take by way of example, the case of two salts, which, for convenience, shall be of a constitution somewhat analogous to one another, and to assume that they are mixed with one another in a liquid form, either by fusion or by solution in water. I will, by way of example, first take the case of hydrochloric acid and nitrate of potash—



and I will take the old atomic weights. Now, when these two bodies are mixed, and when the reaction of the one upon the other commences, the interchange of materials causes the formation of two other compounds—chloride of potassium and nitrate of hydrogen.



I conceive it will be accordant with the ordinary acceptance of the term "affinity," if I say that the greater affinity exists in the new compounds; for, in comparing the quantities of two bodies which co-exist in the same circumstances, the greater affinity will be at work, in holding together those that remain in greater quantity. I only judge of the force by its effects, and I conceive that the only measure we can have, and the only way in which we can judge, of the relative forces which will hold together different compounds which are mixed with one another is that of comparing the quantities of the two which remain with one another. It will be clearer by taking an example. For instance—supposing the hydrochloric acid and the potash to unite, I will call that interchange by which they decompose one another, the "decomposing change," as that is a convenient term; and let us assume that the forces, by way of example, which hold together the hydrochloric acid and the nitrate of potash, and the forces which hold together the elements in the other compound are equal to, in the first example, the forces which hold together the products of their decomposition—chloride of potassium and nitrate of hydrogen. Now, I say in that case the quantities of the original salts, and of the products of their decomposition will of necessity be equal, if the affinity which holds together the atoms of the original substances is equal to the affinity which holds together the atoms of the new salts. If, on the contrary, there are less of the products of decomposition contained in the liquids after once it has gained a state of equilibrium than in the original compounds, I should then say that the affinity of the elements for one another, according to this new arrangement, is less than the affinity of the elements in the old arrangement of the particles.

Let us suppose another numerical case. I really do not know in this case what the proportion is, for we have no numbers; but let us assume that the affinity of the substances, chloride of hydrogen and nitrate of potash, is twice as great as the affinity of the same elements for one another in the form of chloride of potassium and nitric acid and water. The result would be, that as soon as the mixture of the original salts had been left for a sufficient time, and the equilibrium had established itself between the decomposing forces, and the reproducing forces of the original substance, that we should have, according to the starting-point which I have adopted,

twice as many of the original atoms as of the compound formed by the decomposition. Now, that being premised, the dynamical consideration, or the consideration of the process of the motion of the atoms, which we identify as chemical affinity, to which the formation of the new compounds is due, has to be established in the following manner:—By way of illustration, suppose we have 100 atoms of the original compounds, and that I only get 50 atoms of the products of decomposition; and that this proportion is the result of the forces which hold these together being twice as great as the affinity of the elements in the new compounds for one another. Let us assume that under these circumstances no further change in the composition of the liquid takes place, and that no further disturbance in the proportions occurs; now, the fact that hydrochloric acid in the liquid is constantly decomposing, nitrate of potash would disturb its equilibrium, and the fact that chloride of potassium and nitric acid are interchanging would also disturb it in the opposite direction; and if this equilibrium is maintained undisturbed, we must admit that these 100 atoms of hydrochloric acid are re-acting as many times in the unit of time, and that these 100 atoms of the original compound are interchanging bases as often in the same time as these 50 atoms of the products of decomposition are interchanging bases so as to reproduce the original compound. I mean to say that the per centage of atoms which are decomposing so as to reproduce the original bodies must be as much greater as the actual number of atoms in these products of decomposition. I conceive in that statement I am making no hypothesis whatever. If it be true that salts when dissolved in liquid are interchanging bases constantly so as to form a product of decomposition of this kind, and if it be also true that this process of interchange stops at a certain point, and does not change the whole of the original salts, but only goes so far as to decompose a certain proportion of them, then the equilibrium must be owing to the fact of the equality of those products,—that is to say, the number of atoms in the original salts multiplied by the proportion of them must be equal to the number of the products of decomposition multiplied by the per centage of those which are interchanged in the unit of time, so as to reproduce the original compounds.

The only general theory of chemical action which has been worked out to any considerable extent that I can at present allude to is that of Berthollet, which explained the decompositions which occur by an assumption which has prevailed in the infancy of many branches of science, and that is the assumption of a metaphysical principle called "predisposing affinity." I dare say most of the members of the Society will agree with me that that is merely a name—and not a very good name, perhaps—for our ignorance of a process. For instance, when we say of the action of zinc and hydrochloric acid that the predisposing affinity of the acid makes zinc decompose water which it could not have done by itself, it is certainly a method of reasoning which, although excusable in the infancy of science, is not excusable at the present time. We should, therefore, put away such entities, and disuse such principles by simply stating the fact that zinc does displace hydrogen and unite with chlorine. The illustrious Berthollet said that when two salts are mixed, and when, by the interchange of bases, one or more insoluble products can be formed, the knowledge that such results would ensue induces the salts that you mix to decompose when they would not otherwise. Berthollet's theory has generally been used in connection with predisposing affinity. On the old assumption, chloride of silver would be produced by hydrochloric acid decomposing nitrate of silver in solution,—the nitrate of silver undergoing decomposition from the "looming in the distance," I suppose, of the consciousness of this result. I wish to show how, in this case, we can, by simple

statement of fact, explain those results, and explain the beautiful general law of Berthollet, that, when we mix two salts, which, by interchanging bases, would form an insoluble salt, of course that result takes place. Of course the salts interchange bases. This is well known to be a truth, so established as to amount almost to a natural law. Now, according to this principle, we see at once, without any assumption whatever, why the result which Berthollet points out must ensue. If the hydrogen and silver interchange, producing chloride of silver and nitrate of hydrogen, the chloride of silver being an insoluble body gradually falls to the bottom—gets out of the way of the nitric acid, preventing a change by which the original compounds would again occur. Thus this fact of the insolubility of one of the products prevents that process by which the original bodies would, under ordinary circumstances, be gradually reproduced in proportion as they were decomposed; and I conceive that that is one, and not an unimportant, use of the application of this general fact of the motion of particles in solutions of this kind—a motion by which analogous particles are constantly undergoing a certain interchange.

Now, from other considerations in chemistry we are unfortunately not able to derive as exact information as we need, numerically, regarding the relative quantities of heat contained in matter; I mean, not even the specific heat of elements which have been studied with great attention and care by the distinguished Berthollet and Regnault, and others. I conceive that the only direct physical estimate we can have at present of the quantity of heat or motion contained in the particles of any amount of matter must be derived from a measurement of the quantity of heat which it absorbs in undergoing a given rise of temperature—one degree Fahr., for instance,—and in this subject our information is unfortunately defective in some respects, owing, among other reasons, to the great variations in the state of matter under different conditions: for one and the same body has been found to possess a different specific heat at different times; and it has not been as yet possible to follow out the connection between the different states of one kind of matter. Doubtless much of the disagreement of the results we have on that important subject is owing to the insufficient observations of the different conditions under which the different observations were made. For instance, one would be disposed at first to say that those metals which require the most heat to pass into a different form contain most specific heat. But, unfortunately, our measurements have only been made within the limited range of the thermometer. We do not know the quantity of heat absorbed by any substance when it is gradually warmed from the observed zero up to a given temperature; and it is more than probable that the actual specific heats which we observe in elements and in compounds, at temperatures under which we observe them, are very different, and do not stand to one another necessarily in the same relation as the total quantities of heat contained in the substances stand to one another; because some bodies absorb heat and increase their specific heat, more rapidly than others; therefore, by change of temperature certain differences may occur. However, taking the specific heats of bodies as we possess them, I think, allowing for all these imperfections, and others, we have notwithstanding a very advantageous light upon our subject. In the first place, we are able to compare the proportions of one and the same element in different states. Take the element carbon, for instance, which occurs in a variety of forms, some more easily igneous than others, and capable of combining more readily with oxygen, and burning very readily, as in the form of lamp-black; the same element occurring in other forms, such as coke, plumbago, or diamond itself, which possess chemically this property in a very much less powerful degree. It is well known from the researches of M. Regnault, M. Silbermann, and others, that some varieties of carbon have a less specific heat

than others, that some absorb less heat in order to undergo a certain rise of temperature than the more chemically active varieties; and it has actually been shown that those varieties which have the greatest specific heat evolve, on combustion, the greatest quantity of heat. Take another instance. Phosphorus, as is well known, can be transformed by various processes into another state which is more dense—in which the particles are more closely aggregated together. It is well known that the red phosphorus does not melt so easily as the other sort; (more especially is it known that it has a less specific heat; and estimating this specific heat as the measure of the motion in its particles, the particles of the red phosphorus are in a less rapid state of vibration or momentum than the particles of the clear phosphorus. Then also we find, side by side with this, that the particles of red phosphorus, in combining with oxygen to form phosphoric acid, evolve less heat than clear phosphorus. The kind that has got less heat in it evolves less on combustion. Again, which is more important for our purpose, the particles of the red phosphorus are less easily ignited, having a less tendency to undergo the process of combustion, than the particles of the clear phosphorus. They have a less active tendency, under the ordinary temperature, to combine with oxygen. And again, if we immerse red phosphorus in sulphate of copper, it is well known that none of the metal is reduced by it. That is a distinctive test between red and clear phosphorus. In one case oxygen is taken from the sulphate of copper, and it effects the precipitation of the copper; so that in this respect we see by comparing the two states of one and the same element that the clear phosphorus is transformed, and that the particles of the red variety are deprived of a portion of the vibration which they possessed in the state of clear phosphorus, and that the properties of the element are for the time altered so as to be less active chemically. That is a most important point which I wish to establish on that particular case; but I think that the comparison of the specific heat of analogous elements—elements which belong to one and the same natural family—may serve to establish the same conclusion, perhaps even in a more striking and practically useful manner. For instance, we will take Deville's law as an approximation to the truth, that the quantity of heat contained in each unit of weight of one element is as much less as the atomic weight of that particular element is greater, or the products of the atomic weights of each element. Let us take merely the case of some elements belonging to one well-characterised natural family, and which are perfectly analogous to one another. Take, for instance, the natural family of chlorine, iodine, and bromine. Now, Deville's law enables us to express in terms, by the thermo-dynamic theory, that an atom of chlorine which is light has the same momentum—the same actual mechanical force in each atom—as an atom of bromine or iodine; and, accordingly, if these atoms are moving in a medium with no resistance, the velocity with which the atom of chlorine will move must be as much greater than the velocity with which the other atoms are moving, as its atomic weight is less. Thus Deville's law, if it be true, is merely another statement of the fact.

And now let us see how a comparison of the chemical properties of these elements will bear upon our previous notion. The theories we were just now reviewing had regard to the effect upon affinity of rapid motion compared with the effect of less rapid motion. Let us ask ourselves which of these three elements—chlorine, bromine, or iodine—is the most active chemically. I will take one particular case of the relative action of chlorine, bromine, and iodine in combining with one and the same element—hydrogen. It is well known that chlorine combines more powerfully with hydrogen than bromine does; and that bromine, in its turn, combines more readily than iodine does, and it has been shown by a measurement of

the quantity of heat evolved in the respective combinations that chlorine evolves a great deal more heat in its union with hydrogen, than bromine, and bromine more than iodine. We should accordingly, in the case of their decompositions, say that chlorine has the most affinity for hydrogen, and iodine the least, and we should put at the top of the series, as regards affinity, the element which has, as I just now said, the greatest velocity of motion. I say that Deville's law leads to the confirmation of the result which we have arrived at before, that the body which has the greatest affinity, evolves the greatest amount of heat. For it has been shown that the quantity of heat evolved when chlorine unites with hydrogen, is greater than when bromine unites with hydrogen; and that when chlorine displaces bromine a certain quantity of heat makes its appearance.

Now, amongst the other considerations which may reasonably be referred to in order to obtain a confirmation, or verification, or authentication of these results, I may refer to the process of diffusion, in which particles are left to themselves with as little disturbing force as perhaps under any experiment one can refer to. Now, it is well known that in diffusion the process amounts generally to nothing else than what I have just now affirmed to be the normal action of particles upon one another. Suppose we put into a jar a quantity of hydrochloric acid, and fill up the jar with water, we find that the hydrochloric acid, although heavier than the water, gradually makes its way to the top of the jar, and the water goes down in the place of it. But we really do not know, it having never been proved, that the hydrogen of the hydrochloric acid goes up, and that the hydrogen of the water goes down. All we can say is, that chlorine goes up and oxygen goes down. The fact stated in its simplest terms amounts merely to an interchange of constituents—an interchange of the chlorine for the oxygen without the aid of any external force whatever. Now, it is well known, from the researches of Mr. Graham, that after measuring the quantity of hydrochloric acid which diffuses itself in a given time, if a similar experiment be made with hydrobromic acid, it diffuses in about the same time; and if in a third experiment hydriodic acid be taken, and the quantity diffused in the same time, under the same conditions, be measured, it is found they are just about equal. Now what does that amount to? In equal weights of hydrochloric acid and hydriodic acid the hydrochloric acid diffuses as many more atoms than the hydriodic acid as its atomic weight is less than that of the hydriodic acid. The atomic weight of chlorine is 35½, and that of iodine is 127; 35½ would go between 3 and 4 times into 127; so that there are 3 or 4 times as many atoms of chlorine diffused as there are atoms of iodine. Bromine is 80; so that there would be about 2 atoms of iodine for about 3 of bromine. In fact, we find that those elements which we had reason, from other considerations, to consider as possessing the most rapid motion, actually do move out quickest of the three particles we have been considering, as we had reason to believe from Deville's law. The observations of the velocity in other cases of the same beautiful phenomena lead us to results which are accordant with this provision. I do not mean to say that I could explain all the results which have been observed, by this or any other theory; yet there are striking coincidences of the kind, and they consist of the cases of simple and well-known compounds which can hardly be supposed to be accidental.

Now, when we are judging of the force with which bodies unite, we generally are inclined to think that those elements which, on combining, evolve a great amount of heat are uniting more powerfully than those which, on combining, evolve less heat; that the disturbance which is produced by the intensity of the heat which is evolved on combination, does suggest itself to the mind as the measure of the amount of force with which the combination takes place; and, more than that, I think that the

action of chemical affinity itself usually carries along with it this further idea—that the more completely the product of any particular combination has lost the properties of the bodies of which it was formed, the more inert it is, and the less disposed to re-act in any way whatever: the more powerfully do we consider the elements of that body to be held together.

I will now describe the results of the experiments with specific heat. We should conclude from what I have just said, that those bodies which, on combining, evolved much heat, would form a compound containing less heat than others, which, on combining, evolved less heat. It would seem that the greater the evolution of heat on combustion, the less would be the specific heat of the product compared with that of the constituent bodies. We ought to find, when bodies unite with great energy, and when they form a product in which the elements are held together with great force, and great heat is evolved, that the product will have a much less specific heat than the original bodies. And, now, upon this subject I am really at a loss to know what to say. I have looked carefully and repeatedly over the results of the observations on the subject; and I have found many cases which certainly agree with this notion, but also many that do not. There are many determinations from very competent authorities of the specific heat of compounds and determinations of the specific heat of the substances from which those compounds are formed, in which it is found that the products actually contain more specific heat than the original constituents. Now, I conceive, in the first place, in order to compare properly and fairly the product, for instance, of baryta and sulphuric acid—sulphate of baryta—one ought to take it as formed by a specific process. One measures the quantity of heat which is given off when sulphuric acid combines with baryta, but we ought to take its specific heat and compare it with the specific heat of the different bodies. Experiments of this kind in direct relation have not, to my knowledge, been as yet made on any extensive scale; but, certainly, I think I am not overstating the case when I express my belief that in some of the best examined cases an agreement of the kind occurs. For instance, amongst the compounds of sulphur with metals, numerous instances are found in which the sulphides have had very considerably less specific heat than the sulphur and the metals before combining; and I think they are, perhaps, the most fair cases. Sulphur and the metal can be weighed, and the product can also be weighed in a solid state, and there is a great resemblance between the elements and the compound. I think, certainly, that taking the specific heat of oxygen and a metal, and measuring the heat evolved when the metal is burnt in oxygen, and then taking the weight of the solid oxide is hardly fair, because the change of state is hardly consistent with a direct comparison of the substances. However, although many cases of chemical combination do appear to agree as far as regards the capacity for heat of the products, in the manner which is stated in the process I have been submitting to you, there are some cases which not only do not agree with it, but which, I conceive, ought not to agree with it. There are certain bodies obtained by means of indirect action. Take, for instance, the case of anhydrous nitric acid, which everybody knows cannot be got originally from oxygen and nitrogen. Anhydrous nitric acid, which we have every reason to believe ensues, has more heat stored up in the particles, than the oxygen and the nitrogen contained in them when uncombined. Now, it is well known that this particular body, anhydrous nitric acid, by a very slight disturbance of the equilibrium of its particles, undergoes a violent explosion, in which heat is evolved, and free oxygen and free nitrogen liberated; so that the indirect process by which the bodies were persuaded to enter into that particular combination, may, in each case, have been accompanied by an evolution of heat; but the circumstances under which we after-

wards examine it being totally different, the alteration of specific heat enabled it to retain a greater quantity than the elements in their free state.

Take as another case the compound hydriodic acid. According to Messrs. Favre and Silbermann when elements unite without the presence of water, you would actually need to add heat to the free iodine and the free hydrogen. It is known that the compound when formed, especially by itself, is extremely unstable, and readily gives off its hydrogen. The elements separate with such extreme facility that they are evidently not held together with great force, and such cases as that rather afford a confirmation of the case in which it is generally found, that when bodies contain less heat they are then moving more slowly, and are more inert, and have less mechanical motion in their particles, in accordance with the notion with which we started. But in the consideration of chemical force of combination, without more exact data than any we have obtained, such cases as those which I have had the honour of submitting to you are unsatisfactory at the best. No general notion can lead to any accurate scientific results. I do not mean to say they may not have their uses in order to classify bodies in a particular way, but the only process which I know by which one can get anything like a quantitative comparison of the relative forces with which different elements unite, is by the working of that beautiful machine—the galvanic battery. I know of no other process by which we can in so easy and certain a manner compare the forces which are at work in different cases of chemical combination. In order to avoid too remote an analogy with the actual process, and in order to express the matter in exact terms, I ought to classify chemical action a little, in a manner that I think will be readily admitted to be applicable to it. For instance, when we have the case of the oxidation of a metal by nitric acid, as when we throw copper into nitric acid, it is said that is a process of oxidation. It is true enough that the copper is oxidised, but it is equally true that it is a process of de-oxidation; because we have to think of nitrogen as well as oxygen. And so in the greater number of other cases chemical action consists, either directly or indirectly, of two processes. The case of zinc and sulphuric acid might be said to be one of combination, which happens when the zinc is thrown in contact with the sulphuric acid and combines with oxygen; but you might equally say that it is also a case of liberation of the hydrogen. We will take the case of a battery of a very simple kind. [The section of a battery was drawn upon the black board.] We will take merely a plate of zinc and a plate of platinum in it. In this cell I have some hydrochloric acid. This shall form the chain between the zinc and the platinum, and there is my connecting wire. In this ideal battery all the hydrogen is being evolved from the water. We have combination of the zinc with the chlorine; a combustion is going on there. On the platinum plate we have the opposite process—the separation of chlorine and hydrogen. If we were to call this process of the combining of the zinc “burning,” we should call the separation of the hydrogen, to use an un-English expression, “un-burning.” Now, if we follow out the appearances which present themselves in special relation to heat, we find that the process in its general features is not accordant with what we know. For if we burn zinc in chlorine, we get a very large quantity of heat evolved, which we do not find makes its appearance on the surface of the zinc plates in the battery. There is not that amount of heat evolved, although there is no doubt that the chloride of zinc is forming; and on the other side where hydrogen is being separated from the chlorine we do not find that quantity of heat absorbed, or cold produced, by this un-burning. Everybody knows there is a frigorific mixture on this plate, and combustion, or the means of producing heat, on the other plate. All the heat we get produced is the difference between

the zinc which is combining, and the hydrogen which is being uncombined. For instance, to take Favre and Silbermann's determination of the heat in the presence of a large quantity of water of hydrogen combined with chlorine: we will call the heat 40,000, which is evolved when 1 gramme of hydrogen unites with 35½ grammes of chlorine, when one equivalent of zinc unites with chlorine we have 56,000 degrees of heat, and it is well known from the experiments of Mr. Joule and Mr. Wood that if you measure the total quantity of heat which is evolved in the whole battery—I mean in the connecting wire and the battery altogether—measuring the heat which is produced by the simple resistance which that wire overcomes, let it do what kind of work you like—if you measure it you find it is exactly equal to 16,000 degrees of heat, which is equal to the heat which the zinc evolves in combustion, minus the heat which the hydrogen being evolved absorbs on being separated from its compounds. Now, the question arises how, in the arrangement of our machine, the heat which the hydrogen wants in order to be evolved is communicated. The zinc has evolved 56,000 degrees of heat, but the hydrogen wants 40,000 of them. It is a question how the hydrogen gets that quantity of heat. It is well known that in the liquid which connects the two plates of zinc and copper there is a certain motion of the particles,—that when the chlorine which is next to the zinc has combined with it, and chloride of zinc is formed in that way, there is an atom of hydrogen left next the zinc. When an atom of chlorine combines, chlorine again takes the place of the chlorine which has gone out, so that the process we had at first re-establishes itself, in fact, in the liquid which connects the positive and negative metals. We have a constant change of places of analogous elements; we have, moreover, a number of properties, which we describe in general terms as a current of electricity passing through the liquid; and we have, besides, a perfectly similar appearance in the wire which connects the plates themselves—there is a current of electricity passing through the wire exactly as much as the current of electricity passing through the liquid. Of course the state of the particles in the wire is the same as the state of the particles in the liquid, and produces the same effect; and there must be a state of motion in the particles of the wire of the kind that we know to occur in the particles of the liquid. We cannot distinguish one atom of copper from another atom of copper to see how they are moving; but in all the cases we have an identity between the effects produced in the liquid itself; and it would be certainly unwarrantable, as far as I know, to assume that a different process occurred, so as to produce this identity of effects. So that the result comes to this, that we consider the connecting wire as serving to carry over from the zinc which is combined with chlorine, to the hydrogen which is uncombined, and separating, to carry over all the heat which the hydrogen needs in order to be set free, and constitute free hydrogen; and that the particles of the wire are in a state of vibration which is communicated, by waves, from one particle to another—from the zinc, which is burning, to the hydrogen, which is un-burning. Now, in the battery it is well known that by interposing wires of different resistance, we can measure the forces which are at work; and I think you will agree with me, that what occurs is precisely identical with those processes which occur if a lump of zinc is thrown into some hydrochloric acid. Hence we have free hydrogen being evolved over certain points of its surface, and there is no doubt whatever that when those points are less active they evolve hydrogen more easily. There is no doubt that on the surface of a lump of zinc there are little local galvanic currents occurring, the only difference being, that in this beautiful arrangement we keep all the combining process in one portion of the apparatus, and the separating process in another, so that the one cannot take place without the other; and therefore by interposing different resistances

we can operate with the most beautiful perfection, and measure, with the most wonderful accuracy, the force, by the amount of resistance which is overcome. I conceive there is no test for the measurement of forces to be compared with the amount of work which can be done. There is no doubt that where a body which is combining evolves very much more heat than the body that is separating absorbs, then great resistance can be overcome; and the force by which the one element displaces another may be measured by the quantity of heat which is evolved in the displacement. I believe, in stating that, I am not advocating any hypothesis whatever, but merely giving well-established results from the best observers.

(To be continued.)

NOTICES OF PATENTS.

Salts of Alumina. T. RICHARDSON, Newcastle-on-Tyne.

THE ideas which gave rise to the former patents of this chemist for the use of sulphurous acid, are pushed still further in another patent, in which the action of the same acid upon aluminous minerals is claimed.

Artificial Fuel. J. INGLIS, Glasgow.

THERE have, perhaps, been as many silly processes patented for forming artificial fuel as for any other thing whatever. Mr. Inglis's patent takes an unenviably distinguished position among them. He mixes excrement, "human or animal," with any kind of wood in the form of chips, saw-dust, or shavings, and coal-dust. Fortunately for society, this patent was not proceeded with. It is probably, therefore, a failure in practice.

Improvements in the Manufacture or Production of Nitrate of Potash and Soda, and in the Application and Use of Certain Products of such Manufacture in the Manufacture of Soap. W. ANDERSON, Glasgow.

THE patentee prepares nitre by means of the well-known double decomposition which ensues between potashes, or any salt of potash, and nitrate of soda. The nitrate is not procured directly, but by evaporation of the spent liquors obtained in the manufacture of soap with a mixture of potashes and nitrate of soda. The nitre is obtained by successive evaporations, until, at last, carbonate and caustic soda begin to deposit. The final mother liquor contains much caustic soda, which may be used for the preparation of hard soap or any other purpose for which soda, or soda ash, is applicable. One of the improvements claimed is the conversion of the sulphate of potash contained in the potashes of commerce, into nitre. This is effected by double decomposition with the nitrate of lime, obtained by neutralising with lime the nitric acid washed away from palm oil after bleaching it with nitric acid.

Evaporating-Pans for the Manufacture of Salt. J. CORBETT, near Bromsgrove.

THE patentee makes the evaporating-pans of cast instead of wrought-iron plates. The castings are constructed with flanges to enable them to be bolted together. A trough or depression is also claimed running either all round, or only on two sides of the pan. This trough is for the same purpose as the depressed end of common salting down pans, namely, to allow of the salt being raked together as it deposits during evaporation. The patentee also claims, both in cast and wrought-iron pans, making a gradual rise from the ends and sides to the middle.

Manufacture of Cyanides of Barium and Strontium.

W. CLARK, Chancery Lane, London.

THIS is a patent for the cyanuration of barium and strontium in presence of carbon. The process is precisely similar to that by which cyanide of potassium has often been obtained. The patentee mixes carbonate of baryta with sawdust and charcoal, and, after placing it in a tube heated to redness, passes nitrogen gas over the mixture. The current of gas is kept up until the mass is cold. On treating with water the contents of the tube large quantities of cyanide of barium will be obtained. We do not think this is likely to be a profitable method of obtaining cyanides. The patentee does not seem to be aware of the researches which have been made by numerous chemists of eminence upon the action of atmospheric nitrogen at high temperatures upon mixtures containing carbon, iron, and alkalis.

CORRESPONDENCE.**Magenta Dye.**

To the Editor of the CHEMICAL NEWS.

SIR,—I enclose you a small sample of Magenta powder which is soluble in water. When dissolved it will dye either silk, wool, or cotton, without any mordant or assistant. If you will go to the trouble of pouring a little hot water on the powder, and then pass the solution through a piece of cotton (so as to separate the insoluble matter), then dip a little silk or wool in the liquor, you will find the same dyes a beautiful shade and about the brightest in the market. I have made this on a new principle.

The French have sent over here a paste Magenta, which will dye silk, but not wool, and nothing near so bright as the enclosed sample. The reason I send you this is to show that a Magenta can be made in a powder, and be quite soluble in water and dye on any fabric without using any spirits of wine or methylated spirit.—I am, &c.

MAGENTA.

[The sample of Magenta dye has answered very well in our hands in dyeing both silk and wool. If such an article could be introduced into commerce at a reasonable price, it would, we doubt not, meet with a ready sale.—Ed. C. N.]

The Adulteration of Food.

To the Editor of the CHEMICAL NEWS.

SIR,—I beg leave to say that the only series of experiments I ever performed upon the adulteration of bread was made on loaves purchased at forty different shops in the metropolitan district of Lambeth; and that, in nine out of forty samples, I was unable to detect the presence of alum.

My experience in the matter of food-adulteration has led me to believe that the morality of dealers in eatables and drinkables, though not unimpeachable, is quite equal to that of professional grievance-mongers and dealers in mares'-nests.—I am, &c.

WM. ODLING, M.B., F.R.S.

Kennington-road, London, S.

MISCELLANEOUS.

Chemical Society.—The next meeting of this Society will take place on April 18, when a discourse will be delivered "On the Application of Electricity to the Explosion of Gunpowder," by F. A. Abel, F.R.S.

Silver in Sea Water.—Captain Maury, of the American Navy, has been making some calculations, based

upon the discovery of traces of silver on the copper bottom of a vessel docked at Valparaiso, and from these calculations Captain Maury concludes that at least 200,000,000 million of tons of silver exist, in an exceedingly diffuse solution, in the waters of the ocean.

Royal Institution of Great Britain.—At the General Monthly Meeting, held on Monday, April 1, 1861, William Rutherford Ancrum, Esq., Stephen Jennings Goodfellow, M.D., and William Newmarch, Esq., were elected Members of the Royal Institution. Rev. Charles Forster, H. G. De Mussy, M.D., Rev. A. Denny, and W. E. M. Tomlinson, Esq., were admitted Members of the Royal Institution. The presents received since the last meeting were laid on the table, and the thanks of the Members returned for the same. The following is the arrangement of Lectures for the ensuing week:—Tuesday, April 16, at 3 o'clock, Professor Owen "On Fishes." Thursday, April 18, at 3 o'clock, Professor Tyndall "On Electricity." Friday, April 19, at 8 o'clock, John Ruskin, Esq., "On Tree Twigs." Saturday, April 20, at 3 o'clock, Max Muller, Esq., "On the Science of Language."

Uses of Tea.—At a recent meeting of the Society of Arts a paper was read on "The Uses of Tea in the Healthy System," by Dr. E. Smith. The author began by stating the opinions brought forward by Professor Johnstone and other chemists on this subject—views which were still very generally held. These might be summed up thus: that by the use of tea—1. The waste of the body is lessened; 2. The body is nourished; 3. By lessening waste, we may lessen supply, and yet the bodily powers shall be duly sustained. He stated his reasons for differing from these statements, and explained the nature of the experiments which he had undertaken in order to ascertain the actual effect produced by tea. If there be an abundance of food in the system, and that especially of the farinaceous or fat kinds, tea is a powerful digestive agent, and by promoting the transformation of food, it aids in nourishing the body; but with a deficiency of food in relation to the waste to the tissue by exertion, or the waste of heat by cold, or by too profuse evaporation from the skin, it wastes the tissues of the body, and lowers the vital powers. He, therefore, considered that it was not suitable as a partial substitute for food, as was generally supposed; for it increased the waste of the body.

ANSWERS TO CORRESPONDENTS.

* All Editorial Communications are to be addressed to Mr. CROOKES and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

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Tar.—We intend to give a report of Mr. Perkin's Lecture "On the Colouring Matters Obtained from Coal-tar," which will be delivered before the Chemical Society on May 16.

Silicic Ether.—We do not know where this can be obtained. It certainly cannot be got ready made, and few manufacturing chemists would undertake to make it unless it were ordered in quantity. Dr. Frankland was asked £6 per lb. for it in quantity.

T. W.B.—We never heard the name of the gentleman you mention.

C. R.—1. We do not know at present; but any process would answer if devised by a skilful chemist who knew what he was about. 2. No attempts have been made except as experiments. 3. It would; but not in sufficient quantity to pay for the battery power employed.

THE CHEMICAL NEWS.

VOL. III. No. 72.—April 20, 1861.

TECHNICAL CHEMISTRY.

On the Adulterations of Food, &c., in Melbourne,
by WILLIAM SIDNEY GIBBONS.

I SUBJOIN a few notes of observations that I have made from the year 1852 to the present time, on articles of food largely consumed and commonly adulterated. In the present paper I shall therefore confine myself as much as possible to cases that have come under my own notice in Melbourne. The first case of contamination that I remember to have noticed here was the presence of copper in some raspberry-jam, imported from Hobart Town in 1852. The contamination was obviously accidental, and the risk of its recurrence is reduced by the extensive use of enamelled iron preserving vessels. I remember, however, that the announcement did not appear to awaken any anxiety, but, on the contrary, seemed to be regarded as something very funny by the audience to whom it was incidentally mentioned in a lecture.

Flour.—I find in my notebook a record, bearing date November, 1856, of the examination of several samples of flour at one time. They were eight in number, and of the eight, four were notably adulterated with plaster-of-Paris. Many cases occurred about the same time of the seizure of flour, which, after examination, was condemned as unfit for sale, the vendors being punished whenever the act could be brought home to them. Most of the flour so condemned was musty, or otherwise deteriorated by organic change. About the same time a somewhat singular circumstance occurred which is worthy of mention in this connection. I had been lecturing in a suburb, not then famous for the production of good bread, on the chemistry of bread. After discussing the various phenomena connected with nutrition, with bread-making, &c., I proceeded to illustrate the most familiar adulterations by actual experiment, and by the citation of cases. When I concluded, some local bakers, virtue impelled, rose to say a few words for the credit of the craft. One had been in the trade for twenty-seven years and had never heard of alum, another was all innocence on the subject of potatoes, and a third was horrified by the bare mention of plaster. I was about to disclaim all personal allusion, as I did not know at which of their shops my sample had been bought, when a gentleman in the body of the crowd rose, and having premised that he was unconnected with the "mystery," told the following pertinent anecdote without comment:—"He had imported a cargo of whitening, an article in great demand among the makers of effervescing draughts, but unfortunately for him his shipment arrived in cold weather, and was stored at a cost almost beyond its then value. There it remained for some time, 'eating its head off' in the store, for there was no hope of a rise until the next spring; when one day, to his surprise and delight, the broker in whose

hands it was placed, called and reported the profitable sale of the lot. 'What on earth occasions the demand at this season?' inquired the delighted vendor. 'Flour's up,' was the laconic reply."

Bread.—Although I am willing to believe that bread is less injuriously, and less extensively doctored here than in London, it is often far from being what it professes. Damaged flour and deteriorated grain are certainly sold specially for the behoof of fowls and pigs, and contribute their share to the prevalent diseases among the former, but they nevertheless often find their way to the table. I have lately had some samples of bread under examination for fungi; but although I could have taken a hint from the oft-quoted Dauphine, and have eaten pastry, rather than such bread, I was not sufficiently satisfied of their presence to feel justified in condemning it; as while the conditions for the growth of fungi subsisted, the plants themselves were not to be found. I have already made mention of potatoes as a common, I may say, a general adulteration of bread, not in the form of potato-flour or starch, but entire or simple boiled mashed potatoes. The fraud here is the substitution of a less for a more nutritive article, and one too that holds a larger quantity of water. The same objections, with others on which it is needless to enter, apply to the use of rice.

Spirits.—To the liquors sold in Melbourne I have had opportunity of giving special attention, and I have notes, the publication of which, with names and addresses, would probably astonish the persons to whom they refer. It must not be supposed that liquors sold over bars are the same as those delivered into cellars, or from bonded stores. Such little trifles as water, salt, alum, burnt sugar, frequently enter into the composition of the nobbler, and one or other may almost always be found. Were it otherwise, there would, probably, be a much larger amount of drunkenness produced with the expenditure of smaller sums of money. People could get "wholesomely" drunk too soon upon spirits of the full strength, and the most profitable kind of trade—viz. tippling, would run a risk of being curtailed; while if the diluent alone were added—and I believe that this may be the only addition in some few of the best houses—the reduction could not be carried to so great an extent as is common. To take an example, one of many, over the bar of a house that shall be nameless, one of a class, a spirit (rum) was sold, which was within a fraction of forty degrees of proof below an average sample obtained from a respectable wine merchant, and employed as a standard for comparison. The solid constituents were very little reduced, and the extractive matter, including the gum, sugar, &c., were but little less. How then had the change been effected? The strength had been brought down by water, the colour, and part of the flavour restored by burnt sugar, and other, perhaps less agreeable, organic matters, and the roughness which might tickle unrefined palates was imparted by alum, the constituents of which were present in larger quan-

tities than could have been attributable to the water. In the matter of brandy, less consumed, perhaps, by the class of customers thus played upon, I found less doctoring of the sort described. Brandy is even more exposed to doctoring than rum, but to do it well requires rather more nicety, and the ingredients are not of the same rough and ready kind. In the series of experiments to which I refer, I find a brandy fifty-three degrees below the assumed standard. In this instance there had not been any addition of alum or other mineral astringent, probably the water was almost the only adulteration, the use of some highly concentrated astringent, such as catechu, being alone admissible under the circumstances. Neither capsicum nor tobacco were found, nor had any mineral acids or metallic salts, except common salt and alum, been added to any of the spirits examined. The advance in price was the same as in the former case cited, but the bulk was doubled, so that the profit bordered on the fabulous.

Beers.—In the beers it was more difficult to discriminate between the work of the English and the Colonial brewer, and to determine the publican's part in the doctoring. I believe, however, that his share is limited to collecting all the drains of the counter, and the other beer-engine waste flow into a butt, whence it returns in combination with original beverages, and to drawing from two butts. The latter operation has place more particularly with ale, the pipe going down from the engine is branched, one end going into the English brand, to whose name it answers, and the other into some colonial abomination; or, perhaps, into the *omnium* before mentioned, though the chief use of that is, no doubt, for the ready production of half-and-half.

Confectionery in its most artificialised forms is a fruitful source of disease; and well it may be. The most noxious types of those really intended for eating (though many ornamental devices not addressed to the palate yet find their way into the stomachs of children) are the comfits of dead white, body coloured externally. In some of these I found the following colouring matters: the red was a harmless organic pigment, one of the lakes commonly used; the yellow was the poisonous chromate of lead, and formed a thick coating on the surfaces of some of them, while in others the pigment was diffused; the same deadly paint was used in combination with the red to produce orange; the blue was ultramarine, and the same pigment faced with red furnished a purple. The white opaque mass, though uninviting enough, was nearly all sugar, and did not contain any plaster-of-Paris or other earthy matter.

Coffee.—Perhaps the most remarkable example of local adulteration and sophistication is coffee. I long regarded it as the sole case of the kind that demanded notice. The question "What is coffee?" should now have for its reply, "Coffee is a manufactured article, varying according to the products of the country in which it is made; in Melbourne it consists in part of the berry whose name it bears, and in great part of gram, maize, peas, roasted corn or potatoes, and chicory." The case of coffee is a very singular example of the gradual perversion of public taste, by means of gradually increasing adulteration. Formerly, in England, coffee in its purity was a staple commodity, its flavour was well known and was highly esteemed. Presently chicory was introduced with specious and false recommendations as an improver, an economiser, &c.; then those grocers who had before used it increased the proportion to keep pace with the times, and before long the coffee with an admixture of chicory, might be called chicory with an

admixture of coffee. The chicory then became a mixture, as it is now, and the diluting ingredient required a disguise in the shape of colour; and the coffee mixed with mixtures became a very indefinite compound, and needed all sorts of sophistication to correct flavour and colour. At this point, then, when coffee is merely a subordinate ingredient in the compound, begins the history of our adulterations, and the Melbourne manufacturer avails himself of the state of the public palate to acclimatise the usages of the London market. The state of the markets dictated whether maize, or gram, or peas, or potatoes should be the staple, and they were flavoured and coloured with chicory, enough coffee being added to tone the whole, to impart a modicum of their essential qualities which are possessed by coffee alone of all the ingredients, and to satisfy what served the artist in place of a conscience.

A few words in conclusion. Without intending to defend the traders who either adulterate or sophisticate our food, I must distinctly throw the blame upon the consumer, who is the cause. No one who goes out of his way to a cutting shop, or who endeavours to purchase an article at a less sum than it can remuneratively be produced for, has any right to complain of adulteration, nor is he entitled to raise a cry against sophistication, if he insist on an ideal standard of colour or texture. A manufacturer of genuine mustard, then almost the only one, tried for years to get his mustard into the English market, but was always at a disadvantage. It was of a dingy-brown colour, whereas the sapient public would not be content with anything but bright yellow. Of course, said public was accommodated by the help of starch and turmeric. And so it will be with everything else. If people set their minds on having coffee at ninepence per pound instead of eighteenpence, grocers would be found to supply the article.—*Dublin Medical Press (from the Australian Medical Journal).*

Natural Oxide of Silver.

WE extract the following remarks on the above subject from the *London Review*:—

No little excitement has been caused by a recent announcement in the *Times* of the existence of silver, in the form of oxide, in the neighbourhood of the ordinary metallic silver deposits. The result being, first, that from a ton of ore, in its natural state, where the normal average of yield of the precious metal would be 13 ounces, by the new process an average of 113 ounces of silver would be obtained, the overplus being derived from that which has hitherto been rejected as useless; and, second, that the cost of this new method of reduction is relatively so small as to render the expense but little more than nominal. The letter in which this announcement is made prefaces it by stating that, up to within a very recent period, the existence of such a thing in nature as oxide of silver has been denied by the most eminent chemists, but that science has now demonstrated that it does exist in enormous quantities; and the writer concludes by saying that the importance of this discovery to our own mines, where silver is often found in close approximation to copper and other metals, will be very great, but to some of the foreign ones, where the deposit is large and unmixed with other metals, the result, in the shape of returns, will be almost fabulous. This letter was evidently intended merely as a preliminary to a longer one, but still anonymous,—

from "R. G."—in which more detail is given, and the happy locality hinted at. Like the former one, it commences with the assertion that scientific men, of the highest chemical attainment, either still wholly deny the existence of natural oxide of silver, or assert that if it does exist it can only be extracted in such small laboratory experiments as to render the discovery commercially valueless. The writer then proceeds to confirm the previous statements, asserting that Cornish gozzans, which, in their natural state, give, upon assay, from 5 to 13 ounces of silver per ton, yield, after the new method of treatment, from 54 to 216 ounces, whilst some have been found to be fabulously richer. Taking into consideration the great value of labour in England, it will not pay to work ores containing silver only in less quantity than 10 to 12 ounces per ton. It follows from this that the lowest yield, according to the new statement, 54 ounces, would bring in an immense profit. The expense of raising, reducing, and crushing these neglected ores could not exceed 3*l.* per ton, whilst the value of 54 ounces of silver would be over 13*l.*, and that of the average yet obtained, per ton, over 35*l.*

These statements are very extraordinary, and deserve the careful attention of every one interested in the development of England's mineral wealth; whilst the fact of their publication in the *Times* renders it doubly important that they should not pass unchallenged by men of science. Divested of all theoretical conclusions, they simply amount to this, that certain silver ores, which yielded, according to the ordinary mode of assay, from 5 to 13 ounces of silver per ton, were shown, by another method of assay, to contain from 54 to 216 ounces. The conclusion is, therefore, jumped at that the cause of the discrepancy between the two assays is, that the lower numbers represent silver in its ordinary state, whilst the higher figures show what may be obtained by a different mode of working; the increased amount of silver arising from the presence of the precious metal in some mysterious combination with oxygen, the existence of which compound in nature the public are inferentially led to believe is only just beginning to be admitted by the most eminent chemists, and we are to assume that the product, oxide of silver, although present in the ore in such enormous quantities, possesses so remarkable a disinclination to give up its metallic constituent under ordinary chemical treatment, that the usual methods of assay are quite inadequate to detect its presence.

Now, we do not for a moment intend to dispute the fact that valuable silver ore has been and is habitually thrown aside as worthless; such a thing only coincides with every day experience. Every new discovery in the metallurgical or mechanical treatment of ores, every new chemical fact which is made out in reference to the precious metals, is liable to be followed by the discovery that millions of tons of mining refuse will now "pay" to work over again. Not long ago, the adoption of a very simple suggestion of a practical chemist was the means of very considerably increasing the yield of gold in one of the Australian gold-fields; immense quantities of "tailings," the accumulated refuse of many years, being by this means found to yield nearly as much gold as the original ore. A similar instance occurred in our own iron districts. One of the best and richest ores was for many years cast on one side as valueless, until a metallurgical chemist pointed out to the ironmaster that he had under his feet unlimited quantities of iron ore better than what he was putting himself to a considerable expense to bring from some distance to his furnaces. Again, not many years ago, a clever mineralogist showed

that gold was very widely distributed in various rock and strata in the British isles, and published analyses to show that in many localities it would pay for its extraction. We are, therefore, quite prepared to believe that the same holds good with the less precious metal, silver, but we must really take exception to the unscientific manner in which the announcement has been made public. In the first place, why is it assumed that the silver exists in the form of oxide? From the pains taken by the writers to set aside the universal opinion of chemists, they seem to admit that this is a weak point of their argument. Oxide of silver can be prepared artificially with the greatest readiness; its properties have been studied and experimented upon over and over again, and the opinion which chemists have in consequence arrived at, that it is an extremely improbable substance to be found in a mineral state, is confirmed by the fact that no work on mineralogy mentions such a body. It is one of the most unstable bodies ordinarily met with; simple exposure to the air, to rain, to light, or to heat, at once cause its decomposition, or passage into something else,—carbonate or chloride, both known minerals. When we add to this that it would require the utmost refinement of chemical skill to be able to pronounce definitely as to its presence in so heterogeneous a mixture as Cornish gozzan, it will be seen that we require more evidence than a simple anonymous assertion to credit its existence.

The inference which we are led to draw from the discrepancies between the former and latter assays also deserves comment. To a properly qualified person, the assay of a silver or any other metallic ore, and giving the theoretical yield per ton, would present no more real difficulties than the act of counting the books in a book-case, and giving a correct return of their numbers, would to an intelligent child; whilst the methods adapted by the assayer are so searching that the occurrence of other bodies in combination with the silver (whether it be as oxide, chloride, bromide, sulphide, &c.) can no more interfere with his results than can the different coloured bindings cause a false return of the number of books to be arrived at. The fortunate possessor of these Cornish gozzans evidently had the first assay made by a person who was not qualified to perform such an operation. He now finds that his mineral is richer than he expected; but what need was there to trumpet the discovery in the *Times*, prefacing it with the remark, that whereas eminent chemists denied the existence of such a thing in nature as oxide of silver, science, on the contrary, had demonstrated that it does exist.

New Discovery in Printing with Mauve, Magenta, &c.

WE have recently been shown some extremely beautiful specimens of muslins printed by a new process in mauve. The bloom and lustre are superior to anything we have seen; but perhaps the most remarkable fact connected with it is, that it stands boiling, soap, and violent rubbing fully as well as the fastest madder purple. The process is also applicable to Magenta, and—which, perhaps, shows its universality of application better than anything else—it may be used for the new chrome green: moreover, almost any number of colours may be combined in the same pattern with as much ease and success as in working them separately. One great feature in this process is, that the whites are retained of the utmost purity, whereas by the dyeing process, with gallic acid and tin, they are by no means all that can be

desired. From what we have been able to gather, it is evident to us that the albumen, gluten, and dyeing processes for mauve will be quite put on one side as soon as this important discovery becomes properly known. We have not been able to ascertain as yet the nature of the process, which, we believe, has, as yet, only received provisional protection; as soon as the patent is completed we shall hasten to lay a criticism upon it before our readers. We may perhaps mention that it is rumoured that the discovery originated at Messrs. W. Miller and Sons' print works, of Springfield, near Glasgow.

THE STOCKPORT WATER SUPPLY.

THE Stockport Water-works Company have tried another action against some proprietors of print-works who discharge their poisonous refuse into the Company's source of supply. We may at once express our surprise that it should ever have been found necessary to bring such an action. The introduction of arsenic into the process of calico-printing is of but recent date. The town of Stockport has been supplied with water from the present source for five-and-twenty years. It seems to us, then, more than astonishing that, setting aside the legal rights of the case, a manufacturing firm of great wealth should turn their refuse liquor, known to be strongly charged with a virulent poison, into a stream which they were well aware yielded the supply of water for drinking purposes to a populous town; and our astonishment as chemists is not at all lessened by the knowledge that the means of removing the poison before the liquor is discharged into the stream is obtainable with little trouble and at small cost. It is satisfactory to know that the defendants in the former action have adopted a plan which perfectly succeeds in arresting the poison; and there can be no reason why the Messrs. Potter, and the owners of other print-works on the stream, should not do the same. They have only to take the advice of a skilful engineer and a competent chemist, and they will soon be taught how they may treat their refuse so that it may be discharged into the river without a trace of arsenic.

The plaintiffs in the action are evidently not animated by any vindictive feeling: they offered to refer the case to arbitration, but the offer was declined, the defendants resting their case on a technical objection to the right of the plaintiffs to take the water, which they admitted was to some extent contaminated. All this is very well in a legal point of view; but there is a higher point of morality involved in these cases than can be adjudicated upon in a court of law. We have before urged the dangers which may arise from proving the ubiquity of arsenic. It greatly increases the probability of that poison being selected for criminal purposes when a ready defence is suggested that the presence of arsenic in the body of a person supposed to be murdered may be accounted for by its presence in the water the deceased habitually drank. Mr. Grove would no doubt skilfully avail himself of this fact, if at the next Chester Assizes he should be called upon to defend a prisoner accused of poisoning, and he would be quite justified in doing so.

Let us hope, then, that we shall hear no more of these actions. Stockport may be a very healthy place, and it is quite possible that a much larger quantity of arsenic than was found in the water taken at the George Inn, may be perfectly harmless. But we would still contend that to allow the minutest quantity to pass into the water supply of a large town is a criminally negligent act, and we shall be glad to hear that the Messrs. Potter have taken, and acted on, the opinions we have suggested, in which case they will have the consolation of knowing that they have obeyed the injunction which commands us to "avoid the appearance of evil."

The following is a condensed report of this trial:—

At the Chester Assizes, last week, an action was tried, brought by the Stockport Water-works Company, against Messrs. Potter and others, the owners of certain print-works on the Mersey, or its tributaries, for throwing certain noxious substances or poisons into that stream, and so polluting the water, to the detriment of the plaintiffs' property. The case against the defendants was substantially the same as against Messrs. Turner, Norris, and Co., in the action tried last year, and reported in the *CHEMICAL NEWS*, Vol. ii., p. 111.

In his opening speech for the plaintiffs, Mr. Collier suggested that the matter should be referred to some competent and disinterested individual, who should visit and examine the works, and then decide what should be done to abate the nuisance, and the plaintiffs would abide by his decision. Mr. Grove, on the part of the defendants, refused to accede to this proposal, and the case proceeded; the trial lasting four entire days. The chemical evidence for the plaintiffs was much the same as in the former case.

Dr. LYON PLAYFAIR said he had tested samples of water taken above and below Messrs. Potter's works for arsenic. Above the works there was none. The stream below the works contained arsenic in suspension, but none in solution. Samples of mud taken above the works, contained minute traces of arsenic. In one pound of dried mud taken below the works, he found five grains of arsenic, with some lead and a little copper. The mud of the Company's reservoir contained arsenic. In water taken from a tap at the George Inn, Stockport, he had found traces of arsenic. The arsenic in the water above Messrs. Potter's would be insoluble, but that taken at the George Inn, was directly attributable to the print-works. The mud of many rivers contained arsenic.

Dr. MILLER, in his evidence, confirmed the finding of the arsenic in the water and mud below the works, and the complete absence of the poison in the water above the works. In the water taken at the George Inn, Dr. Miller said he had found about one-hundredth of a grain of arsenic in the gallon. The presence of arsenic is not referable to natural causes, but immediately referable to the print-works.

Mr. CRACE CALVERT had examined the water at the same time as Drs. Miller and Playfair, and had obtained just the same results. Calico printers are able to neutralize the effects of the poison before throwing it into the stream. It is now done at Messrs. Turner's works.

Mr. BATEMAN, civil engineer, in his examination, said that in his opinion a settling reservoir with filter could be constructed by Messrs. Potter, which would have the effect of intercepting the arsenic and other poisons. By a settling reservoir, he meant one which holds the refuse, to be periodically emptied by mechanical agency.

Mr. GROVE, for the defence, after making some technical objections to the right of the Company to use the water from Nabb's Pool, ridiculed the idea that arsenic in such infinitesimally small quantity could do any harm at Stockport. It was even admitted in evidence, he said, that arsenic existed in waters which were not mineral; and the quantity existing here was so small that it might be looked on as a normal ingredient. Professor Playfair had said that there was hardly anything which did not contain arsenic, whether food or drink; but in the present case it existed in such small quantities that it was not injurious to health, as was testified by the mortality of Stockport, which was no higher than that of other manufacturing towns. It was impossible that any arsenic could get to Stockport to do harm, for it had been stated in evidence that lime, magnesia, and oxide of iron, which exist in this river in great quantities, tend to combine with it, and render it innocuous, and no more deleterious to health than ordinary sandstone. It had also been stated that so much arsenic had been found in the mud of the river; but it should be remembered that people did not eat mud,

and carbonate of lime tended to precipitate arsenic. The learned gentleman then contended that on Dr. Playfair's own showing there was sufficient salts in the water to precipitate any arsenic that might find its way there, and expressed his surprise that so little of the poison had been found at the outlet from Messrs. Potter's, which showed the extreme caution used there.

No evidence was called for the defence.

Mr. Baron CHANNELL summed up at great length, and submitted ten questions to the consideration of the jury, who, after two hours' deliberation, returned a verdict which was substantially for the plaintiffs, with forty shillings damages, leave being given to move for a new trial on the points of law.

PHARMACY, TOXICOLOGY, &c.

On Foreign Disulphates of Quinine, by JULIUS SCHWEITZER.

A NOTICE in the *Pharmaceutical Journal* for March 1860, p. 463, about the deficiency of weight in samples of foreign quinine, drew the attention of chemists generally to this preparation; and as an expensive article offering some difficulties in analysis is readily believed to be open to fraud and adulteration, a suspicion was cast on all the somewhat cheaper foreign quinines. Having had occasion, since the appearance of this notice, to examine a quantity of recently-bought German quinine, my results proved so different to all expectation that I was led to extend my inquiry to as many different quinines as it was possible for me to collect. By the assistance of some friends I obtained original and (with one exception) unbroken samples, which had found their way into this country in the ordinary course of business, before the appearance of the article above alluded to. These comprised specimens prepared by almost all the principal quinine manufacturers of Europe—Pelletier of Paris, Thomas of Argenteuil, Jobst of Stuttgart, and Zimmer of Frankfort, to which I added that of Messrs. Howard of Stratford.

All differed somewhat in colour, lightness, and crystalline structure, the appearance being in favour of Messrs. Howard's, Jobst's, and Thomas's preparation. Pelletier's quinine, while nearly half as light again as any of the former, presented by its minute broken crystals the less slightly appearance of a loose spongy mass. On the appearance of the second German sample, I can offer no opinion, as it came into my hands some time after the bottle had been opened, and it had evidently been badly kept.

In analysing these quinines, the correct weight of the whole samples was first ascertained, and then the amount of moisture and water of crystallisation determined by carefully drying a certain quantity on a water-bath and noting the loss. The sulphuric acid and pure quina were estimated by dissolving equal weights of the anhydrous disulphates in water slightly acidulated with nitric acid, dividing the solution into two parts and precipitating one with nitrate of baryta and the other with ammonia. Both precipitates were collected, washed, dried, and weighed, and the sulphuric acid calculated from the sulphate of baryta. The purity of the precipitated quina was further ascertained by solution in ether, redissolving the ethereal extract in water slightly acidulated with sulphuric acid, and allowing the solution to crystallise for the estimation of the pure disulphate of quinine.

The amount of sulphuric acid and basic quina obtained by simple precipitation so closely corresponded to each other, and to the theoretical values of base and acid given by Pereira and others, as to leave no doubt of each being a pure disulphate. In the subsequent estimation of the crystallised disulphate, the discrepancies arise partly from the actual solubility of quina itself in water and alkaline ammoniacal solutions, and partly from the unavoidable loss which may well take place when a vegetable alkaloid is subjected to various chemical manipulations. To avoid, however, as far as possible, every irregularity in the results of these analyses, all the quinines were treated exactly alike, the same quantity of alkali, acid, and water being used in each case, so that the causes of error might bear equally on each.

	German. (Bought after Notice.)	English.	French.	French.	German.
1. The quinine was packed or sent out	In tin box lined with paper	In plain tin box	In bottle	In bottle	In bottle
2. Weight	Rather short	Good	Very good	Good	Could not be ascer- tained, as part had been used
3. Appearance	Perfectly crystal- line. White	White, crystalline	White, crystalline	White, loose mass	Yellowish
4. 100 grains by weight measured	850 grains	800 grains	775 grains	1200 grains	600 grains
5. Moisture and water of crystallisation	15 per cent.	13.3	14.8	11.6	3.3
6. Deficiency of water of crystallisation	1 per cent.	2.7	1.2	4.4	12.7
7. 100 grains of the dry disulphate precipi- tated, dissolved in ether and recrystal- lised, gave	87.0 per cent.	84.0	86.6	82.3	81.6
8. Loss in recrystallising 100 grains of the dry disulphate as indica- tion of quinoidine	4.0 per cent.	6.0	6.6	7.0	7.6
9. Cinchonine	None.	None	None	None	None

In re-crystallising a disulphate of quinine containing quinidine in its minimum of water (30 parts), we meet with a loss, which will be greater in proportion to the amount of quinidine in the sample, and considering the mother liquor as a saturated solution of disulphate of quinine, we may add this to the re-crystallised disulphate and estimate with tolerable accuracy the amount of quinidine by the remaining difference in weight. The performance of these operations, however, requires the greatest possible nicety, for the disulphate of quinine is easily decomposed by heat and converted into uncrystallisable quinine, which might lead an unskilful analyst to estimate the quinidine at too high an amount.

Perfectly crystallised disulphate of quinine contains 16 per cent. of water of crystallisation; but in this all the samples were more or less deficient (as will be seen by a reference to the tabular statement). The crystallised disulphate of quinine is, in fact, a salt whose small delicate crystals are easily damaged, and subsequently part with their water of crystallisation, and it is simply a matter of age and keeping to make this change more or less considerable and apparent. When kept in a close sealed vessel, and in contact with paper, or any other absorbent substance, the liberated moisture may be readily observed in the damp condition of the paper; while in the absence of the latter, as in a stoppered bottle, the same moisture would remain mechanically mixed with the quinine. On exposure to air, however, the same loss would speedily take place. We know by experience that this change from the crystallised to the anhydrous state is by no means uncommon, as many a dispensing chemist can testify. Quinine in medicine chests, when these are sent to be refilled, will never look as it did when sent out; and even when none has been used from a bottle, a considerable loss of weight may be found. This loss of crystalline structure and of weight is a serious matter to those who are obliged to keep large quantities of the disulphate of quinine; and as moisture on the quinine itself proves less objectionable than the same moisture in the paper, we would recommend manufacturers to avoid lining their quinine tins with anything at all, to keep the orifices for filling them as small as possible, and to select for closing their smaller bottles such corks and paper as are least likely to absorb moisture. The buyer and retailer will always suffer some loss, but, by taking similar precautions, he may keep his loss within the least bounds.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, March 21, 1861.

Professor BRODIE, F.R.S., President, in the Chair.

(Continued from page 221.)

But another question attaches itself to this. The chief use of the results that we obtain in this manner is that they agree and give us tangible evidence of those same results which one arrives at from a general unquantitative comparison of the action of bodies upon one another. I mean to say that the processes where bodies contain much specific heat, and in which the particles are in a very rapid state of motion, which on combining evolve much heat, and accordingly form compounds containing little motion of particles, are those in which the force is greatest during combination, and that the tendency of bodies to re-act chemically, to combine, I will not say in direct proportion to their specific heat, because that would

be a quantitative statement which is not authorised at present; but, at all events, that those bodies in which the particles are in the most rapid state of motion are those which have the greatest tendency to re-act chemically, and *vice versa*. Whether this takes place in direct proportion to the rapidity is a numerical point which I will not attempt to speak to.

There are one or two other points which I may be allowed to raise before I leave the subject. One is, whether one ought to suppose that in the case of two elements of very different weights—take a heavy one like iodine, and a light one like hydrogen—which, if they contain the same specific heat, must have very different velocities of motion—whether the two elements, which have very different weights, on uniting assimilate their movements to one another. I think the matter ought to be stated clearly, if possible. There are some reasons which would rather incline me to believe that something of the kind occurs. It is singular how difficult it is to get heavy elements and very light ones to hold together with any great force. Everybody knows in such cases how very unstable the compound is compared with the stability of the compounds of elements more like one another in their atomic weights. When one compares the compounds of iodine with metals to those of chlorine with metals, or to those of chlorine with non-metallic bodies like phosphorus, there are a good many cases in which we find that phosphorus or copper would take up a quantity of chlorine, and that it is incapable of holding in combination as many atoms of iodine. I believe it seems to be not improbable that these effects may be owing to the difficulty which iodine has to assimilate the motion of its atoms to that of an element very dissimilar from itself in weight, and which is more inclined than itself to move with great velocity.

I will not detain you longer than to add my apology for bringing before the Society considerations still so incomplete as these must necessarily be at present; but I felt if there were any body in the world before which I could venture to bring them, it was the Chemical Society of London. My excuse is chiefly a hope that it would lead the members of the Society to collect facts, and to consider the bearing of facts that have been already established in connection with this fact; because it is, I believe, undoubted, that the atoms of matter are in a constant state of motion relative to one another.

The CHAIRMAN, in the name of the Society, thanked Dr. Williamson for the highly interesting and philosophical discourse which he had just delivered, and invited the gentlemen present to express their views on the subject which had been brought forward.

Dr. ODLING: Among the very interesting topics which Dr. Williamson has suggested, there is one which, I must confess, struck me as being rather weak, and that is the relation of the amount of heat given out in combination, to the altered specific heat of the products. I was struck by one observation that Dr. Williamson made in reference to the diffusiveness of chlorine, and bromine, and iodine. He said that all cases of diffusion could not be explained in that manner, but here we had, with three remarkably simple bodies, well-defined illustrations of the fact, that the mobility of the particles was in proportion to their specific heats; and arguing from these well-defined compounds, he expressed an opinion that this must be something more than a coincidence. This, he said, or seemed to imply, ought to be the normal condition of things, to which the others, which are at present irregular, will be brought in time, when we are able to ascertain the interfering causes. Now, in precisely a similar manner, I would make a remark upon specific heat. If we take those remarkably simple compounds, the iodides, the chlorides, and the bromides, the rule is that the specific heat of the product is identical with the specific heats of the elements entering into combination. That rule seems

to be constant and absolute in connection with proto-bromides, proto-iodides, and proto-chlorides. For instance, the specific heat of chlorine is 6, that of silver 6, and that of chloride of silver 12; the specific heat of chlorine is 6, and of lead 3, and that of chloride of lead 9, omitting decimals. Now, this is true of proto-chlorides, proto-bromides, and proto-iodides, and I may say of perchlorides also; and it is not only true of these compounds, but of many others, the sesquioxides and sesquisulphides, for instance. Now, I would rather be inclined to regard this circumstance in something of the typical light in which Dr. Williamson regards the phenomenon of diffusion. I think his view with regard to the relation in specific heat between the two bodies which unite and the products of their union, rests on a somewhat unsubstantial basis.

Dr. TYNDALL: I am not quite competent to follow the whole of the discourse; but the thoughts which have pervaded it are certainly quite physical in their character, and as such command my sympathy. Professor Williamson appeared at the commencement disposed to extend the term "latent heat,"—to push it beyond its present limits, and to apply it to phenomena to which the term is not now applied. I go quite with him, as far as to say, that these phenomena to which he has referred—for instance, expansion of gas, have quite an equal right to have the term applied to them as a change of aggregation; but instead of extending this term to other phenomena, because they have the same character as those where change of aggregation is concerned, I think it would be rather better—and I submit this to his consideration—to abolish the term altogether. Sooner than extend it to others, let us rather eradicate it from those phenomena to which it has been hitherto applied. I am perfectly persuaded that you will agree with me that the term is founded upon a misconception. For instance, you have a certain mechanical action for the purpose of raising that black-board. Suppose it is raised by Professor Williamson's own arm, some of the heat would be expended in the raising of the board. Would you say that that heat was rendered latent in the board? I do not know if the term will quite apply there. And thus, if we take the case of a pile-driving engine, the weight was raised up to a certain height by the action of steam; in raising it a certain height against the force of gravity a certain amount of heat would be expended. Now, there is a certain amount of gravitating force stored up in this weight, and when the weight is liberated it descends, and, therefore, an amount of heat must be evolved equal to what was expended in lifting it; but I don't think the term "latent heat" would apply to the weight suspended. Now, I would again submit to Professor Williamson—and the fixing of these terms in science is an important point—whether it would not be better to eradicate this term, which was based on the material theory of heat, and the assumption that it was actually hidden in the pores of a gas and squeezed out of this gas, such as aqueous vapour, upon condensation; and to substitute something more consonant with the real physical changes that take place.

Dr. LONGSTAFF said it seemed that the use of terms in this discussion was of great importance. He entirely concurred with the opinion of the last speaker that the term "latent heat" should be abolished: he did not believe, however, that its use had arisen from the theory of the materiality of heat. He had arrived at precisely the opposite conclusion, and it was only by retaining that theory that the term "latent" could be abolished.

Dr. WILLIAMSON: I may, perhaps, be permitted to make the remark, with regard to Dr. Odling's interesting criticism, that I was not unaware of the fact that a certain amount of agreement has been asserted between the specific heats of bodies and those of their constituents. But I think Dr. Odling must be also well aware that cases of disagreement are considerably more numerous. When

agreement was shown, the experiments in most cases were exceedingly rough. (No, no.) I think that physicists are rather inclined to lay stress upon those determinations which came somewhat near, because they thought that was the law which ought to govern the phenomena. However, I do not profess to be able to prove from the result of experiments on the subject that the specific heats show diminution generally; but the experiments certainly are not consistent with the notion of equality. A few cases of agreement are as much exceptions as anything else; and I think that the dynamic theory of heat leaves us no choice. With regard to Dr. Tyndall's suggestion about latent heat, I am not at all fond of the idea of heat being stored up, because it is not stored up; but still, when a term is in general use, I conceive it better not to be too critical. I should certainly by no means be the last to give up the term if physicists are willing to give it up; although I must say, that if that board be lifted by a mechanical force, I don't see why we should not call the heat latent in it, as well as when water evaporates and is lifted into the atmosphere; for the quantity of heat which disappears when water is converted into steam does certainly serve to lift it into the atmosphere, just like lifting a machine or a pile. However, I shall be very glad to give up the term. Dr. Longstaff's notion of the material nature of heat would lead us into questions which perhaps it would be rather difficult to settle to-night. I am not at present disposed to extend our notion of matter beyond that which is ponderable; therefore, according to that test of matter, which I am disposed to retain at present, I should not call heat a kind of matter.

Dr. TYNDALL said that if a weak current of electricity were sent in a certain direction through a bismuth and antimony junction of a thermo-electric pile, cold would be developed; or when a certain amount of heat fell upon the face of the pile it was actually converted into electricity, or another kind of force. This might be called a case of latent heat, just as in the other case.

Dr. WILLIAMSON: One just as much as the other.

Anniversary Meeting, March 30.

Professor BRODIE, F.R.S., President, in the Chair.

The Report of the Council was read, from which it appeared that the Society consisted of 342 Fellows, 30 Foreign Members, and 10 Associates. During the year there had been a loss of 3 Fellows by death, and an acquisition of 22 new Fellows, making an increase of 19. At the Ordinary Meetings of the Society, there had been 33 papers read, and 4 lectures delivered. The following were elected Officers and Council for the ensuing year:—*President*—A. W. Hofmann, Ph.D., LL.D., F.R.S. *Vice-President*, (who have filled the office of President)—W. T. Brande, F.R.S.; B. C. Brodie, F.R.S.; C. G. B. Daubeny, M.D., F.R.S.; Thomas Graham, F.R.S.; W. A. Miller, M.D., F.R.S.; Lyon Playfair, Ph.D., C.B., F.R.S.; Colonel Philip Yorke, F.R.S. *Vice-Presidents*—H. Bence Jones, M.D., F.R.S.; Robert Porrett, F.R.S.; Alfred Smee, F.R.S.; A. W. Williamson, Ph.D., F.R.S. *Secretaries*—Theophilus Redwood, Ph.D.; William Odling, M.B., F.R.S. *Foreign Secretary*—E. Frankland, Ph.D., F.R.S. *Treasurer*—Warren De la Rue, Ph.D., F.R.S. *Council*—Thomas Andrews, M.D., F.R.S.; William Francis, Ph.D., F.L.S.; J. H. Gladstone, Ph.D., F.R.S.; G. D. Longstaff, M.D.; W. Maroet, M.D., F.R.S.; John Mercer, F.R.S.; Normandy, A. R. L. M.; W. H. Perkin; H. E. Roscoe, Ph.D.; Edward Schunck, Ph.D., F.R.S.; John Stanhouse, LL.D., F.R.S.; and Robert Warington.

Thursday, April 4, 1861.

R. WASHINGTON, Esq., in the Chair.

Messrs. T. Wood, R. Collyer, J. Hearn, and F.

Norrington were elected Fellows, and Mr. J. H. Smith an Associate.

Dr. FREDERICK GUTHRIE read a Paper "On some Derivatives from the Olefines." This communication was a continuation of the author's previously read papers on the action of compound halogens on the hydro-carbons of the formula C_5H_{10} . The test of fractional solution was applied to prove the homogeneity of the bodies $C_5H_8S_2Cl$ and $C_5H_{10}S_2Cl$. When either of these bodies is partly dissolved in alcohol, the entire substance has the same per-centage composition as the dissolved part.

The relation between $C_5H_8S_2Cl$ and $C_5H_8S_2$ was established by showing that the action of chlorine, in both cases, gave rise to $C_5H_8S_2$. When $C_5H_8S_2Cl$ is recomposed by KOHO the body $C_5H_8S_2HO_2$ is formed: just as $C_5H_{10}S_2HO_2$ is formed when $C_5H_{10}S_2Cl$ is recomposed by the same reagent. Further the chlorine in $C_5H_{10}S_2Cl$ was replaced by cyanogen and sulpho-cyanogen by the action of the corresponding potassium compounds; the bodies

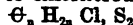
$$\begin{array}{l} C_5H_{10}S_2Cy. \\ C_5H_{10}S_2S_2Cy. \end{array}$$

being produced.

From this and previously described reactions, the conclusion was drawn that—towards chlorine bodies of the formula,



act like the sulphides of chloriferous radicles,



while towards metallic oxides, hydrated oxides, cyanides and sulphocyanides, they act like the chlorides of sulphuriferous radicles,

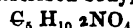


The author regarded this as additional evidence of the impotence of a single formula to express the construction or potential recomposition of a body.

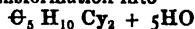
The body $C_5H_{10}S_2$ was formed when $C_5H_{10}S_2Cl$ was submitted, in alcoholic solution, to the action of Zn.

The body $C_5H_{10}Cl_2$ was produced by the reaction between C_5H_{10} and PCl_5 .

From the already described body,

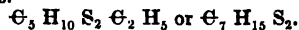


the remarkable transformation into

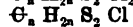
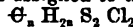


was effected by the action of cyanide of potassium. From the penthydrated bicyanide of amylen, attempts to form the pimelate of potash by the action of caustic potash, were unsuccessful.

By the action of zinc-ethyl upon $C_5H_{10}S_2Cl$, a body was formed having the composition of the bisulphide of ænanthyl, viz.—



A reply was given to M. Wurz's critique as to the rational formulæ to be assigned to the bodies,



and their derivatives.

Dr. FRANKLAND remarked upon the extreme importance of the reaction by which Dr. Guthrie had apparently succeeded in converting a member of the amylo-valeric into a member of the ænanthyl group. Supposing that by a similar reaction effected by zinc-methyl or zinc-ethyl we were able to displace the chlorine of chloride of methyl or chloride of ethyl by a radicle, we might successively build up all the primary carbon groups from one another.

Dr. ODLING referred to the analogy of the reactions of protosulphide and bisulphide of chlorine upon ammonia and amylen respectively. With ammonia we obtained the sulphochloride of chlorammonium $NH_3Cl.ClS$, the sulphide of chlorammonium $(NH_3Cl)_2S$, and the bisulphide of chlorammonium $(NH_3Cl)_2S_2$. With amylen we obtained the sulphochloride of chloramyl $C_5H_{10}Cl.ClS$, the sulphide of chloramyl $(C_5H_{10}Cl)_2S$, and the bisulphide

of chloramyl $(C_5H_{10}Cl)_2S_2$. He advocated the reference of these last compounds to the mercaptoral types.

MANCHESTER

LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, April 2, 1861.

J. P. JOULE, LL.D., President, in the Chair.

Mr. John Parry and Mr. Thomas Carrick were appointed Auditors of the Treasurer's accounts for the present Session. M. Durand Fardel, M.D., was elected a Corresponding Member. Messrs. Henry Brogden, John Thomas Hobson, Ph.D., William Alexander Cunningham, and George Robert Haywood, were elected Ordinary Members.

Mr. T. T. WILKINSON, F.R.A.S., exhibited an Amulet, which, having been worn by an African chief who was captured in war about a century ago, had been recently examined, when it was found that the centre boss of about an inch and a-half diameter contained an Arabic manuscript, the translation of which, kindly undertaken by Professor Theodores, was laid before the meeting.

Mr. WILKINSON also laid before the meeting some Geometrical Theorems.

A Paper was read by W. FAIRBAIRN, LL.D., &c., "On the Temperature of the Earth's Crust, as exhibited by Thermometrical Observations obtained during the sinking of the Deep Mine at Dukinfield."

During the prosecution of researches on the conductivity and fusion of various substances, an opportunity occurred of ascertaining by direct experiments, under favourable circumstances, the increase of temperature in the crust of the earth. This was obtained by means of thermometers placed in bore-holes, at various depths, during the sinking of one of the deepest mines in England, namely, the coal mine belonging to F. D. Astley, Esq., at Dukinfield, which has been sunk to a depth of 700 yards.

The increase of temperature in descending, shown by these observations, is irregular; nor is this to be wondered at, if we consider the difficulties of the enquiry and the sources of error in assuming the temperature in a single bore-hole, as the mean temperature of the stratum. At the same time it is not probable that the temperature in the mine-shaft influenced the results. The rate of increase has been shown in previous experiments to be directly as the depth, and this is confirmed by these experiments. The amount of increase is from 51° F. to $57\frac{3}{4}^\circ$, as the depth increases from $5\frac{1}{2}$ to 231 yards, or 1° in 99 feet; but, in this case, the higher temperature is not very accurately determined. From 231 to 685 yards, the temperature increases from $57\frac{3}{4}^\circ$ F. to $75\frac{1}{2}^\circ$. This is a mean increase of 1° in 76.8 feet, which does not widely differ from the results of other observers. Walferdin and Arago found an increase of 1° in 59 feet; at Rehme, in an Artesian well 760 yards deep, the increase was 1° in 54.7 feet; De La Rive and Marcet found an increase of 1° in 51 feet, at Geneva. Other experiments have given 1° in 71 feet. The observations are affected by the varying conductivity of the rocks, and by the percolation of water. The Author has exhibited upon a diagram, in which the ordinates are depths, and the abscissæ temperatures, the results obtained between the depths of 231 and 717 yards. The strata of the mine are also shown in section. Additional to these, the Author gives a Table of similar results in another pit at the same colliery, taken between the depths of 167 $\frac{1}{2}$ and 467 yards, and showing an increase of temperature of 1° in 106 feet of descent.

Assuming as an hypothesis, that the law thus found for a depth of 790 yards, continues to operate at greater depths, we arrive at the conclusion that at $2\frac{1}{2}$ miles from the surface, a temperature of 212° would be reached, and at forty miles a temperature of 3000° , which we may

suppose sufficient to melt the hardest rocks. The author then discusses the effect of pressure and increased conductivity of the rocks in modifying this result. If the fusing point increased $1^{\circ}\cdot3$ F. for every 500 lbs. pressure, as is the case with wax, spermaceti, &c., the depth would be increased from 40 to 65 miles before the fluid nucleus would be reached; but as the same increase is not observed with tin and barytes, the influence of pressure on the thickness of the crust cannot yet be determined. Again, Mr. Hopkins has shown that the conductivity of the dense igneous rocks is twice as great as that of the superficial sedimentary deposits of clay, sand, chalk, &c. And these close-grained igneous rocks are those which we believe must most resemble the strata at great depths. Now, if the conductivity of the lower rocks be twice as great as that of the strata in which the observations were made, correcting our former estimate, we should probably have to descend 80 or 100 miles, instead of 40, to reach a temperature of 3000° , besides the further increase due to the influence of pressure on the fusing point. On entirely independent data, Mr. Hopkins has been led to conclude that the minimum thickness of the crust does not fall short of 800 miles, in which case the superficial temperature of the crust would have to be accounted for from some other cause than an internal fluid nucleus.

A paper was read by J. C. DYER, Esq., Vice-President, entitled "*Brief Notes on the Nature and Action of Steam in Relation to Boiler Explosions.*"

He stated that several essays had lately appeared on boiler explosions, wherein discordant theories and opinions are offered on the action of steam in some anomalous cases of explosion, and which may justify the bringing before the Society a few established facts and principles relating to the subject, in the hope of arriving at more settled views concerning causes and effects in such cases than appear to prevail at present among our most distinguished engineers. The author objected to the appeals made to Dr. Dalton's theory of atoms for explaining the nature of steam as an elastic force mechanically employed, since the law of definite proportions of Dalton had no reference to elastic vapours except as to the constituents of the liquors whence they arise. He then cited the fact that water, like steam, is an elastic body, and the pressure would therefore be of the same nature and force above and below the water line in a boiler; but that explosions from fractures above or below that line would have different effects, owing to the amount of expansion of water and steam being so widely different when issuing from similar apertures and under the same pressure. Many obscure cases of explosion would be explained by the more or less rapid generation of steam issuing under those circumstances, as set forth in the paper. That free space, when suddenly and amply afforded, is to highly heated water, under great pressure, nearly the same as fire is to gunpowder. And this will account for the most destructive cases of boiler explosions; whilst those of a more harmless nature show that the fractures were small at first, and then gradually extended.

He also objected to the term "superheated steam," as being inapplicable to it in any state; because, when steam is in contact with water, it will be of the same temperature as the water; and if heated apart from water, the same laws of expansion by heat apply to steam as to air, and neither can be "superheated," though made very hot.

Again, steam can never be "mixed up with the water" in a boiler when both are under the same statical pressure, and the steam formed will rise into the chamber, so that the water will always be in contact with the boiler except when steam is drawn off. Still, in rapid escapes, it may drive out water and become entangled therewith, as in many explosions.

It having been shown that most, if not all, explosions are occasioned by simple steam pressure, acting on the

weakest parts at first, and thence extending more or less rapidly, it would seem needless to seek for any other cause or force to account for them; yet, in some cases, the effects appear to imply a more sudden and violent action, like that of explosive compounds. In such instances, may they not arise from the actual decomposition of the water by heat alone? Although we have high authority (cited) against this, yet the Author held it rash to conclude that water could not be resolved into its constituent gases by direct action of heat from the boiler upon water pressed into contact with the metal plates. It has been proved, long since, that by heat, in the most intense form known to us—that of electricity—water is decomposed and both of its constituent gases are liberated. Therefore, since no evidence has been adduced to show that this does not take place in any water when so confined and heated, the affirmative may at least be possible, and seems probable, in some instances, as before named.

However, he held it desirable that the question should, if possible, be set at rest by experiment; and to this end a method was suggested for putting the matter to a direct test; but he might not be able to make the experiment himself, and hoped it might be done by some one more competent to the task.

SECTIONS FOR STATISTICS AND SOCIOLOGY.

March 12, 1861.

A Paper was read by Mr. THOMAS R. WILKINSON, furnishing statistics connected with the sale of beer and spirituous liquors, with a view to enquire whether such sale had any effect on the criminal returns, and how far the Legislature would be justified in a further interference with it.

The number of licensed victuallers in the United Kingdom in 1860, was 107,024, and the number of beer sellers, 45,198.

The licensed victuallers represent interests which directly concern 50,000,000*l.* worth of property, and maintain 1,000,000 people.

The revenue derived from beer and spirits in the year 1832 was 15,000,000*l.*, or 29*·*41 per cent. of the whole revenue. In 1858, this revenue amounted to 21,000,000*l.*, or 31*·*34 per cent. of the whole revenue. The increase of this revenue had been 40 per cent., whilst the increase of the population was 25 per cent.

In 1831, the amount derived from licences for the sale of

	£.		£.
Beer amounted to ..	273,871	And the year 1859 to ..	384,178
Spirits ..	395,697	" ..	563,400
Wine ..	73,026	" ..	88,238
	<u>£742,594</u>		<u>£1,035,816</u>

An increase of 39*·*62 per cent.

An Act for closing public houses, from Saturday night at twelve o'clock, until one o'clock on Sunday, came into operation in 1847-8.

For eight years before the passing of that Act, the annual average number of persons taken into custody by the Manchester police was 10,658, whilst after the Act was passed the annual average was only 5371.

MICROSCOPICAL SECTION.

March 18, 1861.

A communication from Captain ANDERSON, R.M.S. Canada, written at sea on his homeward voyage, excited considerable interest. He states that Dr. Wallich's pamphlet would be communicated to the Boston Society of Natural History by Professor Agassiz, who was particularly interested in the *Ophiocoma* found at so great a depth. The Professor is now engaged in preparing a work upon

the natural history of that class of echinoderms, which he has studied for many years, and claims to have the finest collection of those animals in existence, made on the coasts from Greenland and Labrador to Mexico and round Cape Horn to California. Since the publication, in 1843, of the "Principles of Zoology," by Agassiz and Gould (a copy of which is presented to the Section), the Professor has ascertained that the system of tubes and water pores, described at page 123, exists in all animals which much vary their depths of water in the sea, and in the herring they may be seen with the naked eye along the side of the neck.

With reference to the removal of tallow from soundings, Dr. Hayes, the Assayer for the State of Massachusetts, stated to Captain Anderson that heated turpentine poured amongst the soundings, will remove all the tallow with it through filtering paper; the operation should be twice repeated, and the residue finally washed with sulphuric ether.

Dr. J. BACON presented a copy of his Report upon the Chemical Composition and Microscopical Characters of the Pearl, said to have been formed in the interior of a cocoa nut at Singapore, in the possession of Frederick Bush, Esq., and exhibited by Dr. Winslow.¹

Captain Anderson, in a very able manner, gives the outline of a plan which has occurred to him for rendering available to science the services of commanders of merchant vessels and seamen generally, in collecting specimens of natural history and information in natural science, whether in zoology, ethnology, botany, or meteorology, for which they have such facilities in all parts of the world, for the use of those scientific institutions which may desire to join in it and also with a view to elevate the mercantile marine of England in the social scale by stimulating a taste for such knowledge amongst seafaring men. The consent and co-operation of ship owners will of course be necessary, and Captain Anderson seeks the additional influence of merchants and scientific bodies. The subject met with the unanimous approval of the members present; and it was resolved that the portion of Captain Anderson's letter relating to it should be published at the expense of the Section, for the purpose of eliciting opinions upon the feasibility of the scheme, and upon the best practical method of carrying it into execution.

Commander M. F. MAURY, U.S. Navy, forwarded copy of a letter from Lieutenant John M. Brooke, the inventor of the Detaching Deep Sea Sounding Apparatus, and enclosing for the Section a number of soundings from the North Pacific Ocean, which were obtained with small twine and spherical weights of about 70lbs., which were detached upon contact, and left at the bottom of the ocean; Lieutenant Brooke observes that "Nine consecutive casts (soundings), varying from 2000 to 2900 fathoms, were made with the same piece of twine and detaching apparatus, which last weighed less than 1lb." As the specific gravity of a wet flax line is nearly that of water, a line that can be pulled down by a weight, may be pulled up by hand, provided the weight be detached at the bottom. One of the specimens obtained in 3030 fathoms, nearly three and a-half miles, is the greatest depth from which material has yet been brought up from the ocean bed. Lieutenant Brooke sent also a few specimens obtained by soundings in shallow waters, on the east coast of Nippon, Japan, by him during his boat voyage from Simoda to Halloedadi in 1855, under the orders of Commander Rodgers, U.S. Navy.

Mr. BINNEY described to the Section the appearance of certain nodules found in the middle of a seam of coal, in the lower part of the Lancashire coalfield which contain fossil wood associated with marine shells. Specimens of the former were exhibited to the members, the most perfect of which was that of *Sagenaria*, the old *Lepido-*

dendron elegans, in transverse, parallel, and tangential sections. The marine shells associated with the fossils belong to the genera *Aviculopecken*, *Orthoceras*, *Nautilus*, &c.

Mr. BROTHERS exhibited a Section of Pearl, *Isthmia nervosa*, infusoria, &c.

Mr. WHALLEY exhibited living Diatomaceæ from Southport.

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 28, 1861.

Mr. E. W. BINNEY, F.R.S., F.G.S. &c., in the Chair.

Professor Clifton, of Owens College, was elected a member of the Section. The following gentlemen were elected Officers of the Section for the ensuing year:—*President*—Mr. Robert Worthington, F.R.A.S. *Vice-Presidents*—Mr. Baxendell, F.R.A.S.; Mr. Binney, F.R.S., F.G.S. *Treasurer*—Mr. George Mosley; *Secretary*—Mr. Thomas Heelis, F.R.A.S.

A Paper was communicated by Mr. ATKINSON, from Mr. T. T. Wilkinson, F.R.A.S., of Burnley, entitled "Geometrical Theorems relating to the Triangle and its Inscribed and Escribed Circles."

Mr. G. V. VERNON, F.R.A.S., produced Manuscript Copies of thirteen years Barometrical Observations made at Greenwich, which had been kindly communicated by the Astronomer Royal, showing the observations once a day for every day instead of the daily means, and presented such series to the Section, for which present he received its thanks.

Resolved, that the Secretary be requested to prepare and issue a circular, applying for copies of Logs and Meteorological Observations made at sea, and especially on board the various mail steamers.

PHARMACEUTICAL SOCIETY.

Wednesday, April 3, 1861.

T. N. R. MORSON, Esq., President, in the Chair.

The first paper read was "On the Preparation of Smelling Salts," by Mr. A. ALLCHIN. Mr. Allchin considers the ordinary method of making smelling salts by filling a bottle with small fragments of sesquicarbonate of ammonia and adding a little perfume and some strong liquor ammonia to be altogether unsatisfactory. The contents of a bottle so filled soon lose their pungency, and a nearly inodorous residue remains. His own plan is first of all to convert the sesquicarbonate into the monocarbonate of ammonia, which is accomplished in the following way. Forty ounces of sesquicarbonate of ammonia are broken into fragments about the size of filberts and placed in a jar having a well fitting lid. Into this is afterwards poured 20 ounces of liquor ammonia, sp. gr., 880°. This mixture is frequently stirred for a week, and the jar is then set aside in a cool place for three or four more weeks. If the mixture is not stirred for the first week it sets as hard as a stone; but after stirring it becomes solid and dry, but can be easily removed from the jar. It is now reduced to a roughish powder something like salt of tartar, and in that state it is ready for filling the bottles, and improves by keeping. When placed in the bottles some volatile essence or strong ammonia perfumed with essential oils is added. The volatile essence Mr. Allchin uses and recommends is the first given in Professor Redwood's edition of Gray's Supplement to the Pharmacopœia, and is as follows:—Take of English oil of lavender and essence of musk, of each four drachms; oil of bergamot, two drachms; oil of cloves, one drachm; otto of roses, ten drops; oil of cinnamon, five drops; strongest liquor ammonia, one pint. In the above way, a salt is made which retains its pungency as long as any remains in the bottle. One that

¹ Page 290, vol. vii., Proceedings of the Boston Society of Natural History for 1860.

was filled five years ago was exhibited to the meeting, and although nearly all the contents had evaporated, what remained still possessed a pungent, agreeable odour.

Mr. SQUIRE noticed the brown colour of the salt, which he attributed to the oil of cloves in the perfume, and suggested that the salt would be improved if it were quite colourless. He also asked why Mr. Allchin preferred an aqueous solution of ammonia to the alcoholic. It was well known that a large quantity of a particular smelling-salt was exported from England, and Mr. Squire said he believed this salt owed its superiority and its fame to the alcoholic solution of ammonia used in its preparation.

Mr. WAUGH said he preferred a salt which was not liable to change colour. Great discredit attached to articles which changed in appearance after leaving the hands of the vendor, and it was a great matter to avoid the possibility of such changes.

Professor REDWOOD considered the scent quite a secondary matter. The most important thing was the preparation of a salt which should retain its pungency a long time, and Mr. Allchin had shown how such a salt might be prepared. Sesquicarbonate of ammonia was a compound of two salts, one pungent and one scentless. The one soon evaporated, and what was left in the bottle was worthless. It was important to obviate that. Rose's method of procuring a perfectly volatile carbonate of ammonia was to distill the sesquicarbonate quickly in a retort, when the part which condensed furthest from the body of the retort was the neutral carbonate. Mr. Allchin's method was much simpler. In the common way of filling a smelling-bottle there was no chance of adding sufficient liquor ammonia to convert the sesqui- into the mono-carbonate.

Mr. ALLOCHIN said he preferred the aqueous to the alcoholic solution of ammonia, because he believed the latter did not contain enough ammonia to effect the conversion of the sesquicarbonate, and because the water combined chemically, and formed a definite compound.

Mr. HASLEDEN said that smelling-salts could be made colourless if the oil of cloves were omitted from the perfume. The use of alcoholic ammonia, he thought, would be expensive, and of no practical value.

The CHAIRMAN said the Society was much indebted to members who brought practical subjects like this before the meetings. He believed it was preferable to use aqueous ammonia, for the reason that the water entered into combination with the salt, which the alcohol would not.

Mr. SQUIRE could not agree with the Chairman. Alcoholic ammonia must possess some advantages, or it would not have been used by those who had exported the salts for more than a hundred years. He believed, too, that alcohol could be made to take up as much ammonia as water if the condensation was effected at a low temperature. Mr. Squire then referred to the preparation of sal-volatile, and repeated, that as the cloves were the cause of the coloration of this preparation when made according to the Pharmacopœia, he hoped they would in future be omitted from the formula. The oil of cloves had a high boiling point, and did not come over until all the spirit had passed; but as the carbonate of ammonia was not very soluble in spirit, it was necessary to have some water, and with it the oil of cloves distilled. He thought it would be found convenient to have two preparations: one like the Edinburgh, a simple solution of caustic ammonia in spirit; and another, like the London compound spirit of ammonia, without the cloves.

A MEMBER stated that a mixture of eight parts chloride of ammonium, four parts carbonate of potash, and two parts sesquicarbonate of ammonia made a superior smelling salt.

Mr. HASLEDEN then read a short paper on "*The Tincture of Actea Racemosa*," which he believes is best made by percolation. The tincture so prepared has a darker

colour, a greater density, and seems in every way preferable to one made by maceration. Proof spirit was said to be the best menstruum, and the proportions used were four ounces of the root to a pint of spirit.

After some inquiries as to where the actea root could be purchased,

Mr. HANBURY inquired if any member could tell where black hellebore root could be bought. He believed it was not to be found in the market. Of four samples he had purchased as black hellebore, two were actea, and the others neither actea nor hellebore.

Mr. SQUIRE said when he wanted black hellebore root he dug up the Christmas rose.

Professor BENTLEY remarked on the striking resemblance between the roots of the actea and black hellebore. All the specimens of what was called black hellebore in the market he had found to be the roots of the actea spicata. The adulteration or substitution was not a recent one, but it was unknown to Dr. Pereira. Specimens called black hellebore, which had been in the Museum fifteen years, were actea root.

Professor REDWOOD exhibited to the meeting an improved form of Condenser; and Mr. HANBURY called attention to two fungoid bodies—*Mylitta lapidescens* and *Sclerotium stipitata*—which were used as medicines in India.

This was the last Pharmaceutical meeting of the season.

CORRESPONDENCE.

Copper in Mineral Waters.

To the Editor of the CHEMICAL NEWS.

SIR,—A number of the *Comptes-Rendus* has just been shown to me. I notice in it that M. Béchamp, in a letter to M. Dumas, "On the Presence of Copper in the Mineral Water of Balaruc," has the following, with regard to the cupreous sulphate it contains:—"It (*i.e.*, the sulphate of copper) explains the purgative properties of this water much better than the presence of the other mineral ingredients." How can this be? The water, according to the analysis of St. Pierre, holds three magnesia salts in solution, and to these ought to be attributed the aperient properties of the spring. Sulphate of copper is an astringent. How could M. Béchamp make so wrong a statement? Pereira says, "As an astringent it has been used with great benefit in chronic diarrhœa and dysentery."—I am, &c.

SHERIDAN MUSPRATT, M.D., &c.
Professor of Chemistry.

College of Chemistry, Liverpool.

The Adulteration of Food.

To the Editor of the CHEMICAL NEWS.

SIR,—Although it is far from my desire to criticise too minutely the communications of those who hold an opposite view to myself on this question, yet I think I may be allowed to remark that Mr. Scott's last letter on this subject is scarcely characterised by that simple diction and purity of style which we have a right to expect from one who is so ready at pointing out the deficiencies of others in these respects. Mr. Scott evidently avoids giving a direct answer to any question that may be asked, and the manner in which he attempts to twist the obvious meaning of words to suit his own ends comes with a very bad grace from one who takes me to task for not bringing forward my opinions in a "simple, straightforward manner," complains of my "perversion of his statements and contortion of his writings," and suggests most gratuitously that I have some "mysterious" purpose to serve in stating

my views on this question; in short, his arguments and train of reasoning appear to me to be those of one who is anxious to support former statements rather than of one who is simply desirous of arriving at the truth. My question regarding essences admitted of a full and satisfactory answer being given in a very few words, and there was certainly no necessity for endeavouring to set it aside, in a manner which is evidently intended to be anything but complimentary.

My "useless" query as to the quantity of alum found was based upon the belief that many chemists were in the habit of weighing as alumina a precipitate which often contained a large proportion of phosphates. In following the published methods for the detection of alum in bread, I had found it necessary to adopt some means for the separation of phosphoric acid from the precipitate produced by ammonia before I could obtain results that were to be depended upon. Your very excellent article on this subject in No. 70 of your Journal so fully enters into all the bearings of the case that it is unnecessary for me to say more than express my belief that had such a method of analysis as that you propose been adopted, we should never have heard of bread containing such large quantities of alum, nor would the use of this adulterant have been asserted to be so general.

After stating his "utter inability to reply to the interrogation," and much preliminary nonsense about arithmetical means, real and theoretical quartern loaves, &c., Mr. Scott condescends to furnish the information which it must have been quite evident the question was intended to elicit. It seems that the quantities found by Mr. Scott were very variable, and ranged between the extremes of 19 and 182 grains to the quartern loaf. Presuming that Mr. Scott, in the prosecution of these analyses, adopted the method which he has publicly recommended for the detection of this adulterant, it is evident, from your own observations, that a considerable proportion of the precipitate which was weighed as alumina consisted really of phosphoric acid, lime, and magnesia, and it is impossible to say how far the extreme variation of the weight of the precipitate was due to that circumstance. It is very certain, however, that his results must be open to correction, and that his assertion—to adopt a favourite expression of his own—can only be taken for what it may happen to be worth. It appears from a letter of Dr. Odling, in your last week's Journal, that the information respecting his analysis of bread, which was published in a letter to the Editor of the *Journal of the Society of Arts*, was altogether erroneous, and that the fact is, that out of 40 samples examined, he found 31 to contain alum. Now, I am quite ready to admit that this is so much evidence in favour of Mr. Scott's assertion; but, granting that every precaution had been taken, and the presence of alum fully proved in these 31 samples, yet I feel confident that Dr. Odling would be very far from saying, on the strength of the examination of 40 samples, that 77 per cent. of the bread of London is adulterated. The question whether the presence of rice and potatoes is indicated by brittleness of structure, or crumbly texture, is one upon which those who are interested in the matter can easily satisfy themselves by direct experiment. The next time I have sample loaves made from flour purposely adulterated with known quantities of foreign ingredients I shall be most happy to furnish you with samples, to be forwarded to Mr. Scott, on condition that he will analyse them and allow a statement of the quantity of each ingredient he finds to be published in your Journal, along with that of the quantities of the adulterants actually employed, which shall be forwarded to you in a sealed letter, to be opened on the receipt of Mr. Scott's numbers. As regards the reliance which may be placed on the evidence of merchants and manufacturers, the idea that men of the standing of such firms as Messrs. Huntley and Palmer, Messrs. Phillips and Co., and Messrs. Crosse and Blackwell require

a certificate of character from Mr. Scott before their statements may be quoted as worthy of credence can only excite a smile. By what right does Mr. Scott assume that his statements are entitled to be received as truth, whilst the testimony of men whose knowledge of the subject has been acquired from long experience is to be disregarded? or, why is it to be supposed that, whilst self-interest warps the judgment of the merchant or manufacturer, it can have no such influence on the professional man?

It is very certain that nothing can tend more to shake the faith of the public in chemical science than the assumption on the part of its professors of a position to which it is clear they have no title.

It is, moreover, unfortunate that we find these very lofty ideas associated with a course of conduct with which they are strangely inconsistent. It is surely beneath the character of one who claims to be considered as a man of science to associate himself with the "puffing" advertisements of the day, or lend his name to statements which are intended to serve the same purpose among the educated classes as is effected by "Cheap Jack" among the lower orders, by means of enormous handbills and a voluble tongue.

The neighbourhood of London has lately been favoured with an abundant supply of circulars from Messrs. Dakin and Co., announcing that they have "retained the scientific assistance of Dr. Hassall, M.D. Lond., Analyst of the *Lancet* Sanitary Commission, Author of 'Food and its Adulterations,' 'Adulteration Detected,' and other works." Messrs. Dakin and Co. have magnanimously resolved to supply an alleged defect in the Adulteration Act, and have determined upon giving a special warranty with every article of food or drink they supply, and thus to "render themselves liable under the Act," as if it were compulsory to do so, whilst the public are further to be protected by a Quarterly Report from Dr. Hassall of the results of the analyses of articles purchased from time to time at their establishments.

After this flourish of trumpets, let us see what it all amounts to:—

Tea, coffee, arrowroot, sago, tapioca, and various others are warranted "genuine as imported."

Cocoa, chocolate, and mustard, "genuine as received from the makers."

Mixed spice, "genuine as received from the grinding-mills."

It is, in fact, simply a declaration that Messrs. Dakin do not for themselves adulterate, a fact which I am disposed to believe to be true as regards the greater number of respectable houses in the trade.

Mr. Scott's assertion about strychnia in bitter beer already serves as a heading for the hand-bill of a Westminster brewer, whilst Messrs. Brown and Polson make Mr. Scott their mouthpiece through whom they cry their wares and seek the public custom.

Does it ever occur to these advertising gentlemen that a chemical certificate for purity can only be regarded as appertaining to the sample actually examined, or to the bulk which such sample strictly represents, and can afford no criterion whatever of the quality of other samples sold by the same firm?

Surely it is the duty of those chemists who have any regard for the credit of their profession to discountenance in every possible way practices which can only tend to bring upon it contempt and ridicule. The position of an analytical chemist is undoubtedly a high one, and the tasks he has to perform involve much labour, careful research, and patient study. He ought however to recollect that he draws to some extent, on the faith of the public who have to take much upon credit, inasmuch as they have often not the means of disproving his assertions. Of all classes of professional men, it behoves him to be especially careful to guard against anything either in his conduct or statements which by any possibility can have

a tendency to lessen that confidence which as a class they are warranted in claiming.—I am, &c.

VERAX.

Alum in Bread.

To the Editor of the CHEMICAL NEWS.

SIR,—In reading the various letters in your valuable paper on this subject, and the difference of opinion thereon, the suggestion occurred to me that it would be much better that the analyst could say, "There is the alum obtained from the bread," which he can easily do if he is the chemist he professes to be, instead of trusting to a precipitate, which is doubtful. In a case of poisoning by arsenic the poison has to be produced in court. Why not the alum likewise? It is to be hoped that any baker prosecuted for such adulteration will insist on the production of the alum with which he has been said to adulterate his bread. I saw the alum produced lately from an ounce and a-half of bread to such an extent that it could be judged of by the taste and pronounced such by parties tasting, unaware of what it was or whence it came.

I am, &c.

STRAIGHTFORWARD.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Action of Nitrous Acid on Cuprous Oxide.—Lenssen mentions (*Journal für prakt. Chemie*, Bd. lxxxii. s. 50) a curious reaction of nitrous acid. If a drop of red fuming nitric acid be added to a tolerably dilute solution of cuprous chloride rendered strongly acid by hydrochloric acid, a deep indigo blue colour is produced of magnificent intensity. A similar result follows when a solution of nitrite of potash is used instead of red fuming nitric acid. On heating the mixture with the addition of an alkali the reaction is destroyed. By adding ammonia carefully, the blue colour of the ammoniated copper compound may be seen at the upper part of the test-tube, and in the lower part the indigo blue of the Cu_2O , NO , (?) the two being separated by a stratum of clear liquor. A solution of cuprous chloride indicates the presence of a very small amount of nitrous acid, but the reaction only takes place in a strong hydrochloric acid solution. Sulphuric acid, however, may replace hydrochloric acid, a mixture of equal volumes of hydrated sulphuric acid and water being used, and the reagents being added after the mixture has cooled.

Preparation of Crystallized Soda Alum.—Gentele describes (*Journal für prakt. Chemie*, Bd. lxxxii. s. 56) the following method of getting crystallized chrome and iron soda alums:—Dissolve one equivalent of soda alum in a small quantity of water, and one equivalent of potash chrome alum in much cold water without the assistance of heat, and then mix the solutions. Some ordinary pale, reddish-coloured potash-alum immediately separates, and on spontaneous evaporation more, in large crystals. When the fluid has evaporated to a small bulk, it is separated, and a layer of alcohol is poured over without mixing, and the vessel is set in a cool place, whereupon after some time, octahedral crystals of soda chrome alum shoot out. These are not red like the potash chrome alum, but green. Crystals of soda iron alum can be obtained in exactly the same way. These are colourless.

II. ORGANIC CHEMISTRY.

Solid Acids of Cocoa Butter.—Oudemans has carefully examined fresh cocoa butter, and decides that no acid having the formula $\text{C}_{22}\text{H}_{34}\text{O}_4$ (the cocinic acid) is present. The solid acids, he says, (*Scheik. Onderz.* D. iii. p. 84) are a mixture of a little palmitic and myristic with much lauric acid.

New Salts of Oxamic Acid.—E. Bacaloglo describes (*Journal für prakt. Chemie*, Bd. lxxxi. p. 379) some new oxamates which he has prepared in Erdmann's laboratory:—

Neutral Oxamate of Lead, $\text{PbO} + \text{C}_4\text{H}_2\text{NO}_5 + \text{HO}$, obtained when oxamate of ammonia is decomposed by neutral acetate of lead, is a white crystalline salt, easily soluble in hot and slightly soluble in cold water. The solution has an acid reaction. At 100° the salt loses its equivalent of water, without undergoing any apparent change, and it may be heated to 175° without further decomposition.

Basic Oxamate of Lead, $2\text{PbO} + \text{C}_4\text{H}_2\text{NO}_5$, is a white powder insoluble in water, obtained when oxamate of ammonia is precipitated with basic acetate of lead. When treated with boiling water this salt is obtained in an anhydrous state.

Oxamate of Copper, $\text{CuO} + \text{C}_4\text{H}_2\text{NO}_5 + \text{HO}$, is a blue granular powder, soluble in hydrochloric acid, and slightly so in water and nitric acid. When a dilute solution of oxamate of ammonia is added to a moderately strong solution of sulphate of copper only a cloudiness is produced; but on boiling, or on the addition of a small quantity of nitric acid, the greater part of the oxamate of copper separates. Like the basic lead oxamate, the copper salt appears to lose water on boiling.

Oxamate of Nickel, $\text{NiO} + \text{C}_4\text{H}_2\text{NO}_5 + \text{HO}$, is most easily made by mixing hot solutions of oxamate of ammonia and sulphate of nickel. It is a greenish white, granular powder, soluble with difficulty in hot water.

Oxamate of Iron, $\text{FeO} + \text{C}_4\text{H}_2\text{NO}_5 + \text{HO}$, forms beautiful yellow microscopic crystals. Freshly precipitated ferric oxide, boiled with a solution of oxamic acid dissolves, forming an oxamate of ferric oxide, this exposed to direct sunlight slowly decomposes, evolving some gas, and a yellow precipitate falls, which, probably, contains the oxamate of ferrous oxide.

Oxamates of Mercury.—By decomposing the ammonia salt with per- or proto-nitrate of mercury, the quicksilver salts are obtained as white, insoluble powders.

Reactions of Oxygen.—Schönbein continues (*Sitzungsb. der Königlich. bayer. Akad. d. Wissenschaft*, July, 1860) his important investigations on the reactions of oxygen in its three modifications. Some of these (on Aniline) we have given already; at page 98 of this Volume. We now add some others.

Action of $\ominus$ on Hæmatoxylin.—White filtering paper dipped in a concentrated ethereal solution of hæmatoxylin appears when dry of reddish yellow colour. Introduced into strongly ozonised air it quickly changes to a brown red, and at last becomes bleached, the paper then having a sour taste, and reddening moistened litmus paper. Powdered hæmatoxylin in a watch-glass exposed to the action of strongly ozonised air is soon coloured as red as peroxide of iron, then becomes moist and changes to a brown mass, which eventually turns to a colourless liquid that tastes strongly acid. An aqueous solution of this matter gives a white precipitate with lime water, which the author concludes is oxalate of lime. Combined $\ominus$ behaves in the same way. The paper, prepared as above and moistened with water, is soon coloured by the peroxides of manganese, nickel, &c. Dipped in the solution of a hypochlorite or permanganic acid it is instantly changed to a dark red.

Behaviour of $\oplus$.—Hæmatoxylin dissolves in water containing HO_2 , giving a colourless solution; but for this experiment to succeed, not the least trace of an alkaline substance must be present. The mixture very gradually becomes violet. Turpentine containing $\oplus$ behaves like HO_2 .

Behaviour of Neutral O.—A colourless aqueous solution of hæmatoxylin in contact with ordinary oxygen is slowly reddened, and after twenty-four hours becomes deeply

coloured. The coloration is hastened by making the solution of hæmatoxylin hot. Schönbein has not yet succeeded in detecting HO_2 in the coloured liquid, but he believes some to be formed. A hundred grammes of the hæmatoxylin solution, containing one-thousandth part chromogen, shaken up in a two pint bottle, with five or six drops of a strong soda solution and atmospheric air, until the liquid becomes a deep cherry colour (which happens in the course of half-a-minute), and then immediately supersaturated with a little dilute SO_2 , gives a yellowish mixture which possesses the following properties:—

1. Pure ether shaken up with an equal volume of the mixture and a drop of a dilute solution of chromic acid is coloured distinctly blue.

2. The mixture decolorises a solution of permanganate of potash, setting free O gas.

3. It turns a solution of indigo green, which only gradually changes to a yellow, but the addition of a drop of a dilute sulphate of iron solution quickly effects the change.

4. After shaking up with platinum black, peroxide of lead, &c., the mixture loses the power of producing the foregoing reactions.

These facts leave no doubt in the author's mind that a small quantity of HO_2 is formed during the oxidation of hæmatoxylin effected under the influence of soda, apparently by neutral O ; and he believes, therefore, that the chemical polarisation of neutral oxygen precedes the oxidation of hæmatoxylin, which is only effected by means of the Θ .

MISCELLANEOUS.

The Electric Light.—The French Minister of Marine has contracted for the establishment of eight electric lights on the coast between Cape de la Heve, near Havre, to Treport, east of Dieppe. The object of these lights is to maintain a communication with ships within sight of land, and to transmit the news rapidly to the interior.

The Atlantic Cable.—The report of the Atlantic Telegraph Company states that in the cable recovered and brought home by Captain Kell, there was not the slightest symptom of deterioration or decay in the gutta-percha. It had been subjected to a severe electrical test, and a comparison between its present state of insulation, and the records of original tests of the most perfect portions of the cable, when it left the gutta-percha works, three years ago, showed that an actual improvement had taken place in its conditions since it was laid down.

Royal Institution.—The following is the arrangement of Lectures for the ensuing week:—Tuesday, April 23, at 3 o'clock, Professor Owen "On Fishes." Thursday, April 25, at 3 o'clock, Professor Tyndall "On Electricity." Friday, April 26, at 8 o'clock, Professor Owen "On the Scope and Appliances of a National Museum to Natural History." Saturday, April 27, at 3 o'clock, Max Müller, Esq., "On the Science of Language."

Bricks and Tiles made by Machinery.—We have been much pleased with an inspection of Eccles' Patent Brick and Tile Machine now on view at Messrs. Dray, Taylor, and Co.'s, London Bridge. The machine is about 12 feet long by 9 broad and is capable of producing 24,000 bricks in ten hours. The clay being taken direct from its native state, and made into a brick in a few minutes, without any previous expensive preparation. They are made perforated, which gives them many advantages over bricks of ordinary pattern. The following is the mode of employing this machine for making bricks:—The clay is taken from the embankment and placed into

waggons, which run on a tram-road on a slight incline to the hopper, fixed at a height of about four feet six inches from the ground level, thus dispensing with the expense of fixing hoisting machinery, and power for working it. If the clay field is on an incline, the waggons may be run on to the hopper platform on a level. In the hopper are placed two sets of knives or agitators, working transversely, through which the clay passes and is cut up into small pieces, preparing it for the crushing rollers, of which there are three, placed at distances apart to suit quality of clay, thence passing into a horizontal cylinder fitted with the ordinary pug-mill shaft and blades, working on the screw principle, and is then forced by a side knife or wiper, through the dies on the tables fitted with rollers, where the mass is divided by wire cutters, (placed at equal distances apart) into the required size for Bricks. They are then removed on trays to the drying shed, where they remain until ready for the kiln. The drying sheds are heated by steam and hot air pipes, the former by using up the exhaust steam from the engine, and the latter by pipes inserted in the boiler flue, (not, however, interfering with the working of the boiler in any way, and no extra fuel being required.) The hot air is driven by a fan into the sheds through pipes, which when they reach the sheds are perforated, to allow the air to escape, and the heat to be equally diffused amongst the bricks. This plan of drying commends itself as the most economical extant.

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the *CHEMICAL NEWS*, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 5d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part III. of the present Volume is now ready.

* In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

* All Editorial Communications are to be addressed to Mr. CROOKES, and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the *CHEMICAL NEWS*, containing a copious Index, is now ready, price 12s., by post, 12s. 6d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

M. P. S.—You can easily concentrate the nitric acid by mixing it with oil of vitriol and distilling. If required to be quite free from sulphuric acid, it must be rectified a second time over nitrate of potash.

Reginald R.—Geranium oil may be detected in rose oil by mixing with concentrated sulphuric acid. If geranium oil be present, a peculiar unpleasant odour is produced, whilst the odour of pure rose oil is not affected by sulphuric acid. We do not know a quantitative means of estimating this adulteration. The strength of an essence may be roughly determined by mixing with three times its bulk of water and seeing how much oil is separated. This is, however, not very trustworthy.

T. C. Bell—Your idea is far from new. It has been proposed frequently, and Ransome's process is in effect the same thing. He uses silicate of soda and chloride of calcium, producing silicate of lime in the stone, and chloride of sodium which washes off. This is obviously better than your suggestion of using chloride of barium and sulphate of lime.

J. B.—Either sulphate of magnesia or molybdate of ammonia. We rather prefer the latter.

L. M. N.—It is now in type, and will be inserted as soon as we can find space.

Books Received.—"On the Application of Different Manures to Different Crops." By J. B. Lawes; and "On the Comparison of Oxen, Sheep and Pigs." By J. B. Lawes and Dr. J. H. Gilbert.

THE CHEMICAL NEWS.

VOL. III. No. 73.—April 27, 1861.

THE NEW BLUE DYE, "AZULINE."

For some months there has been a blue colour in the market which has caused an endless series of inquiries as to its nature and origin. We have been at some pains to procure information about it, but, at present, to no purpose. One of our informants stated it to contain Prussian blue, but, as it leaves no residue on ignition, this is evidently incorrect. It differs from the aniline dyes in many particulars, not the least of which is its requiring the bath to be acidulated with sulphuric acid. This treatment with sulphuric acid has also to be resorted to after dyeing, to remove a reddish impurity which tenaciously adheres to the goods, and can only be removed by acids. Some chemists have supposed it to be prepared from chinoline, but this appears to us very unlikely; for, in the first place, chinoline blue "takes on" with wool or silk with the most extreme readiness, whereas azuline dyes wool with difficulty, but silk with tolerable ease. Azuline, moreover, resists the bleaching influence of light as well as indigo, whereas a piece of wool dyed with chinoline blue, and exposed to a brilliant light, soon begins to fade. In fact, azuline reminds one of indigo in more ways than one; its solubility in methylated spirit is, however, quite contrary to our notions of an indigo colour. Any person wishing to know the shade of colour of a solution of azuline may get an idea of it by boiling aniline in a test-tube with a little indigo. The hot aniline dissolves indigo, forming a solution having almost exactly the tint of the solution of azuline in methylated spirit. On cooling, the indigo crystallises out in beautiful coppery spangles. This method of crystallising indigo from a solution in boiling aniline does not appear to be generally known to chemists.

The smell of some of the solutions of azuline seems to betray a tarry origin; but, considering the number of operations which a colour undergoes before its final solution in spirit, such evidence loses most of its value. We are aware that naphthalamine can be converted into a blue dye; but our experience of it is, that it is fugitive, and in other respects unlike azuline. Nevertheless, there was one peculiarity in a specimen of blue which we prepared from naphthalamine, reminding us of azuline: *it bore the action of acids*. Another property of azuline, which seems to remove it from the aniline dyes, is its comparative insolubility in cold water, and the complexity of the process required for dyeing it properly on wool or silk. The annexed formula, which accompanied a specimen of azuline imported from Paris, will show this complexity very forcibly:—

Directions for Dyeing with Azuline on Silk.

"**Solution.**—The azuline is placed (in the cans, as delivered), for half-an-hour, in a water-bath, heated to about 120°, and is well agitated from time to time. It is then filtered through a fine linen fabric, and the clear fluid is ready for dyeing. Great care should be taken to ascertain that all the colouring-matter is either dissolved

or in suspension. What remains in the filter should be dissolved in methylated spirit, filtered and used up. If any colour still remains, again dissolve and filter; for all, or nearly all, the colour will be dissolved in the fluid sent with it if due care is taken.

"**Dyeing.**—Dye in a bath slightly acidulated with sulphuric acid, and at natural temperature. Add the necessary quantity of azuline, in five or six successive portions, and after each addition of azuline work the silk quickly in the bath, and when the desired shade is obtained carry the bath to the boil by means of a jet of steam. Work the silk again well in the bath as it brightens the blue. The bath, being then of a dirty red, is thrown away, and a new bath made with boiling water, strongly acidulated with sulphuric acid; work the silk in this, passing it through seven times. The blue is again improved by losing its red tint. Wash the silk in cold water, and then pass it through a soap bath; wash, and pass it through a warm bath, slightly acidulated with sulphuric acid. The silk is then wrung out and dried.

"**N.B.**—The first bath should be cold to obtain uniformity; the boiling water with sulphuric acid gives brilliancy to the colour. It is of the greatest importance that the directions for entirely dissolving the colour be attended to. One gallon will dye about 3 lbs. of silk medium shades, or at about 2s. per pound of silk."

It will be seen, by the directions for dyeing, that azuline is only recommended by the inventor for silk, and, indeed, it is very troublesome to apply to wool. This, again, is entirely unlike the characteristics of aniline dyes generally.

We have written the above with the view of directing the attention of practical dyers and scientific men to azuline, chiefly with a hope that they will be able to throw some light upon its history. Although appearing to possess some valuable qualities, it is the least known of the many new dyes owing their existence to the recent progress of organic chemistry.

We need scarcely say that we shall be happy to receive communications upon the subject from any one who has real information to give.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On a New Colouring Matter (Rhamnozanthine) extracted from the bark of the Buckthorn (Rhamnus frangula), by T. L. PHIPSON, Ph.D., Member of the Chemical Society of Paris, &c.

IN the year 1858 I discovered, by accident, in the bark of the buckthorn (*Rhamnus frangula*) a very remarkable colouring matter. Upon plunging into water, the little baskets in which the butter of Brittany is sold at Paris, I observed that the wood of which these baskets

are constructed communicated to the water a vivid yellow tint, which finally became red, and then brown.

At first I was inclined to believe that the colouring matter came from the butter, which had probably been tinted with some well-known yellow dye in order to improve its appearance. I soon convinced myself, however, that this was not the case, and that the colour came from the wood of the baskets. The first thing to be done, therefore, was to ascertain with what species of wood the little butter baskets were constructed. It was not without much difficulty that in the course of three weeks I found that the wood in question was evidently that of the buckthorn (*Rhamnus frangula*).

In fact, when the branches of this shrub were placed in water, the liquid took the same yellow tint obtained with the fragments of basket, and those of the purgative buckthorn, *R. catharticus*, coloured the water in a similar manner.

In the next place I endeavoured to ascertain in what part of the tree this colouring matter was lodged, and I found that it occupied the cells of the layers of *liber* and the vessels of the *medullar tube*. In no other parts of the plant could I detect it. It is easily recognised with the naked eye in these two places upon the dried branches, but in the living shrub recourse must sometimes be had to re-agents in order to render its presence evident—which, however, never fails to be the case. Moreover, I found that if the branches be submitted to maceration for some days, their whole structure is imbibed with the colouring matter, which appears to prove that in the living plant the latter exists in solution. I then isolated the colouring principle in the manner hereafter described, and submitted it to a detailed examination.

Finally, it was necessary for me to ascertain if any other chemist had ever observed this same colouring principle, and, after having gone through a great many publications, I at length found that M. Buchner, a distinguished chemist of Munich, had extracted a yellow colouring matter from the bark of the root of *Rhamnus frangula*, to which substance he had given the name of rhamnoxanthine, and which was evidently the same substance I had extracted from the bark of *R. frangula* and *R. catharticus*. His observations were recorded in a short note published in the *Journal de Pharmacie et de Chimie*, for October, 1853. I published a slight account of this substance, under the name of Rhamnoxanthine, in the *Journal de Pharmacie et de Médecine*, of Brussels, in 1858, and I afterwards found that rhamnoxanthine had also attracted the attention of M. Casselmann in the same year, who published a note upon it in the *Journal de Pharmacie et de Chimie*.

The results of the observations of these chemists have been generally confirmed and completed by my own, and will be reported in the present paper, with my experiments. M. Buchner observed that some bark taken from the root of *R. frangula* had communicated to the paper in which it was enveloped a bright yellow tint, and that the inner side of the bark itself was covered with a quantity of small brilliant crystals of a golden yellow. M. Buchner then extracted the colouring matter from the root by exhausting its bark with ether, or by treating with ether the alcoholic extract obtained from the bark of the root. In these circumstances, however, the colouring matter retains obstinately a little grease, which it is difficult to separate entirely by repeated crystallisation from ether.

I obtained rhamnoxanthine from the bark of *R. frangula* and *R. catharticus* in a somewhat different

manner. The branches were plunged into sulphide or carbon, where I allowed them to remain for three or four days. The liquid, which at the end of that time had acquired a golden yellow colour, was evaporated to dryness at the ordinary temperature of the atmosphere, and the residue treated with alcohol, which dissolved the colouring matter and left behind a peculiar greasy substance of a brown colour. The alcoholic solution was evaporated to dryness, and the residue dissolved in ether, which, by spontaneous evaporation, left crystals of rhamnoxanthine. Thus obtained, rhamnoxanthine is insoluble in water and in diluted acids; it is soluble in ammonia and the alkalies, in alcohol, ether, and sulphide of carbon. Water precipitates it from these last three solvents; it can also be dissolved in fixed oils by means of heat, in benzin, and in essence of turpentine. With concentrated sulphuric acid it presents some curious phenomena, which I shall refer to presently. Nitric acid converts it into oxalic acid at a boiling heat, at the same time, according to M. Casselmann, a new acid is produced, to which he has given the name of nitrofrangulic acid, of the composition of which I shall speak further.

When heated quietly with a spirit-lamp rhamnoxanthine melts at about 250° C.; at the same temperature it sublimes, without decomposition, almost completely; but when heated in a tube, more or less is always carbonised, whilst the rest is converted into a vapour of an agreeable odour, which condenses itself in quadrangular microscopic lamellæ, or in oily drops, which crystallise on cooling. It can be thus obtained directly from the extract of the bark procured by sulphide of carbon, alcohol, or ether, in a sufficiently pure state. Upon analysis, rhamnoxanthine has given me the following results:—

	i.	ii.	Theory.
Carbon . . .	56.97	56.88	57.15
Hydrogen . . .	4.80	4.91	4.76
Oxygen . . .	—	—	38.09

100.00

whence we may deduce the formula, $C_{12}H_4O_6$, which corresponds also with the results obtained by M. Casselmann.

Ammonia dissolves rhamnoxanthine, giving a magnificent purple solution; potassa and soda have a similar action, but the colour they produce is not quite so delicate. With alkaline carbonates the solution obtained is of a reddish-brown. These combinations with the alkalies are destroyed by acids, and the primitive yellow substance is regenerated. By these reactions the colouring matter of which I speak resembles somewhat certain other yellow principles which have not been particularly studied. Moreover, I think I have detected the presence of rhamnoxanthine in several other plants, but have not yet completed my researches on this subject.

When concentrated sulphuric acid is poured upon rhamnoxanthine, this substance undergoes a very remarkable change. It immediately loses its golden yellow colour, and becomes of a bright emerald-green. I have observed the same striking phenomenon to take place with the yellow colour of leaves in autumn, and with the yellow colouring matter of the orange. If the action of concentrated sulphuric acid be allowed to continue, the fine green colour disappears, passing into a purple hue which dissolves in the acid. Water, and solutions of alkalies, appear to regenerate the primitive yellow. I have not examined particularly the combinations which take place with sulphuric acid, but the pro-

duction of the green emerald colour in these circumstances appears liable to useful applications.

By *eremacausis*, or fermentation, rhamnoxanthine is decomposed, and transformed into a brown colouring matter, which I have not examined. The same change occurs when it is heated a little in concentrated acids. This brown colour, which is insoluble, is likewise obtained when rhamnoxanthine is treated with a mixture of caustic soda and protochloride of tin, when it is precipitated as a laque: it can also be formed by putting rhamnoxanthine in contact with nascent hydrogen. It is obtained invariably of the same fine brown tint by means of protochloride of tin and caustic soda, the liquid which contains the mixture being heated nearly to ebullition. When treated with hot nitric acid, rhamnoxanthine is oxidated and converted into oxalic acid. At the same time, a new acid is formed, which has been examined by M. Casselmann, who has called it *nitro-frangulic acid*.

This product, submitted to analysis, gives:—

Carbon . . .	39.0
Hydrogen . . .	1.9
Nitrogen . . .	11.4
Oxygen . . .	47.7
	100.0

which corresponds nearly with the formula:—



This product is soluble in alcohol, from which it crystallises in yellow crystals, which easily detonate when heated; it is also soluble in boiling water and in alkalies, to which it gives a purple colour.

With oxides of magnesium, zinc, aluminium, tin, lead, &c., rhamnoxanthine forms laques, the tints of which may be varied to a great extent. Thus, if the branches of *R. frangula* be plunged into a weak solution of ammonia, the colouring-matter is dissolved, giving a red-purple liquid; if, then, the ammonia be saturated with citric acid and magnesia added, a beautiful violet-coloured laque is obtained. Again, on adding protochloride of tin to the yellow decoction obtained by macerating the branches in water, and precipitating with carbonate of ammonia, a brown-yellow laque is obtained, which sulphuric acid converts into a chocolate laque. Other brown, red, and yellow laques may be procured in a variety of manners.

When exposed to the damp, the wood of *R. frangula* and *R. catharticus* becomes gradually purple, which is probably owing to a formation of ammonia by putrefaction.

On attempting to dye stuffs with rhamnoxanthine, I found that this colouring-matter has a greater affinity for silk and wool than for cotton. But fine golden-yellow and purple dyes can be obtained by the use of mordants, and, in the hands of an experienced dyer, rhamnoxanthine may one day become a useful product.

In conclusion, I will state that this substance probably exists in other plants; it resembles very closely the chrysophanic acid obtained from the lichen *Parmelia parietina*, and which is supposed to exist in the root of rhubarb.

On Certain Reactions of Salts of Iron—Separation of Uranium and Iron, by M. F. PISANI.

UNTIL the present time the property of impeding certain reactions has been attributed almost exclusively to non-

volatile organic acids, such as tartaric acid; but it has lately been remarked that, in certain cases, oxalic acid possesses the same property. Thus, it is known by a recent experiment that its presence prevents the blue precipitation of salts of iron by ferrocyanide of potassium. The following are some analogous facts which I have observed:—

Salts of Iron.—If to salts of iron, as neutral as possible, acetic acid and an excess of oxalate of ammonia are added, the solution preserves its yellow colour, and does not become red, as is generally the case in the formation of acetate of iron. From this solution phosphate of soda does not precipitate phosphate of iron; but ammonia, as well as the ammoniacal sulphide, separate from it, as usual, all the iron. The liquid is not more coloured if, instead of acetic acid, acetate of soda is employed; but it then precipitates by phosphate of soda. To obtain these reactions, oxalate of ammonia must be added to the solution of iron, so that its colour changes from yellow to green-yellow.

Salts of Uranium.—In the presence of oxalate of ammonia nitrate of uranium does not give a red precipitate by ferrocyanide of potassium. In my paper on the estimation of uranium I stated that it could be precipitated by phosphate of soda in an acid liquid; but it must be remarked, that if oxalate of ammonia is first added to the solution, phosphate of soda gives no precipitate. If ammonia is added to it, all the uranium is separated in the state of phosphate; but if phosphate of soda has not been added, the precipitation by ammonia is incomplete. What remains can be precipitated only by the addition of phosphate of soda.

Salts of Aluminium.—When oxalate of ammonia is present in great excess, alumina is not immediately precipitated by ammonia and ammoniacal sulphide; but in a certain time, according to its proportion, alumina is precipitated, especially by the aid of heat. If we have to treat phosphate of alumina it is even possible to recognise phosphoric acid in the solution by a salt of magnesia; but this plan is not to be recommended, for it will probably precipitate as quickly as the alumina. Thus, for salts of aluminium oxalic acid fulfils but for a short time the purposes of tartaric acid.

Separation of Uranium and Iron.—When, as is generally the case, carbonate of ammonia is employed for this separation, it is well known that some iron is always dissolved with the uranium. The following is, however, a method for rendering the separation complete:—As oxide of uranium in solution in carbonate of ammonia does not precipitate by ammoniacal sulphide, we have only to add to the liquid, separated by filtration with oxide of iron, several drops of the latter re-agent to eliminate from it in the state of sulphide the small quantity of iron which has been dissolved. After filtering again, we get a liquid containing all the uranium without any trace of iron.—*Comptes-Rendus*.

On Certain Properties of the Platinum Metals, particularly Ruthenium, by Dr. C. CLAUS.

(Continued from page 195.)

THE platiniferous metals constitute a well-marked group of analogous metals, the characteristics of which are based on the following properties:—

1. These metals have nearly the same atomic number.
2. Their characteristic double salts, allied in composition, are isomorphous.
3. These metals, particularly when in a pulverulent condition, exert a great attraction for

oxygen. 4. Their oxides are reducible by hydrogen at the ordinary temperature—a phenomenon always accompanied by a heat sufficient to make them red hot.

Besides these common properties, these metals differ greatly from each other. The properties of some are allied to those of the so-called "noble metals," and particularly to gold. Others, while preserving some points of similitude to the noble metals, are still more allied to the electro-positive metals, iron and manganese.

These metals may be arranged in two superimposed series, the *superior horizontal* of which I designate the *principal series* (*haupt reihe*), because the metals which constitute it predominate in the various platinum ores. This series is characterised equally by an elevated atomic number and by almost the same specific gravity, and by the circumstance that its isolated members are particularly apt to form with chlorine the combination MCl_2 , which in its double salts with the chlorides of the alkaline metals maintains itself in the general type $MCl + MCl$, the crystalline form of which belongs to the tesseral system. The second *horizontal series*, which I call the *secondary series* (*neben reihe*), contains the remainder of the platiniferous metals, which also possess almost identical atomic and specific weights, but have in this respect but half the quantities of the principal series. These metals readily unite with chlorine according to the type MCl or M_2Cl_3 ; their double salts, with the alkaline metals, have the composition $M'Cl + MCl$, $2M'Cl + M_2Cl_3$, or $3M'Cl + M_2Cl_3$. Their crystalline form does not belong to the regular system, but to other systems with oblique axes.

If we divide the isolated members of these two series by vertical lines perpendicular to the horizontal line, we shall have a grouping of pairs of metals resembling each other, which, excepting the characteristics mentioned in the horizontal series, present in all other respects, decided analogies,—groups in which each pair consists of a principal metal (*haupt metall*) and a secondary metal (*neben metall*). In these pairs the resemblance is very evident, as I have already shown with respect to two pairs,—ruthenium and osmium, rhodium and iridium,—a resemblance which I shall make more apparent in the present article. The analogy of these metals, above all in their complicated combinations, extends so far, that not only are they alike in form, but their reactions are identical. Is not the newly-discovered volatile hyper-ruthenic acid another example in favour of this grouping? Palladium and platinum also constitute an exceedingly characteristic pair; in fact, they appear to be exactly alike in all their combinations, in their ammoniacal bases, in their cyanuretted combinations, &c.; and the fact of palladium in solution precipitated by cyanide of mercury is repeated by platinum. This fact is already known, but not the circumstances under which the precipitation occurs; these circumstances are the same as with palladium.

In fact, platinum, like palladium, ought to be found in the solution in the state of chloride; if, as has been observed, cyanide of mercury precipitates a platinum solution, treated by an organic substance, this is not the direct consequence of the presence of the organic substance, but rather because it determines the change from the state of platinic chloride to platinous chloride. Were it possible to prepare a solution of neutral chloride of palladium perfectly pure, and free from palladous chloride, it is very probable that cyanide of mercury would produce no precipitate from it. It has already been remarked that of all platiniferous metals, platinum itself is the one most closely allied to the precious metals,

particularly to gold; palladium bearing a less striking resemblance to it, is allied to it through platinum, to which it bears so great an analogy; and this leads me to call this pair of metals, "the pair at the negative end of the horizontal series." The analogy is still more decided at the positive end of this series, that is to say, between osmium and ruthenium, as between iron and manganese, which on their side resemble, in their mutual relations, those of the pairs of the platiniferous metals. The solutions of a composition like that of ferrate of potash and osmite of potash, are so much alike that they are undistinguishable by their colour.

(To be continued.)

TECHNICAL CHEMISTRY.

Notes on Benzoic Acid and some Benzoates,
by JOHN M. MAISCH.

Benzoic Acid.—When benzoïn is treated for benzoic acid by Mohr's process, it is necessary to gradually increase the heat, and if the process is continued until vapours cease to rise, the sublimate generally contains a variable quantity of empyreumatic matter, to which it owes its odour, and which likewise imparts colour to the product. The acid ought to be free of colour, and to obtain it thus a second sublimation is often found necessary. In order to avoid this, it is well to have two paper hoods ready made to fit the subliming vessel, and remove the first before it has been requisite to raise the temperature. At about 300° F. a quantity of benzoic acid may thus be obtained, which is perfectly white, of a high silky lustre, possessing but little of the empyreumatic odour, and, when dissolved in an alkali, affording a colourless solution; its appearance and chemical behaviour prove it to be far superior to the article usually met with in commerce.

If the sublimation is now continued, the acid obtained subsequently is found to contain empyreumatic colouring matter, as the process proceeds; it may be freed from it by a second sublimation. This increase of empyreumatic products in the sublimed acid is not owing merely to an increase of the temperature, but principally to the fact that the bibulous paper stretched over the vessel has become saturated with the same, and is unable to absorb and retain any more at the temperature to which it is exposed. If, during the process, the paper is replaced by a fresh piece, the acid sublimes as white as before, and with as little odour adhering to it. This empyreumatic aroma is frequently considered essential for the medicinal value of the acid, and such careful treatment of the balsam would therefore be inadmissible when the acid is to be used in medicine. But I believe that the product which has been obtained by a slow sublimation at a low temperature, will, by its beauty and purity, fully repay for the little trouble incidental with its collection.

Benzoate of Soda.—When benzoic acid is almost neutralised with carbonate of soda, a colourless solution results, which, when sufficiently concentrated, yields, on cooling, small, thin needles, in star-like groups; if somewhat further evaporated, the solution assumes a brownish colour, and may remain liquid for a day or two without separating any crystals. If still further evaporated, the crystals form very slowly in the syrupy liquid. They are very long, needle-shaped, and commence to become opaque while still in contact with the liquid,

together with which they dry to an opaque mass without crystalline structure. When removed from the solution and rapidly pressed between bibulous paper, they preserve only in some measure their original shape. The brown, syrupy liquid, as obtained by evaporation, dries, to the last drop, to a white, opaque mass. Benzoate of soda is best obtained in crystals, by evaporating its solution only so far that on cooling it will just commence to crystallise, and to expose it gradually to a lower temperature. The crystals thus formed by cooling to 32° are to be removed from the thin liquid, and rapidly dried by pressing them between folds of bibulous paper.

Benzoate of Iron.—According to Trommsdorff and Berzelius, the ter-benzoate of iron is formed by dissolving recently-precipitated sesquioxide of iron in an aqueous solution of benzoic acid. The yellow needles are decomposed by water and alcohol, leaving a basic salt behind.

When a concentrated solution of benzoate of soda is added to the officinal solution of ter-nitrate of iron, a voluminous, orange-coloured precipitate is obtained, which is immediately thrown on a filter; when the liquid has nearly all passed, a small portion of water is added to displace most of the ferric solution, and the precipitate is then allowed to dry, spread out upon a brick tile. This was used for analysis.

Five grs. were heated in a porcelain crucible; a little water was first given off, then benzoic acid sublimed in small, white crystals. On increasing the heat, a brown liquid collected on the cover, which congealed into brown-coloured scales, having the empyreumatic odour and taste of benzoic acid. The residue was repeatedly moistened with nitric acid, and exposed to heat until vapours ceased to be given off; it then weighed 0.85 grs., which is equal to 17 per cent.

Ten grs. of the benzoate of iron was repeatedly treated with a large excess of ammonia, the sesquioxide of iron was well washed upon a filter, then washed into a tared capsule, and heated until it ceased to lose weight; its weight was 1.6 grs. = 16 per cent.

Into a glass tube, weighing 48.1 grs., 3.08 grs. benzoate of iron were introduced, and exposed to the heat of a water-bath for half-an-hour. The loss was only about 0.02. The tube was now introduced into a sand-bath of 260° , and kept at this temperature until aqueous vapours ceased to be condensed in the upper part, it now weighed 50.85 grs. The loss, therefore, amounted to 51.18—50.85, = 0.33 grs., or 10.71 per cent.

From these data, the following formula is calculated for this ter-benzoate of iron:—

Fe_2O_3	80	16.91	16.50
$3\text{C}_{14}\text{H}_5\text{O}_3$	339	71.67	71.44 (see below)
6HO	54	11.42	10.71

$\text{Fe}_2\text{O}_3, 3\text{C}_{14}\text{H}_5\text{O}_3 + 6\text{HO}$ 473 100.00

Ter-benzoate of iron, as thus obtained, is in a light powder, of a beautiful orange colour, permanent in the air, turning brown between 270° and 280° ; it fuses and yields when carefully heated at first pure benzoic acid, afterwards an empyreumatic liquid, of an acid reaction, which appears to be principally the same acid, and congeals in scales. Water and alcohol dissolve a portion of it, probably an acid salt, and leave a basic salt behind.

Benzoic acid is scarcely used in medicine, except in pargoric, and occasionally in ointments; when taken internally, it is stated to act as a stimulant. Combined with iron, as in the above salt, it might perhaps prove useful in the hands of the physician. The salt has very little taste, not at all ferruginous, and would certainly

be preferable to the acid or the alkaline salts, which possess a marked acrimony.

When the above ter-benzoate is desired, the directions above given for its preparation should be closely followed; the filtrate from the precipitate separates in the course of two days, another precipitate, which dries upon the filter in a yellowish powder, and in orange-brown scales, evidently a mixture of different compounds; and, after three weeks, more of the light powder is deposited. The drying is to be accomplished in thin layers upon a brick tile.

Bibenzoate of Ammonia.—In order to determine as near as possible the quantity of benzoic acid in the iron salt, the above filtrate from the treatment of the ferric benzoate with ammonia was evaporated, and kept at a temperature of below 190° F., until a dry mass remained behind and the loss of weight ceased. The residue weighed 8.25 grs.; it evolved ammonia when treated with an excess of caustic potassa; and Wackenroder's observation, which has been doubted already by Liebig, that on the spontaneous evaporation of a solution of benzoic acid in carbonate of ammonia, pure benzoic acid is separated in crystals, is thereby proven to be incorrect. Assuming the experiments of Berzelius, that a bibenzoate of ammonia is thus formed, to be correct, the above 71.67 per cent., or 7.167 grs. of $\text{C}_{14}\text{H}_5\text{O}_3$, would require of NH_4O 0.824 grs., and of HO 0.285 grs., making the required whole weight of the salt $7.167 + 0.824 + 0.285 = 8.276$, with which the actual weight of 8.25 closely agreed. Accordingly, the formula for anhydrous bibenzoate of ammonia is calculated as follows:—

NH_4O	26	9.96
HO	9	3.45
$2\text{C}_{14}\text{H}_5\text{O}_3$	226	86.59

$\text{NH}_4\text{O}, \text{HO}, 2\text{C}_{14}\text{H}_5\text{O}_3$ 261 100.00

and 8.25 grs. of bibenzoate of ammonia as found above contain $\frac{8.25 \times 100}{100} = 7.143675$ grs. $\text{C}_{14}\text{H}_5\text{O}_3$ 71.437 per cent. of the above benzoate of iron.

This experiment verifies, on the one hand, the above formula for the neutral benzoate of iron, and tends to prove, on the other hand, that an anhydrous bibenzoate of ammonia remains behind on evaporating a solution of benzoic acid in ammonia, at a temperature below 190° . The influence of a higher heat had not been tried.—*American Journal of Pharmacy.*

Contributions to the History of Aniline: Production of a New Blue Dye-stuff (Paris Blue), by MM. PERSOZ, V. DE LUYNES, and SALVÉTAT.

M. HOFMANN,¹ while studying the action of bichloride of carbon on aniline, was led to the discovery of a new base, to which he assigned the formula $\text{C}_{10}\text{H}_7\text{N}_3$. To purify and separate it from the matters mixed with it, it suffices to wash it in cold alcohol, and crystallise it once or twice in boiling alcohol. Thus treated, the base crystallises, the alcohol retaining in solution a substance of a magnificent crimson colour.

The researches we have so long pursued on the colours derived from aniline, have led us to study the possible relations existing between the crimson substance discovered by M. Hofmann and the red dye-stuff discovered by MM. Renard and Franc of Lyons, to which the name of fuchsic acid has been given. We find that there is no analogy between these two substances; in fact, the Lyons red substance is completely soluble in alkalies,

¹ *Comptes-Rendus*, September 20, 1858, Vol. xlvii. p. 492.

in connection with which it appears in the light of a true acid; it enters into combination with ammonia, potash, baryta, &c., and forms soluble combinations with these bases. These solutions, treated with acetic acid, serve to dye the purest shades. By grounding our process on the solubility of Lyons-red in alkalies, we have been enabled to extract this colouring matter from all the products employed in commerce under such various names.

By strictly attending to the rules laid down by M. Hofmann, we have obtained, like this learned chemist, a viscous oil, gradually solidifying with a crystalline structure, and we have proved that the alcohol which had served to purify the base he investigated, remained crimsoned, the tint being more or less pure according to the circumstances under which the operation is conducted.

We have also ascertained that the product, insoluble in water, formed during the reaction, dissolves in hydrochloric acid, and that the hydrochloric solution yields with potash a dirty red precipitate, soluble in alcohol, to which it imparts a rich crimson colour. In M. Hofmann's opinion this colouring matter is of the same nature as that just mentioned; but we have been led to think that it is not so, and that it is formed by the mingling of two principles, one blue the other red.

This crimson substance, resisting the action of boiling alkalies, cannot be compared to fuchsine acid, and if, in M. Hofmann's experiment, this acid could be produced, it would only be found in alkaline solutions, in which it exists but in infinitesimal quantities; further, certain circumstances relating to bulk, temperature, or time are required to allow of the development or the preservation of this dye-stuff.

In fact, by heating for thirty hours, the same mixture which yielded the very decided crimson tints, either in the soluble portion or the residue, we have seen that this coloration does not extend to the products obtained at the temperature of 180°. There is nothing surprising in this result, since we have proved that a mixture of 3 parts of solid fuchsine and 10 parts bichloride of carbon, heated under the before-mentioned conditions, yields only clear yellow liquids; all red matter disappears.

Moreover, by moderating the temperature, the duration of the experiment, and the respective proportions of aniline and bichloride of carbon, we have produced substances decidedly richer in colouring principle than those obtained by M. Hofmann. That the fuchsine exists is unquestionable, but the point is to seize upon it at the moment of its production. It is, moreover, accompanied by Hofmann's red substance, which predominates, and is distinguished from it by its insolubility in potash.

We have naturally been led by these observations to know what should result on mixing, according to Hofmann's plan, anhydrous bichloride of tin and aniline, which forms Lyons red.

Nine grammes of bichloride of tin and 16 grammes of aniline, heated for 30 hours in a sealed tube, at about 180°, yields, instead of red or violet, a very bright and pure blue, which requires only to be treated with water to dye animal fibres in beautiful brilliant tints. This blue, which resists acids, is deepened by feeble alkalies, and assumes a gooseberry-purple with concentrated alkalies. As its tint and purity are not influenced by artificial light, manufacturers will not fail to take advantage of it. We give it the name of "Paris blue," and it is another addition to the very remarkable series of rich colours derived from aniline.—*Comptes-Rendus*, Vol. lii.

PHARMACY, TOXICOLOGY, &c.

Poisoning by Cyanide of Potassium,

by E. R. SOUTHEY.

THE following is the history of a case as given me by Mr. P. Belcher, the medical man called in on March 23, 1861:—

"Ann W., aged 17, a domestic servant in a photographer's household, had been dead when seen by me about half-an-hour. Body getting cold, very livid, rigor mortis slightly perceptible in the legs; a slight smell of brandy from the mouth; tongue ecchymosed at the tip. The following account was given by eye-witnesses of the attack:—

"Deceased had been slightly indisposed last evening, refused supper, and went to bed; came down in the morning apparently well, took her breakfast, but refused dinner; did her household work as usual till four o'clock in the afternoon, was then sent by her mistress into the portrait-room (immediately adjoining the dark room in which the chemicals were kept) to light the fire; returned in a few minutes, and busied herself in getting the tea ready. Her mistress' attention was now attracted by her extraordinary attitude. She stood with a fixed stare, face twitching, and her hands hanging down from the wrists as if paralysed. Her mistress ran to her, caught her in her arms in the act of falling, and put her in an arm-chair. Now followed a severe attack of general convulsions, with froth from the mouth and nose, supposed by her and other witnesses to be an epileptic seizure. Some brandy was administered, of which she swallowed a little with difficulty, and appeared for some time to be rallying; the convulsions, however, returned; she suddenly stiffened her legs, gave a few gasps at long intervals, and died. The above attack seems to have lasted about twenty minutes, during all which time she neither spoke nor screamed. It appears, also, that the period from the time of her taking the poison—viz., when she was sent to the portrait-room till she was first seized—could not have exceeded ten minutes; so that the whole period from taking the poison till death ensued was between twenty-five and thirty minutes. Some additional evidence was elicited at the inquest, viz.—that she had had a quarrel with her lover the evening before, and that one of the lumps of cyanide in his stock bottle was observed by her master to have had a portion recently broken off."

Post mortem Examination, by Messrs. P. and S. Belcher.

"March 25.—Forty-one hours after death.—Rigor mortis continued. Integuments uncommonly fatty. Head: brain and membranes healthy; venous congestion. Thorax: Lungs healthy; heart enlarged; right side dilated, the walls thin, and those of the auricle translucent, except where crossed by the muscliculipectinati; valves healthy. Left side of the heart healthy. Abdomen: stomach distended with gas; contained half-digested food, having only the usual smell. When removed from the body and opened, the mucous membrane was found to be in a state of intense inflammation, being generally very red, and easily peeled off with the nail; and in two places on the cardiac portion patches of a dusky hue, about four inches in circumference, were seen. Other viscera healthy; pelvic viscera, uterus and ovaries, healthy and unimpregnated."

Jamination of the Contents of the Stomach.

The contents of the stomach, amounting to about four ounces, consisted of a grayish, viscid fluid, with portions of digested food floating in it. Reaction slightly alkaline.

About two ounces were acidulated with sulphuric acid and distilled in a glass retort till about half had passed over. During the distillation the smell of prussic acid was perceptible, but a good deal masked by the stench.

About one drachm of the distillate was boiled with a little sulphide of ammonium till the smell of the latter had disappeared. On addition of a few drops of sesquichloride of iron, the deep red colour of the sesquisulphocyanide of iron appeared, this colour disappearing completely on addition of a solution of bichloride of mercury.

Another drachm of the distillate was rendered slightly alkaline by solution of potassa, a small crystal of protosulphate of iron added, and the whole boiled. A drop of sesquichloride of iron solution was then added, and the mixture acidulated with hydrochloric acid. A blue colour was produced, disappearing on addition of solution of potassa in excess, and re-appearing on acidulating with hydrochloric acid. On standing for twelve hours, the Prussian blue was deposited.

The residue left in the retort was evaporated to dryness, ignited till a greater portion of the carbon was driven off, and digested with hot distilled water, and the solution filtered. On addition of hydrochloric acid, alcohol, and bichloride of platinum, a yellow, crystalline precipitate, seen under the microscope to consist of the octahedra of the double chloride of platinum and potassium.

I therefore concluded that the poison taken was cyanide of potassium.

The above analysis was made about fifty-two hours after death. No smell of prussic acid was perceived, either immediately after death or during the *post mortem*.

PHYSICAL SCIENCE.

Early Researches on the Spectra of Artificial Light from Different Sources.

*Some Experiments on Coloured Flames,*¹ by H. F. TALBOT, Esq.

GREAT progress has recently been made in investigating the properties of light, and yet many of them are still unexamined or imperfectly explained. Among these are the colours of flames, which not only appear very various to common observation, but are shown by the assistance of a prism to be entirely different in nature one from another; some being *homogeneous*, or only containing one kind of light, others consisting of an infinite variety of all possible shades of colour.

1. It was discovered by Dr. Brewster that the flame of alcohol, diluted with water, consists chiefly of homogeneous yellow rays. On this principle he proposed the construction of a monochromatic lamp, and pointed out its advantages for observations with the microscope. This must be considered a very valuable discovery. The light of such a lamp, however, is weak unless the alcohol flame is very large. I have, therefore, made several attempts to obtain a brighter light, and I think the following is the most convenient method. A cotton

wick is soaked in a solution of salt, and when dried placed in a spirit-lamp. It gives an abundance of yellow light for a long time. A lamp with ten of these wicks gave a light little inferior to a wax candle; its effects upon all surrounding objects was very remarkable, especially upon such as were red, which became of different shades of brown and dull yellow. A scarlet poppy was changed to yellow, and the beautiful red colour of the *lobelia fulgens* appeared entirely black. The wicks were arranged in a line, in order to unite their effect for a microscope. A common blue glass has the property of absorbing the yellow light of this lamp, however brilliant, while it transmits the feeble violet rays. If these are also stopped by a pale yellow glass the lamp becomes absolutely invisible, though a candle is seen distinctly through the same glasses. But the most remarkable quality of this light is its homogeneity, which is perfect as far as I have been able to ascertain; I speak of the yellow rays, which form the mass of the light, and quite overpower the feeble effects of the blue and green. The origin of this homogeneous light appears to me difficult to explain. I have found that the same effect takes place whether the wick of the lamp is steeped in the muriate, sulphate, or carbonate of soda, while the nitrate, chlorate, sulphate, and carbonate of potash agree in giving a bluish-white tinge to the flame. Hence the yellow rays may indicate the presence of soda, but they nevertheless frequently appear where no soda can be supposed to be present.

2. Mr. Herschel discovered that sulphur when burning intensely gives a homogeneous yellow light. To examine it, I inflame a mixture of sulphur and nitre behind a screen having a narrow vertical slit through which the flame could be seen. This opening, examined with a prism, gave a spectrum in which there was a very bright yellow line, indicating the combustion of the sulphur. I thought it a point of considerable interest to determine whether this yellow ray was identical with that afforded by the flame of alcohol containing salt, and with that view I placed such a flame behind the other, their light passing through the same opening, so that if the rays were of a different nature two yellow lines should be seen in the spectrum; but if identical, then only one. I found upon trial that the rays coincided, and I obtained a further confirmation of this by inflaming the nitre and sulphur mixed up with a quantity of salt, the effect of which was not to produce a second yellow line in the spectrum, but to increase greatly the brilliancy of the original one. The result of this experiment points out a very singular optical analogy between soda and sulphur, bodies hitherto supposed by chemists to have nothing in common.

3. There are other means of procuring the same light, which I shall briefly mention. If a clean piece of platina foil is held in the blue or lower part of a gas-flame it produces no change in the flame; but if the platina has been touched by the hand it gives off a yellow light, which lasts a minute or more. If it has been slightly rubbed with soap, the light is much more abundant, while wax, on the contrary, produces none. Salt sprinkled on the platina gives yellow light while it decrepitates, and the effect may be renewed at pleasure by wetting it. This circumstance led me to suppose that the yellow light was owing to the water of crystallisation rather than to the soda; but then it is not easy to explain why the salts of potash, &c., should not produce it likewise. Wood, ivory, paper, &c., when placed in the gas-flame, give off, besides their bright flame, more or less of this yellow light, which I have always found

¹ From Brewster's "Journal of Science," vol. v. 1846.

the same in its characters. The only principle which these various bodies have in common with the salts of soda is *water*; yet I think that the formation or presence of water cannot be the origin of this yellow light, because ignited sulphur produces *the very same*,—a substance with which water is supposed to have no analogy.¹ It is also remarkable that alcohol burnt in an open vessel, or in a lamp with a metallic wick, gives but little of the yellow light; while if the wick be of cotton, it gives a considerable quantity, and that for an unlimited time. (I have found other instances of a change of colour in flames owing to the mere presence of a substance which suffers no diminution in consequence. Thus, a particle of muriate of lime on the wick of a spirit-lamp will produce a quantity of red and green rays for a whole evening without being itself sensibly diminished.) The bright flame of a candle is surrounded by the same homogeneous yellow light which becomes visible when the flame itself is screened. The following experiment shows its nature more evidently:—If some oil be dropped on the wick of a spirit-lamp, the flame assumes the brilliancy of a candle, surrounded by an exterior yellow flame. This appearance only lasts until the oil is consumed.

4. The flame of sulphur and nitre contains a red ray, which appears to me of a remarkable nature. While examining the yellow line in the spectrum of this flame, I perceived another line, situated beyond the red end of the spectrum, from the termination of which it is separated by a wide interval of darkness. In colour it nevertheless differs but little from the rays which usually terminate the spectrum. It arises, I believe, from the combustion of the nitre, as the yellow ray does from that of the sulphur; for I have since observed it in the flame of a spirit-lamp whose wick had been soaked in nitre or chlorate of potash. It appeared that this ray was so distant from the rest that it might be less refrangible than any in solar light; and I have been since informed by Mr. Herschel that he had already observed it in a similar experiment, and was impressed with the same idea.

With the hope of establishing this, I admitted candle-light and that of the nitre lamp, which I have just mentioned, through the same aperture, and noticed how far this isolated red ray appeared beyond the spectrum of the candle. I then compared, in the same way, the light of the candle with that of the sun, and I found that the great intensity of the solar light lengthened the red end of the spectrum about as far; so that I was obliged to leave the question undecided, as the faintness of the lamp prevented my comparing it directly with the sun. This red ray appears to possess a definite refrangibility, and to be characteristic of the salts of potash, as the yellow ray is of the salts of soda, although, from its feeble illuminating power, it is only to be detected with a prism. If this should be admitted, I would further suggest that whenever the prism shows a *homogeneous* ray of any colour to exist in a flame, this ray indicates the formation or the presence of a *definite chemical compound*. An excellent prism is, however, requisite to determine the perfect homogeneity of a ray.

5. Phosphorus inflamed with nitre gives a very brilliant spectrum, in which no colour appears to be predominant or deficient. It, therefore, resembles the spectra of ignited lime, platina, and other solid bodies, and differs totally from the solar spectrum, in which

there are now known to be innumerable interruptions of light. And it is worthy of remark that no light has been hitherto discovered at all resembling that of the sun (when analysed with a prism), except the light of the other celestial bodies.

6. The red fire of the theatres, examined in the same way, gave a most beautiful spectrum, with many white lines or maxima of light. In the red these lines were numerous and crowded with dark spaces between, besides an exterior ray, greatly separated from the rest, an probably the effect of the nitre in the composition. In the orange was one bright line, one in the yellow, three in the green, a very bright one in the blue, and several that were fainter. The bright line in the yellow is caused, without doubt, by the combustion of the sulphur, and the others may be attributed to the antimony, strontia, &c., which enter into this composition. For instance, the orange ray may be the effect of the strontia, since Mr. Herschel found in the flame of muriate of strontia a ray of that colour.² If this opinion should be correct, and applicable to the other definite rays, a glance at the prismatic spectrum of a flame may show it to contain substances which it would otherwise require a laborious chemical analysis to detect.

London, March, 1826.

*On a Method of Obtaining Homogeneous Light of Great Intensity,*³ by H. F. TALBOT, Esq.

As it is a desideratum in optical science to procure perfectly homogeneous light of sufficient brightness for many important experiments, I am glad to be able to communicate a method which, in a satisfactory manner, supplies that deficiency. It is only requisite to place a lump of common salt upon the wick of a spirit-lamp and to direct a stream of oxygen gas from a blowpipe upon the salt. The light emitted is quite homogeneous and of dazzling brightness. If, instead of common salt, we use the various salts of strontium, barytes, &c., we obtain the well-known coloured flames which are characteristic of those substances with far more brilliancy than by any other method with which I am acquainted.

*On the Flame of Lithia,*⁴ by H. F. TALBOT, Esq.

LITHIA and strontia are two bodies characterised by the fine red tint which they communicate to flame. The former of these is very rare, and I was indebted to my friend Mr. Faraday for the specimen which I subjected to prismatic analysis. Now, it is difficult to distinguish the lithia red from the strontia red by the unassisted eye; but the prism displays between them the most marked distinction that can be imagined. The strontia flame exhibits a great number of red rays, well separated from each other by dark intervals, not to mention an orange, and a very definite bright blue ray. The lithia exhibits one single red ray. Hence, I hesitate not to say that at optical analysis can distinguish the minutest portions of these two substances from each other with as much certainty, if not more, than any other known method.

¹ Edinburgh Transactions, vol. ix. p. 456.

² London and Edinburgh Philosophical Magazine and Journal of Science, Third Series, 1833, vol. III. p. 35.

³ London and Edinburgh Philosophical Magazine and Journal of Science, Third Series, 1834, vol. IV. p. 114.

⁴ It may be worth remark, though probably accidental, that the specific gravity of sulphur is 1.99, or almost exactly twice that of water.

On the Flame of Cyanogen.

For the opportunity of examining the optical characters of this flame I am likewise obliged to the kindness of Mr. Faraday, who showed it both to Sir J. Herschel and myself at the Royal Institution. When viewed with a prism, this flame presents a very distinct and peculiar character, separating the violet end of the spectrum into three portions, with broad, dark intervals between. But the most remarkable fact is this, that the last of these portions is so widely separated from the others as to induce a suspicion that it may be more refracted than any rays in the solar spectrum,—a question which I should be glad to have the opportunity of deciding by direct experiment. This separate portion has a pale, undecided hue. I should hardly have called it violet, were it not situated at the violet end of the spectrum. To my eye it had a somewhat whitish or grayish appearance.

*On Prismatic Spectra,*¹ by H. F. TALBOT, Esq.

It is much to be desired that an extensive course of experiments should be made on the spectra of chemical flames, accompanied with accurate measurements of the relative position of the bright and dark lines, or *maxima* and *minima* of light which are generally seen in them. The definite rays emitted by certain substances, as for example, the yellow rays of the salts of soda, possess a fixed and invariable character, which is analogous in some measure to the fixed proportion in which all bodies combine, according to the atomic theory. It may be expected, therefore, that optical researches, carefully conducted, may throw some additional light upon chemistry. Some experiments which I formerly made upon this subject will be found in "Brewster's Journal," for 1826, vol. v. p. 77. In addition to the substances there enumerated, as giving a peculiar optical character to flame, I have found that the salts of copper are exceedingly remarkable. They give spectra so covered throughout with dark lines as to resemble in that respect the solar spectrum. The flames of boracic acid and nitrate of barytes also possess somewhat of a similar character. The most convenient way of obtaining brilliant spectra of these substances is to deflagrate them with chlorate of potash; but this is attended with the inconvenience that the spectrum produced by the chlorate is seen in conjunction with the other, and an allowance must be made accordingly. Another good method is to sprinkle the substance in powder on the wick of a spirit-lamp, and direct a current of oxygen upon it. With regard to the accurate measurement of the lines, it requires the use of very superior apparatus. I have sometimes made approximate measurements by fixing a divided scale transversely to the linear aperture through which the light of the burning body was observed. This aperture was then expanded by the prism into a spectrum parallel to the scale, by means of which it could then be measured. An objection may, perhaps, occur to the reader, that the scale would thus be as much refracted as the light itself, and therefore could not serve to measure it; but this difficulty was avoided by a simple contrivance, viz., by illuminating the scale with homogeneous light.

Spectra of Various Galvanic Flames.—Silver leaf, deflagrated by galvanism, gave a spectrum with several

definite rays, among which two green rays appeared to me to possess nearly the same tint, although differing in refrangibility. Gold leaf and copper leaf each afforded a fine spectrum, exhibiting peculiar definite rays. The effect of zinc was still more interesting; I observed in this instance a strong red ray, and three blue rays, besides several more of other colours. These experiments were made in the laboratory of the Royal Institution, in June, 1834.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting.

On the Nature of the Deep-sea Bed, and the Presence of Animal Life at vast Depths in the Ocean, by Dr. G. C. WALLICH.

OUR first clear glance at the floor of the ocean may be said to date from the period at which submarine telegraphy was first undertaken. For although the depth of the sea has been approximately ascertained over widely extended areas, in the course of the various surveys conducted under the auspices of the British, the United States, and the Dutch Governments, hardly any previous attempts have been made systematically to investigate the characters and composition of its bed. In the absence of any special object, such attempts would have been far too costly and difficult to be practicable. It has been ascertained, however, that the floor of the ocean is but the reflex, as it were, of the dry land; that it is in no place unfathomable; that along its deeper portions certain muddy deposits are to be met with, in many cases, made up more or less entirely of minute calcareous shells belonging to one of the most simple order of beings with which we are acquainted; and that together with these are also to be found, but in, comparatively speaking, small quantity, the minute flinty skeletons of other organisms derived both from the animal and vegetable kingdoms. But no conclusive evidence has been produced to show whether any or all of these organisms normally lived and perished, at the profound depths from whence they were obtained by the sounding lead; or whether, having inhabited distant, and perhaps shallower seas, their dead remains alone, after being transported by currents, or other agencies, had gradually subsided into the deep hollows of the ocean. Taking into consideration the very important part played by these organisms in the structure of the earth's crust, that vast strata have in ages gone by been built up of them, and that similar strata are at the present time being deposited along the beds of existing seas, the investigation of these questions becomes of the highest consequence, as bearing on the successful establishment of ocean telegraphy.

The distribution of animal life in the upper waters, of the sea is determined by climate, by the composition of its waters, the nature of its bed, and its depth in any given locality; the last of these items necessarily involving the relative degrees of temperature, light, aëration, and pressure, as compared with those to be met with near the surface. Of these conditions climate exercises a very powerful influence; for it is found as we advance from the equator towards the poles that a gradual diminution takes place, not only in the number of types met with, but of the varieties ranged under those types. It has been maintained, that in order to compensate for the diminution in the number of generic forms, the number of individuals of each species is much augmented. Although this law holds good as regards the higher orders, it can hardly be said to do so in the case of the lower; for the vast assemblages of these lower forms met with on the surface of the sea in the tropics, are in no wise less extensive than those met with

¹ London and Edinburgh Philosophical Magazine, and Journal of Science, Third Series, 1836, vol. ix. p. 3.

in high latitudes. It will be found that the lower the grade of being, the more equally balanced will be its distribution, at the extremes of the globe; inasmuch as the greater range in depth commanded by these lower forms renders them less amenable to conditions which are variable from being dependent on atmospheric changes.

The composition of the waters of the ocean is well known to become much more equable at great depths; and it therefore, exercises a far less marked influence on the presence of animal life than it does at the surface. The same causes which equalise the temperature in so remarkable a manner as the depth increases, are effective in equalizing the relative proportions of the various ingredients that enter into the composition of sea water, in all latitudes. For, whilst the surface stratum is subject to dilution with fresh water, from various sources, the greater the depths, the less subject can the waters be to this influence, and the less can it operate in modifying the distribution of the organisms that frequent them.

Oxygen is essential to the presence of animal life—with-out it animal life ceases. To air-breathing, as well as water-breathing creatures a due supply of this gas is indispensable; the function of respiration, no matter whether performed by lungs, as in man and the higher orders, or by a simple process of absorption and exudation through the general surface of the body, as in some of the lower forms, being, in every instance, essentially that process whereby oxygen is received into the system in exchange for carbonic acid which is given off. But although oxygen enters largely into the composition of both atmospheric air and water, the supply of this element is not obtained, in the case of creatures inhabiting the sea, under ordinary circumstances, from its decomposition, but from a certain portion of atmospheric air present in water in a state of solution. Most gases are absorbed by water. Under pressure, the quantity absorbed is much increased, as is seen in the familiar case of soda-water. It should be borne in mind, however, when the fact is applied to the occurrence of animal life at great depths in the sea, that in order to produce the absorption of atmospheric air, its contact or mixing together at the surface by the action of wind and wave is necessary, and the effect of this operation can only extend to a limited depth, unless, as has been assumed by some of our highest authorities, the lower strata of sea water, being subject to increased pressure, become capable of holding in solution a greater quantity of oxygen; and, by robbing the superincumbent strata of that which they contain, gradually become saturated with it. Should this view be correct, there must be a point at which the maximum amount of oxygen, which sea water can absorb, is permanently present in it. But, inasmuch as the vegetable cell, simple though it be in structure, can eliminate carbon from the medium in which it lives, it is not unreasonable to assume that the lowest forms of animal life, even where no specialized organs are traceable, may, in like manner, be able to eliminate oxygen directly from the water around them.

The temperature of the sea is materially influenced by the climatic conditions of different latitudes; and, of course, exercises a powerful effect both on the distribution and abundance of the higher orders of living beings present in its waters. But, as has been shown, this influence is not manifest, or at all events, not so manifest in the lower orders, for at great depths the variability of the temperature is reduced within very narrow limits in all latitudes. Now the higher orders of oceanic creatures inhabit only the surface waters, never sinking down to extreme depths. In the case of some of the lower forms, on the other hand, a very extended bathymetrical range exists, putting out of the question those which constantly dwell on the sea bed itself, of which I shall presently have to speak.

In like manner, Light, or rather the absence of it, can hardly be said to determine, in any important degree, the distribution and limitation of the lower forms of animal

life. Light is not essential even in the case of some of the higher orders. A large class of creatures, both terrestrial and marine, possess no true organs of vision, although there is good reason for believing that they do possess some special sensory apparatus, susceptible to the influence of light; whilst certain creatures, whose habitation is in subterranean caves or lakes, as in the Magdalena caves, near Adelsberg, and the Great Mammoth caves in Kentucky, either possess no organs of vision, or possess them in so rudimentary a state, as to prove clearly that the absence or imperfect development of this sense may be compensated for by the higher development of other senses.

It is impossible at present to say to what depth light penetrates in the sea. The photographic art will, no doubt one day solve the problem. But it is almost certain that a limit is attained, and that moreover long before the deep recesses gauged by the sounding machine are reached, where the light-giving portion of the ray cannot penetrate even in its most attenuated condition; and yet, as shall hereafter be shown, creatures have been found down in those profound and dark abysses, whose colouring is as delicate and varied as if they had passed their existence under the bright influence of a summer sun!

Pressure is the last condition which has to be noticed. Although undoubtedly a highly important one, I hope to be able to prove that it is not of essential value as has heretofore been laid down, in determining the final limit of animal life in the sea.

It is almost needless to state that at the sea level there exists a pressure of 15 lbs. on every square inch of surface, due to the weight of the atmospheric column resting upon it; and that the pressure on the successive strata of water in the sea, as the depth increases, is infinitely in excess of this, inasmuch as a column of water only 33 feet in height is capable of counterbalancing the entire atmospheric column, which extends to a height of about 45 miles. Accordingly, for every 33 feet of descent in the sea, putting out of consideration the effect of the superincumbent column in actually diminishing the bulk of the portions beneath by augmenting their density, there is an additional 15 lbs. At great depths, therefore, the aggregate pressure becomes stupendous. As is well known, pieces of light wood let down to a depth 1500 or 2000 fathoms, become so compressed and surcharged with water as to be too heavy to float. But there is a fallacy in this experiment; for the contraction of the woody fibres and cells is a necessary consequence of their submission to an amount of pressure so enormously in excess of that under which they originated. With organisms which have been developed, from first to last, under the full operation of any given amount of pressure, the result would not be of this nature; for the equalization of the pressure within and without their entire structure, although it might possibly exercise some definite effect in determining their shape, size, or even functions, cannot, I submit, operate in causing the creatures living under it to experience any more detrimental results than we experience from the 15 lbs. on every square inch, or about 14 tons, on the general surface of our bodies near the sea level.

It can scarcely be wondered at that under such apparently extraordinary conditions, the maintenance of life, even in its least developed aspects, should have been deemed absolutely impossible at extreme depths; and that it should have been almost unanimously recognised as an axiom, that at a depth of 400, or, at most, 500 fathoms, life, whether animal or vegetable, must be extinct. The fact is unquestionable that as we descend beyond the first hundred fathoms, the traces of life become more and more remote; and it is probably owing to this gradual diminution in the number of animal forms, as the depth exceeds this limit that it has been assumed, rather as a matter of theory than of observation, that a point is speedily reached at which all the conditions essential to life are extinguished.

This view has also derived support from the idea that "animal life depends on the previous existence of vegetable life." In the case of the higher orders of the animal kingdom, the law, no doubt, holds good. Not so, however, in the case of the lower. The conditions essential to the perpetuation of the one are not essential to the perpetuation of the other. Thus, light is indispensable for the healthy respiration and growth of the vegetable. The animal can, on the other hand, respire as freely in the blackest darkness as in the broad glare of day. And this is, no doubt, the reason why vegetable life in the ocean attains its final limit in depth so much sooner than animal life. And yet, considering how very unexpectedly animal life has been proved to exist deep down in the ocean—as I shall immediately show far removed beyond those conditions which had hitherto been considered indispensable—we ought perhaps to pause before we assert that the same plastic skill which has so constituted certain creatures as to admit of their inhabiting the deep abysses of the ocean, may not, in like manner, have so constituted some of the vegetable organisms as to be capable of living under similar conditions.

The *Foraminifera* are the organisms to which reference has been made as performing so very important a part in the formation of certain strata on the earth's crust. They occur abundantly in all existing seas. They are to be met with in a fossil state, not only in chalk, but in almost all marine sedimentary strata; as for instance, in the hard limestones and marbles. The recent *Foraminifera* may therefore be looked upon as the oldest living representatives of any known class of organisms.

In the mud or "ooze," as it has been termed, which is brought up from great depths in many parts of the open sea, immense assemblages of *Foraminifera* are to be met with, chiefly belonging to one species however. In the absence of examinations conducted immediately on their being brought up to the surface by the sounding machine, it is not surprising that the question as to their occurrence in a living, or only in a dead state, should have remained undecided. Most of the authorities who have written on the subject being of opinion that they do not live at great depths, but that their shells and remains have drifted to the positions in which they were found from shallower waters, or have subsided from the upper strata of the ocean. Professor Huxley was one of the very few who leaned to the more correct opinion; he having declared, that although far from regarding it as proved that the *Globigerina* (the species referred to) live at these depths, the balance of probabilities seemed to him to incline in that direction. Other writers have offered surmises on the subject; but there, in the absence of anything like substantial proofs, were, of course, only estimated at what they were worth.

The difficulty is how to determine the point conclusively. For it seems legitimate to infer, that if these organisms are specially adapted to exist under conditions differing so widely from those present at or near the surface, the very circumstance of removing them from one set of conditions to the other, would inevitably destroy their vitality, and perhaps their normal structure, before it could become practicable to subject them to microscopic analysis. Nor is the difficulty an imaginary one. For, taking into consideration the entirely altered circumstances in which these creatures must find themselves placed when brought to the surface, locomotion, or even the protrusion of their filamentary appendages, could hardly be expected. The mere existence of the fleshy parts within their shells, and that too in an apparently recent condition, affords no proof, inasmuch as the great quantity of saline matter present in sea-water, and especially at great depths, would of itself alone account for their perfect state of preservation.

During the recent survey of the North Atlantic, I found that in certain localities, where the *Globigerina* deposit was of the purest kind and in the greatest plenty,

the specimens from the immediate surface stratum of the sea-bed alone retained their normal appearance, both as regards the perfect state of the sarcooid contents of the shells and the presence of the pseudopodia. The latter organs were never seen by me in an extended condition; but in the specimens alluded to, and in those only, occurred as minute bosses, resembling in shape the rounded rivet-heads on boilers, closely appressed to the external surface of the shell; whereas, in specimens from the substratum, the colour was much dusker, and these bosses were absent. And further, in these pure deposits the shells were to be found in every gradation, from the single chamber, of microscopic minuteness, hyaline transparency, and extreme thinness, to the dense Zeolite-like structure of the many-chambered mature shells, which are large enough to be readily distinguished by the naked eye. These facts, when taken in conjunction with the entire absence of the varied remains of other organized structures found in localities where the *Globigerina* are only scantily represented, afford, as I conceive, all but the direct proof, which can only be arrived at on witnessing locomotion, or the protrusion and retraction of the pseudopodia of the organisms in question.

Most fortunately, as it happened, this collateral evidence was rendered doubly conclusive by other proofs of a most unexpected and interesting kind. Before entering on these, I may state that the substratum, spoken of as differing in aspect from the immediate surface-layer, is nevertheless identical in composition; the difference in colour arising simply from decay. It contains no living *Foraminifera*; for the minute particles of matter becoming gradually condensed and aggregated together by molecular affinity, and the enormous superincumbent pressure exerting itself only in one direction, that is, vertically, its permeability by fluids is thus completely destroyed, and it is compacted into a dense mass of far too unyielding a nature to admit of its being traversed by living creatures of any kind. As the *Foraminifera* die off, their shells and decaying contents, together with the minute particles of amorphous matter associated with them, go to build up the calcareous strata of the earth's crust. I would mention, that in order to determine whether the *Globigerina* live as free floating forms in the mid strata of water, I attached a small open-mouthed bag, at about 200 fathoms from the extreme end of the sounding line, in a locality where the species was most abundant in the deposit, and brought it up through nearly 5,000 feet of water without securing a single shell.

But by far the most important and interesting discovery remains to be noticed, namely, the detection of a high order of radiate animal, in a living state, at a depth of a mile and a-half below the surface of the sea.

When we take into consideration the low position of the *Rhipidopod* in the scale of being, and the obvious probability, pointed out by Professor Huxley, that a class of creatures proved to extend so far back in time—that is, in a fossil state—must be able to maintain existence under extraordinary and variable conditions as regards light, temperature, and pressure, the sentiment engendered is rather one of wonder, that their vitality at great depths should have been so long and so stoutly maintained, than that it should now be so fully proved. But few persons were bold enough to suspect that creatures of a far higher type, namely, *Radiata*, could exist under similar conditions; and I freely admit, that nothing short of the most incontrovertible proof ought to be accepted in support of such a view. Fortunately, I am in a position to afford that proof.

In sounding midway, in the direct line between Cape Farewell, the southern point of Greenland, and the north-west coast of Ireland, in lat. $59^{\circ} 27' N.$, and long. $26^{\circ} 41' W.$, the depth being 1260 fathoms (or 2520 yards), whilst the sounding apparatus itself brought up a considerable quantity of minute granular particles, looking like a fine oolite, but which was, in reality, a nearly perfectly pure

Globigerina deposit, 13 star-fishes, from 2 to 5 inches in diameter from tip to tip of rays belonging to a genus plentifully represented on our own coasts, came up adhering to the extreme 50 fathoms of sounding line. These *Ophiocoma* were not only alive on being brought up out of the water, but some of them continued for fully a quarter of an hour to move about their long spinous arms. To render intelligible the significance of the entire circumstances, I must mention, that in order to ensure accuracy, it is always necessary, when sounding in deep water, to ascertain the depth by one sort of apparatus, and to bring up the sample of bottom by another. In the present case, the ascertained depth was 1260 fathoms, and 50 fathoms was accordingly "paid out" in the second operation of bringing up bottom, in order to make sure that the more complicated and unmanageable apparatus required for this purpose fairly rested on the bottom.

Now, supposing it possible that these star-fishes were drifting about in some intermediate stratum of water, between the bottom and surface, it is evident that they would have attached themselves indiscriminately to any portion of the entire 1260 fathoms of line; unless, indeed, they chanced to have been directing their course in a closely compacted column, which was traversed by the last extra 50 fathoms of line at the precise moment of their crossing it. Whether it be possible that they were drifting in such a column, or floating on a bed of seaweed or other substance, is immaterial, inasmuch as they could only have attached themselves as they did to the portion of line referred to under this one condition. But the very act of attachment would, I maintain, be impossible in the case of creatures whose movements are so sluggish, when the object which they had to grasp was moving upwards at the rate of two miles per hour (as it does when hauled up by the steam-engine), and without a moment's intermission. But even assuming it to be possible that they had drifted to the position in which they were captured, from distant and less profound depths, the fact of their vitality and vigorously healthy condition would be scarcely less extraordinary; for the distance from the nearest point of land, which is a rock off Iceland, is 250 miles; whilst the next nearest land, Greenland, is distant no less than 500 miles. But it must be obvious to every one who is at all conversant with the structure of the *Ophiocoma* and Echinoderms generally, that they are essentially creeping and crawling creatures, and of far too great specific gravity to float at all, under any circumstances.

Taking into consideration, then, the circumstances under which these *Ophiocoma* were taken, the extreme improbability of their having drifted to the locality in which they were found, from distant and shallower waters; and lastly, the peculiarities of structure, which render them wholly unfit to float or swim for even a brief period, we should have been fully warranted, I think, in believing that they existed in a living state at the bottom. In order to obtain some clue to the solution of the question, I very carefully dissected and analysed the contents of the digestive cavity of a specimen, immediately on its being brought up; and was most amply repaid by the detection of numerous *Globigerina* in every stage of comminution, and with the contained sarcodic matter in greater or lesser quantity. Whilst, therefore, the detection of these organisms in the digestive cavities of the *Ophiocoma*, afforded a most conclusive proof that the Foraminifera were living on the sea bed at the profound depth from which they were obtained; the fact of the star-fishes being captured with the fresh remains of the Foraminifera in their digestive cavities, proves that their normal habitation is at the same great depth, inasmuch as it has been sufficiently established that the *Globigerina* are present only at the bottom. I may mention that, within the past few days, in examining a sample of the *Globigerina* deposit brought up by a previous sounding on the same spot, I detected some Echinoderm spines, which at once struck

me as being identical with those on the *Ophiocoma*; and that, on comparison, my surmise proved to be quite correct: a further and very striking proof of the vitality of the *Ophiocoma* at the bottom being thus afforded.

ROYAL INSTITUTION OF GREAT BRITAIN.

WILLIAM ROBERT GROVE, Esq., Q.C., F.R.S., Vice-President, in the Chair.

On the Action of Gases and Vapours on Radiant Heat, by JOHN TYNDALL, Esq., F.R.S., Professor of Natural Philosophy, Royal Institution.

THE discourse commenced by a reference to the researches of Leslie, Forbes, and Knoblauch; but more especially to the admirable investigations of Melloni on radiant heat. These eminent men had left the gaseous form of matter practically untouched, and to extend our knowledge into this wide region was the object of the investigation on which the present discourse was founded.

The apparatus made use of, and which was applied in the experiments of the evening, consists of the following parts:—

1. A copper cube, C, containing water kept constantly boiling, and one of whose faces, coated with lamp black, forms the source of radiant heat.

2. A brass tube, 2.4 inches in diameter, which is divided into two portions, α and β .

α . The portion of the tube intended to receive the gases and vapours; it is stopped air-tight at its two ends by plates of rock-salt, and is attached to a good air-pump, by which it can be exhausted at pleasure. The length is 4 feet.

β . An air-tight chamber between the tube α and the cube C. It is kept constantly exhausted, and the calorific rays therefore pass from the radiating plate through a vacuum into the tube, thus retaining the quality which belonged to them at the moment of emission.

To prevent the transmission of heat by conduction from the cube C to the tube α , the chamber β is partly embraced by an annular space, in which cold water continually circulates.

3. A thermo-electric pile, furnished with two conical reflectors, and connected with an excellent galvanometer. One of the faces of the pile receives the rays which have passed through the tube α .

4. A second copper cube C', also filled with boiling water, and whose rays fall upon the second face of the thermo-electric pile. The two cubes C and C', thus radiating upon the opposite faces of the pile, tend, of course, to neutralise each other.

Between the cube C' and the adjacent face of the pile a screen S is introduced, being attached to an apparatus of Ruhmkorff's, capable of extremely fine motion; by the partial advance or withdrawal of this screen the two sources of heat can be caused to neutralise each other perfectly.

The tube α and the chamber β being both exhausted, the needle of the galvanometer is brought exactly to zero by means of the screen S. The gas or vapour to be experimented with is now admitted into the tube α , and if it possesses any sensible absorbing power, it will destroy the previously existing equilibrium. The consequent deflection of the galvanometer, properly reduced, is the measure of the absorption. In this way the action of eight gases and thirteen vapours have been examined, and also the action of atmospheric air.

Oxygen, hydrogen, nitrogen, and atmospheric air, respectively absorb about 0.3 per cent. of the calorific rays; this is the feeblest action which has been observed.

The most energetic action is that of olefiant gas, which at the tension of one atmosphere absorbs 81 per cent. of the calorific rays. Between those extremes stand car-

bonic oxide, carbonic acid, nitrous oxide, and sulphuretted hydrogen.

Below a certain tension, which varies for different gases, the amount of heat absorbed is exactly proportional to the density of the gas. Above this tension the rays on which the principal absorptive energy is exerted become gradually exerted, so that every augmentation of density produces a diminished effect.

In the case of olefant gas, for example, where a unit measure $\frac{1}{10}$ th of a cubic inch in capacity was made use of; for a series of fifteen such measures, the absorption was exactly proportional to the quantity of gas; subsequently, the ratios of the successive absorptions approached gradually to equality. The absorption produced by a single measure of olefant gas of the above volume moved the index of the galvanometer through an angle of $2\frac{1}{2}$ deg.; the tension of the gas being only $\frac{1}{11000}$ th of an atmosphere.

In the case of vapours, the most energetic is that of sulphuric ether; the least energetic is that of bisulphide of carbon. Comparing small volumes and equal tensions, the absorptive energy of sulphuric ether vapour is ten times that of olefant gas, and ten thousand times that of oxygen, hydrogen, nitrogen, or atmospheric air.

On a fair November day the aqueous vapour in the atmosphere produced fifteen times the absorption of the true air itself. It is on rays emanating from a source of comparatively low temperature that this great absorptive energy is exerted. Hence the aqueous vapour of the atmosphere must act powerfully in intercepting terrestrial radiation; its changes in quantity would produce corresponding changes of climate. Subsequent researches must decide whether this *vera causa* is competent to account for the climatal changes which geologic researches reveal.

Oxygen obtained from the electrolysis of water exerted four times the absorptive energy of the same substance when caused to pass through iodide of potassium, the greater action being due to the presence of ozone.

The radiative power of gases was examined by causing them to pass over a heated sphere of metal, and ascend in a column in front of the thermo-electric pile. Various precautions were taken to secure accuracy in the results. It was found that the order of radiation was exactly that of absorption; that any atom or molecule which is capable of accepting motion from agitated ether is capable in precisely the same degree of imparting motion to still ether. Films of gas on surfaces of polished metal were found to act like coats of varnish.

The speaker also investigates the physical connection of radiation, absorption, and conduction. In the foregoing experiments free atoms and molecules were dealt with, and upon them individually was fixed the responsibility of the effects observed. These effects are thus detached from considerations of cohesion and aggregation, which suggest themselves in the case of liquids and solids.

The reciprocity of absorption and radiation is a simple mechanical consequence of the theory of an ether.

But why is one molecule competent to stop or generate a calorific flux so much more powerfully than another? The experiments prompt the following reply:—The elementary gases which have been examined all exhibit extremely feeble powers both of absorption and radiation, in comparison with the compound ones. In the former case we have oscillating atoms, in the latter oscillating systems of atoms. Uniting the atomic theory with the conception of an ether, it follows that the compound molecule which furnishes *points d'appui* to the ether must be capable of accepting and generating motion in a far greater degree than the single atom, which we may figure to our minds as an oscillating sphere. Thus oxygen and hydrogen, which taken separately, or united mechanically, produce a scarcely sensible effect, when united chemically to form oscillating systems as in aqueous vapour, produce

a powerful effect. Thus, also, nitrogen and hydrogen, which, when separate or mixed, produce but little action, when combined to form ammonia, produce a great action. So also, nitrogen and oxygen, which when mixed as in air, are feeble absorbers and radiators, when united to oscillating systems, as in nitrous oxide, are very powerful in both capacities. Comparing small volumes and equal tensions, the action of nitrous oxide is 250 times that of air; a fact which perhaps furnishes a stronger presumption than any previously existing, that air is a mixture, and not a compound. Carbonic oxide is about 100 times as powerful as its constituent oxygen; carbonic acid is 150 times as powerful, while olefant gas, as already remarked, is 1000 times as powerful as its constituent hydrogen. In the case of the hydro-carbon vapours, where the atomic groups attain a higher degree of complexity, the action is even greater than that of olefant gas.

The speaker also refers to the experiments and observations of Niepce, Angstrom, and Foucault; but more especially to the admirable researches of Kirchhoff and Bunsen, as regards the influence of the period of oscillation on the rate of absorption. He points out how the grouping of atoms to systems in a resisting medium must tend to make their period of oscillations longer, and thus bring them into isochronism with the periods of the obscure radiations made use of in the experiments.

With regard to conduction, the speaker would illustrate his views by reference to two substances—rock-salt and alum. He was once surprised to observe the great length of time required by a heated mass of rock-salt to cool; but this was explained by the experiments of Mr. Balfour Stewart, who shows that rock-salt is an exceedingly feeble radiator. The meaning of this is that the molecules of the salt glide through the ether with small loss of *vis viva*. But the ease of motion which they are thus proved to enjoy must facilitate their mutual collision. The motion of the molecules, instead of being expended on the ether between them, and then communicated in part to the ether external to the mass, is transferred freely from particle to particle; or, in other words, is freely conducted. This *a priori* conclusion is completely verified by the author's experiments, which prove rock-salt to be an excellent conductor. It is quite the reverse with alum. Mr. Balfour Stewart's experiments prove it to be an excellent radiator, and the author's experiments show it to be an extremely bad conductor. Thus it imparts with ease its motion to the ether, and for this very reason finds difficulty in transferring it from particle to particle; its molecules are in fact so constituted that when one of them approaches its neighbour, a swell is produced in the intervening ether; this motion is immediately communicated to the ether outside, and is thus lost for the purposes of conduction. The lateral waste prevents the motion from penetrating the alum to any great extent, and hence it is pronounced a bad conductor. These considerations seem to reduce the phenomena of absorption, radiation, and conduction to the simplest mechanical principles.

CORRESPONDENCE.

Magenta Dye.

To the Editor of the CHEMICAL NEWS.

SIR,—In your Number for April 13, I observe a letter from "Magenta," stating that he has made on a new principle a Magenta powder soluble in water and which will dye either silk, wool, or cotton without any mordant, the powder only requiring hot water poured upon it and filtering to separate insoluble matter.

Now, my experience almost daily both in the manufacture and consumption of the above colour warrants me in asking "Magenta" to excuse me telling him that he is mis-

taken in having made colouring matter of the above character that can be practicably applied in the dyeing of cotton goods without a mordant having been first applied to the cotton, and without which mordant, cotton will not imbibe more of the colour than a mere tint; indeed cotton has no attraction for the colour until it has received a mordant.

With respect to the silk and wool, both those substances have a powerful attraction for the above colouring without any mordant, the silk in particular; both substances are dyed in quantities in a few minutes without any mordant: by the simple addition of the colour to hot water, both silk and wool will even attract the colour from a weak solution, when cold, in a few hours. I am not aware of another colour that will combine so freely with animal substance when the solution is cold.

In reference to "Magenta's" colour being soluble in water, I may tell him that all colours of this character are soluble in boiling water, no matter which of the metallic salts are used in the manufacture of the colour, water being always used to extract the colour from its carbonaceous impurities, after which it is filtered to separate them: although boiling water holds a large quantity of colour in solution, most of it will precipitate when the aqueous solution becomes cold, the precipitate being of a beautiful green bronze colour. The mere cost of methylated spirit requisite to hold the colour in solution is not worth a consideration when the advantages are compared with the disadvantages. Some of the salts make the colour much more soluble in water than others.

I have tried several of the French Magenta powders and paste, parts of which I have, but have found none that dye silk Magenta, but would also dye wool the same colour; but I fear that attempts to make the colour in England have been more numerous than successful, judging from the amount of patents and the inconsistency of the majority of them, as it would be quite impossible either to produce the very bright colour, or to produce it at a price that would sell it, by simply adhering to the patents.

I am, &c.

JAMES SHARP.

Leeds.

Adulteration of Food.

To the Editor of the CHEMICAL NEWS.

SIR,—One day last week some very fine-looking pickled girkins were placed on my dinner table; having partaken of them, a peculiar metallic taste I fancied they possessed induced me to leave a steel fork in the vinegar (from them) all night; the next morning it was found thickly coated with metallic copper. This, I believe, indicates the existence of the metal in poisonous proportions. The bottle from which they were taken bore the label of one of the oldest manufacturers in London.—I am, &c.

London.

Cu.

Natural Oxide of Silver.

To the Editor of the CHEMICAL NEWS.

SIR,—Having perused the letters which you have re-published from the *Times* and *London Review* on the subject of Oxide of Silver in Cornish Gozzans, and it having come to my knowledge that the fortunate possessor (or possessors) of the gozzans have at the present moment (i. e., say they have) 800*l.* worth of silver in a lot of ore, or gozzan, ready to be operated on by the plan of the equally fortunate gentleman who has discovered the process of obtaining in bulk four times as much precious metal as can be found by the existing old-fashioned method of assay,—I would respectfully suggest that the party who has formed the above estimate

will inform your readers whether it is founded on the new or old system of assay. As it follows, if calculated on the new, the ore cannot be worth more than 200*l.*, if sold in the market, while if valued on the old method of assay, it must be worth 3200*l.* by the new system of reduction, if we are to believe that this system yields at least four times as much as those hitherto in use.

An answer to this question before the reduction, with a report of the yield after, will throw more light on this curious question than pages of theoretical discussion. In questions of this nature, an ounce of fact is worth a ton of theory,—and if this matter will bear enquiry, the discoverer of this new process, and the owner or owners of the gozzans, will not fail to adopt the above suggestion, as a simple and expeditious means of putting themselves in a true position with the public.—I am, &c.

ARGENTUM.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

The Polysulphides of the Heavy Metals.—

It is generally supposed that alkaline polysulphides give the same precipitates with the heavy metals as a solution of sulphuretted hydrogen. It is not so, however. Berzelius observed that the salts of lead give a red precipitate with the pentasulphide of potassium, and that after a little time the red sulphide blackened, and sulphur separated. He observed a similar phenomenon with the salts of copper. The investigation has been extended by Schiff to all the heavy metals, who gives (*Annalen der Chemie und Pharm.*, bd. cxv. s. 68) the following as the results of his experiments. He precipitated dilute solutions of various salts with an excess of liver of sulphur, or yellow sulphide of ammonium, in an equally dilute solution:—

Salts of Lead.—A deep red precipitate; very unstable, whether in water or alcohol.

Salts of Copper.—A dirty-yellow or yellowish-brown precipitate, which loses sulphur without changing colour much. In alcohol it deposits easily, and may be separated from the greater part of the excess of sulphur. It does not appear to be obtained of a constant composition.

Salts of Silver.—A light yellow precipitate, which becomes first green and then black after standing sufficiently long.

Mercuric Salts.—A light yellow precipitate, soon becoming deeper, the containing liquid becoming opaline.

Mercurous Salts.—A dirty-yellow precipitate, which rapidly turns black.

Salts of Nickel, Cobalt, and Protoxide of Iron.—Black precipitates.

Salts of Peroxide of Iron.—A violet precipitate, becoming instantly yellow, and afterwards taking a dirty tint. Perchloride of iron added, drop by drop, to a solution of liver of sulphur produces a beautiful deep blue colouration, which lasts nearly an hour. A yellow precipitate falls at the same time, which first turns green and then blue. The blue colouration is a little disguised by the sulphide of iron which is separated.

Chromic Salts.—A yellow precipitate, which is immediately decomposed, with the separation of sulphur and the disengagement of HS. The precipitate which remains consists principally of hydrated oxide of chrome.

Salts of the Protoxide of Manganese.—A yellow precipitate, which soon changes into ordinary sulphide of manganese. Heat produces the change immediately.

Bichloride of Tin.—A yellow precipitate, from which sulphur separates immediately, but without change of colour.

Protochloride of Tin.—A liver-coloured precipitate, which in time changes into bisulphide of tin.

Chlorides of Gold and Platinum.—Yellow precipitates,

soluble with heat in an excess of liver of sulphur; the reddish-brown solution is precipitated yellow by acids.

Subnitrate of Bismuth.—Liver of sulphur colours it a dark yellow, and, after some time, black.

Salts of Antimony.—A bright yellow precipitate, from which sulphur soon separates. The change into the pentasulphide only takes place slowly in the cold, but rapidly when heated. The precipitate is at first soluble in an excess of liver of sulphur. Acids separate the orange-yellow pentasulphide.

Arsenic and Arsenious Compounds.—In a neutral liquid a yellow precipitate is produced, which is composed of sulphur and pentasulphide of arsenic, and soluble in an excess of the precipitant.

Salts of Zinc.—A white precipitate, sometimes a little yellowish, which, pressed between folds of blotting-paper and dried over sulphuric acid, contains no water, and is of a straw-yellow colour. It dissolves in acids, giving off H₂S and separating sulphur. When heated, sulphur is disengaged, and there remains 43·2 per cent. of the monosulphide of zinc, mixed with a very small quantity of sulphate. It follows that the precipitate is ZnS₅.

Salts of Cadmium.—A precipitate exactly like the monosulphide when dried, but with a slightly different shade in water. It loses sulphur when heated, and leaves 53·4 per cent. of monosulphide; hence the precipitate is CdS₅.

The tersulphide of potassium gives precipitates analogous to those produced by the pentasulphides.

The Tungstates.—Scheibler writes (*Journal für prakt. Chemie*, bd. lxxx. s. 204) that the tungstates which chemists have hitherto regarded as bitungstates are really salts, whose composition may be expressed by the general formula,—



The author has analysed the tungstate of soda, and found that, when crystallised in the cold, this salt contains $3NaO \cdot 7WO_3 + 16HO$; when crystallised from a hot solution it has only 14 HO. After calcination, water splits up this salt into two others,—one soluble and alkaline, $2NaO \cdot 3WO_3$; the other insoluble and crystalline, $NaO \cdot 4WO_3$.

Tungstate of Potash ($3KO \cdot 7WO_3 + 6HO$).—Slightly soluble; lamellar crystals.

Tungstate of Ammonia ($3NH_4O \cdot 7WO_3$).—Crystallises in the cold with 6 molecules of water, and in a hot solution with only 3.

Tungstate of Lithia ($3LiO \cdot 7WO_3 + [16HO?]$).—Is obtained in beautiful oblique, rhomboidal prisms, which are very soluble.

Metatungstates.—These salts are produced when a strong acid is added to a soluble tungstate, as long as the tungstic acid is re-dissolved; or better when an alkaline tungstate is boiled for a long time with an excess of tungstic hydrate. The metatungstates are very soluble.

Metatungstate of Ammonia ($NH_4O \cdot 4WO_3 + 9HO$), according to Margueritte, crystallises in beautiful octahedra. It may be obtained by keeping the dry tungstate of ammonia at 250° to 300°, as long as ammonia is given off, and then re-dissolving in water.

Metatungstates of Potash and Soda have a similar composition to the preceding salt. All three give, by double decomposition with chloride of barium, a salt crystallisable in beautiful octahedra with a square base, $BaO \cdot 4WO_3 + 9HO$. This salt is decomposable by cold water into an insoluble barytic salt and free metatungstic acid, which will combine again on boiling.

Metatungstic Acid is obtained by precipitating the metatungstate of baryta with sulphuric acid. It has a taste at once strongly acid and bitter. Evaporated in a vacuum over sulphuric acid, it deposits small crystals, which appear to be octahedra, with a square base, and contain $2HO \cdot WO_3 + 7HO$. Metatungstic acid may be evaporated over

a water-bath to a syrupy consistence; but when the concentration by heat is pushed too far, it suddenly changes into yellow, insoluble tungstic acid.

Metatungstic acid and the metatungstates completely precipitate the organic alkaloids. A solution containing only $\frac{1}{10000}$ th of quinine or strychnine is rendered cloudy by these re-agents. Other metatungstates may be easily prepared by double decomposition with the metatungstate of baryta. The author has obtained metatungstic ether by the action of iodide of ethyl on crystallised metatungstate of silver in a sealed tube.

The blue, crystallised oxide of tungsten may be prepared by calcining alkaline metatungstates.

II. ORGANIC CHEMISTRY.

New Hydroselenocyanides.—Kypke and Neger describe (*Annalen der Chem. und Pharm.*, bd. cxv., s. 207) two new compounds which they have obtained by passing slowly a current of chlorine through a sufficiently strong solution of pure selenocyanide of potassium. A red precipitate separates at first, which is soluble in an excess of the liquor, but, by degrees, the precipitate becomes permanent and the liquor acid. The precipitate is composed of minute red prisms. By continuing the action of the chlorine they turn yellow. The action is accompanied by the disengagement of an intense heat, and (more particularly during the latter change) chloride of cyanogen is evolved. The red and yellow bodies are not chlorinated compounds; for they are also produced by the action of nitric and hyponitric acids, and even the oxygen of the air on the selenocyanide of potassium. They are very unstable in moist air.

The red body is deliquescent; dissolves in water and hydrochloric acid, with the deposition of selenium. Sulphuric acid sets free hydrocyanic acid, and nitric acid gives rise to the formation of the yellow body. Caustic soda, ammonia, and cyanide of potassium dissolve it, and the solution gives the reactions of hydroselenocyanic acid. Ether and a diluted alcohol dissolve it, and the new body crystallises from these solutions in beautiful red prisms.

The yellow body is an oxidised product of the red. When dry it forms a yellow powder, which turns red or blackens in moist air. Water and hydrochloric acid separate selenium from it. Concentrated sulphuric and nitric acids dissolve it. Alcohol and ether dissolve it, leaving a red powder, which rapidly blackens, and the solutions deposit yellow crystals.

A concentrated ethereal solution changes, when heated, into a red crystalline mass, identical with the red body above described, hydrocyanic acid being evolved. Heated to 130°, out of contact with the air, the yellow body fuses and evolves white vapours, which condense in white crystalline plates, soluble in water, alcohol, ether, potash, and nitric acid, and which are perhaps selenide of carbon.

The authors do not give the composition of the red and yellow bodies, but remark that they appear to correspond to the bodies called pseudo-sulphocyanogen and perhydrosulphocyanic acid.

MISCELLANEOUS.

A New Alloy of Copper.—A substance resembling gold, and known as "oreide of gold," is extensively manufactured in the United States under the licence of the French patentees, MM. Mourier and Vallant. It is composed of 100 parts (by weight) of pure copper, 17 of zinc, 6 of common magnesia, 3·60 sal ammoniac, 1·80 quick lime, and 9 of crude tartar. The copper is first melted in a crucible, then the magnesia added, then the sal ammoniac, lime, and tartar separately, and in powder. These are kept from contact with the air, and are well stirred for about twenty minutes, until they are incorporated

together. The zinc is now added in strips, which are thrust below the scum formed on the top of the crucible. The mass is now stirred, the lid put on the crucible, and its contents kept fused for about twenty-five minutes; after which the crucible is opened, the slag skimmed carefully from the surface, when the molten alloy is poured out into ingot moulds if it is required to be rolled, or into iron moulds if designed for casting. When designed for works of art, however, it is best to cast it into ingot form first, then melt it in a furnace and cast it. This alloy is very ductile, and may be rolled into very thin leaf, but it is nearly as easily tarnished as common brass.

Chemical Society.—At the next meeting on May 2nd, the following papers will be read:—"On the Absorption of Poisons by Plants," by Dr. Daubeny. "On the Amount of Water displaced from the Hydrates of Potash, Soda, and Baryta, by Boracic and Silicic Acids," by Prof. Bloxam. "On the Graphite of Cast Iron," by Prof. Grace Calvert. "On the Action of Dibromide of Ethylene on Pyridine," by J. Davidson, Esq. "On Bromide of Carbon," by A. W. Lennox, Esq.

Improved Mode of Extracting Phosphorus from Bones.—*Le Génie Industriel* describes a process recently patented by M. Cari Mantrand, of Paris, for extracting phosphorus from bones more economically than by the processes heretofore employed. The calcined bones, reduced to a fine powder, are mingled with a sufficient quantity of pulverized charcoal to combine, as carbonic oxide, with all the oxygen of the phosphate. The mixture is placed in an earthenware cylinder varnished on the inside, filling the cylinder to three-fourths of its capacity. The cylinder is then heated red hot, and a current of hydrochloric acid gas is blown into it. The phosphate of lime is immediately decomposed, forming chloride of calcium and carbonic oxide, while the liberated phosphorus is evaporated and driven through a copper tube, which leads into a vessel of cold water, where the phosphorus is condensed. The chloride of calcium, disengaged of the charcoal, in contact with sulphuric acid, regenerates hydrochloric acid for a new operation. The labour of pulverizing the bones may be saved by digesting them with a solution of hydrochloric acid; using for this purpose the water of the condenser from the preceding operation.

The Cause of Fermentation.—In a recent communication to the Academy of Sciences, M. Pasteur, whose labours on fermentation are highly appreciated, gives an account of a curious microscopic insect, which is not merely produced during the process; but which is actually the cause of it. In lactic fermentation various substances are produced, viz., lactic acid, gum, mannite, butyric acid, alcohol, carbonic acid, and hydrogen. M. Pasteur having found that the vegetable ferment which transforms sugar into lactic acid, differs from those which produce gum, and which in their turn do not produce lactic acid, soon ascertained that none of those produced butyric acid. The existence of a special butyric ferment was therefore highly probable, and after many experiments during which M. Pasteur vainly endeavoured to get rid of the infusoria which infested his preparations, he was at length struck with this curious pertinacity, and at length came to the conclusion that these infusoria were themselves the ferment which he was in search of! This view was speedily confirmed by a great many experiments, which showed that the transformation of sugar, mannite, and lactic acid into butyric acid was exclusively due to these animalcules. Their form is that of small cylindrical rods rounded off at their extremities, generally straight and connected together in chains of two, three, or four links each, sometimes more. Their length does not exceed the fifteen-thousandth part of a millimetre! These diminutive creatures advance with a creeping motion, their body remaining nearly rigid, or at best experiencing but slight undulations. They are often bent at one or both of their

extremities, but this seldom happens in their youth. They are flagellarous, that is, they multiply by separation, one link after another dropping off from the parent animal, and becoming a parent of others itself. These vibrios may be regularly sown in any medium which affords them appropriate nourishment; but the most singular circumstance connected with them is, that they can live and multiply without the assistance of the smallest particle of air or free oxygen; nay, air and oxygen kill them, while a current of carbonic acid, a most deleterious gas, as we know, may be made to pass through the liquid in which they are, without affecting them in the slightest degree. This is the first instance on record of animal life being supported without free oxygen.

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the *CHEMICAL NEWS*, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 6d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part III. of the present Volume is now ready.

*. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

*. All Editorial Communications are to be addressed to Mr. CROOKES, and Advertisements and Business Communications to the PUBLISHERS, GARRIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the *CHEMICAL NEWS*, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

E. R. S.—The numbers are now out of print, but will be shortly reprinted. Thanks for the report.

J. W. W.—*Silvering Glass.*—We have a communication for this correspondent: where shall it be sent?

R. C. C.—Apply to Messrs. Munn and Co., publishers of the *Scientific American*, New York.

Ignotus.—What you ask is impossible, only small pictures can be so treated, and then it takes a long time. The ordinary process is the best.

J. Gould.—1. The silver or acid evidently contained some impurity. 2. It is the old name for the salt. 3. The alkali metal forms with absolute alcohol what is called "sodium alcohol."

W. R. R.—Received with thanks: they shall be inserted. We have heard something of the case you mention, and should like to hear further particulars. Can you favour us with them, not necessarily for publication?

Parchment-paper is made by immersing unsized paper in sulphuric acid, made by diluting the strong acid with half its bulk of water and allowing it to cool. Keep the paper in for about five seconds, and then remove to a large vessel of water. Wash well till all the acid is removed, and dry. The paper will have to be rolled to remove the crumpled appearance.

W. E.—No good description has yet been published: one will shortly appear in our pages.

T. E. D.—Clean the plates by rubbing them with very fine emery, and when bright platinise by immersion in a dilute solution of bichloride of platinum, connecting them at the same time with a battery.

Silver Ore.—A correspondent writes as follows:—"The copper mine which presents itself to my notice for experimental purposes is one in which the ore is boiled in muriatic acid to extract the copper, which is then precipitated by the addition of iron. 1st, therefore, Do you suppose that in either of those stages, or from the muriate of iron residue, the silver could be extracted; and how? And suppose the case of the waste ore—too weak in copper to be worth the above process—how would you advise me to proceed in order to 'globulate' the silver from its oxide, in however minute quantity it may be present?"

THE CHEMICAL NEWS.

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SCIENTIFIC AND ANALYTICAL CHEMISTRY.

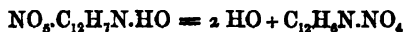
The Action of Heat on Nitrate of Aniline, by M. BECHAMP.

NITRATE of aniline crystallises in large hard transparent crystals, unalterable by air. They lose no water when kept for several hours at a temperature of from 100° to 120° C., nor when heated in a retort to 150°. This salt, then, may be regarded as anhydrous, and may be represented by the formula $\text{NO}_3 \cdot \text{C}_{12}\text{H}_7\text{N} \cdot \text{HO}$. Its stability is, as I before said, very great. It may be remarked that it represents nitraniline plus two equivalents of water: $\text{NO}_3 \cdot \text{C}_{12}\text{H}_7\text{N} \cdot \text{HO} = \text{C}_{12}\text{H}_6\text{N} \cdot \text{NO}_2 \cdot \text{N} + 2\text{HO}$.

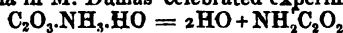
It was this point of view which induced me to undertake the experiment and to attempt to produce nitraniline in this manner. If we heat (in an oil bath) nitrate of aniline in a retort furnished with a receiver, it at first resists, without decomposing, 210°. But if the temperature is maintained for several hours at 150° to 180°, it will be seen to sublime without melting or disengaging water. The portions of the retort above the bath become covered with a crystalline coating, and beautifully white penniform crystals are deposited there. These crystals possess the characteristics of nitrate of aniline; they are soluble in water, and a violet tint is imparted to the solution by hypochlorate of lime; if thrown into a mixture of crystallised sulphate of iron and ordinary sulphuric acid, they give to it the colour of the red peach blossom. But as the last reaction belongs also to the nitrites, the question cannot be decided until after the analysis of the sublimed salt.

The temperature being then raised beyond 190°, the crystals at the bottom of the horn retort appear to become humid, then suddenly a vivid reaction is established and abundant vapours are disengaged. To condense these, it is requisite that the receiver should be followed by several flasks surrounded with cold water. The condensed liquid is a tarry product mixed with a little water. This substance is next treated under heat by equal proportions of concentrated hydrochloric acid and water; a yellow solution is thus obtained, and a semi-liquid black matter remains. The acid liquid, supersaturated with carbonate of soda, furnishes a light precipitate, and a yellow solution, which, if placed in a retort and distilled to dryness, yields a liquid of the same colour in the receiver, the odour of which slightly resembles that of aniline, and it dyes silks and the skin yellow. This product, supersaturated with hydrochloric acid, remains yellow; but when sufficiently concentrated in a water-bath, it ends by yielding colourless crystals of hydrochlorate. The solution, concentrated until it crystallises, and treated with ammonia, deposits a yellow, flaky, crystalline precipitate. That is to say that the hydrochlorate and the base present all the characteristics which have been assigned to nitraniline derived from

dinitrobenzole.¹ Thus, nitrate of aniline, submitted to the action of heat, decomposes according to the following equation:—

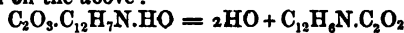


which may be compared to the decomposition of oxalate of ammonia in M. Dumas' celebrated experiment:



oxamide.

or to that of oxalate of aniline in Gerhardt's experiment founded on the above:



oxanilide.

According to this method of formation, nitraniline will be *nitranilide*, that is to say, a compound which is to nitrate of aniline what nitramide would be to nitrate of ammonia. However this may be, the yield is scanty; but my intention is again to attempt to regulate this reaction, which is the first of its kind, and which I propose to study on other organic nitrates.

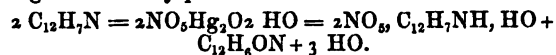
When, instead of heating the nitrate of aniline alone, aniline is added to it, the products and phenomena are of a different kind; but nitraniline is always the result of the reaction. Here is the *résumé* of an experiment: In a retort heated in a water-bath place 100 grammes of nitrate of aniline, in large crystals, and 50 grammes of aniline. This quantity of aniline is sufficient to dissolve at 160° all the nitrate, which re-crystallises out unaltered by cooling. But if the temperature is maintained between 180° and 195°, the colour of the liquid deepens and becomes more and more violet; it remains liquid, the surface being troubled as by a disengagement of gas, while in reality none takes place. The reaction occupies eight hours, during which time about 30 grammes of aniline and water are distilled. The residue in the retort solidifies while cooling. Recovered by hot water, it forms a red solution and a viscous residue. After cooling, decant the supernatant liquid and concentrate it; it will deposit crystallised nitrate of aniline, coloured by a red base.

Wash the viscous mass in water, and boil it several times in a weak solution of carbonate of potash. This treatment hardens it, and renders it pulverizable. The potash solution is yellow; distilled almost to dryness it furnishes in the receiver a yellow liquid which, if acidified by hydrochloric acid and concentrated, produces colourless crystals of hydrochlorate of nitraniline, from which the yellow base can be separated by the ordinary methods.

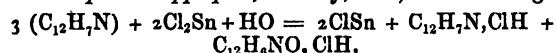
The viscous mass remaining after this treatment contains fuchsine—a substance yielding a blue solution—with hydrochloric acid, and the before-mentioned violet. But to this subject a special paper will be

¹ Care has been taken to determine that this yellow base differs from picric acid, and also from the yellow base mentioned in another memoir.

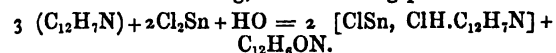
devoted. I now only wish to state further, that, in the action of nitrates on aniline, nitric acid is recovered combined with this aniline, and that it is only by a secondary action that nitrate of aniline can be attacked, as will by-and-by be set forth. In a regular operation, I repeat, all the nitric acid of nitrates is recovered; there is no reason, seeing the great stability of nitrate of aniline, to suppose that fuchsine is a product of the action of nitric acid. The equation of the formation of fuchsine by mercurous nitrate is the following, abstracting the accessory products:—



Let me be permitted, for this purpose, to call to mind that the equation given touching the formation of fuchsine is not, as M. E. Kopp stated (*Répertoire de Chimie pure et appliquée*, January, 1861) the following:



but rather the following, brackets being placed:—



The compound $\text{ClSn}, \text{ClH}, \text{C}_{12}\text{H}_7\text{N}$ (chlorostannite of hydrochlorate of aniline) being an isolated combination with very determinate characteristics (soluble, fusible, and volatile), which does not combine with anhydrous aniline, and which refutes any hypothesis admitting the presence of chlorine in fuchsine. Anhydrous aniline being still unknown, I intend specifying its characteristics in a future paper.—*Comptes-Rendus*, tome lii.

Additional Remarks upon Arsenical Paper-hangings, by Dr. W. FRAZER, Lecturer on Medicine to the Curmichael School of Medicine.

In the last number of the *Dublin Hospital Gazette* there is a note from Dr. Mulock, referring to a brief communication of mine, which recently appeared in the same journal.¹ He says that I did not in that paper, mention "the degree of heat required to make arsenic, or its various compounds diffusible;" and he also states that he is of opinion that "its deleterious properties entirely depend on this" volatility, and further, that "if not acted on by a heat under 150°F ., it could scarcely be injurious to the inhabitants."

It is quite true that I did not say one word in the paper which I published as to the degree of temperature necessary to volatilise arsenical compounds, my chief object being not so much to explain the *modus operandi* of the poison, or even to describe the symptoms which it causes when absorbed in repeated small quantities, as to call attention to the remarkable fact, that a law appears to exist as to the action of minute doses of certain mineral substances upon the human frame, and that when these minute quantities are absorbed, the effects which are noticed as the result of such a mode of exhibition can be distinctly separated from the ordinary action of full medical doses of these same substances, and still further from the effects of what we commonly call poisonous or excessive doses, and which we are most familiar with as the result of the exhibition of a large quantity of the substance at once. I fully coincide in Dr. Mulock's opinion, that absorption, the result of either volatility or diffusion, must in some way occur to explain

the effects of the arsenic upon the system; that, as a general fact, we must admit the truth of the old idea, that insoluble bodies are inert, meaning by insoluble, those which neither dissolve readily nor vaporise; but even this statement requires some qualification, for I do not consider that any satisfactory explanation has yet been given as to how calomel or trisnitrate of bismuth act, or in what manner these very insoluble substances become capable of influencing the human body, as they admittedly do. However, in the case of arsenic, its volatility would appear to be the most likely solution to explain the symptoms of poisoning which occur in rooms hung with papers largely charged with compounds of such a well-known poison, for I can hardly believe that the mechanical diffusion of particles of flock coloured with arsenical tints, and their subsequent ingestion, would suffice as an explanation. I, however, cannot assent to the statement that less than 150°F . would not render several of the arsenical compounds volatile, and as it may be of interest to some of my readers to have a brief statement of all that is at present known upon this point as far as my information extends, and also to have the deficiencies which still exist in this interesting inquiry pointed out, and the directions in which further researches are desirable, I shall give the following brief summary:—

Metallic arsenic does not fuse under 356°F .; it has the remarkable property at that temperature of passing into vapour directly from the solid form without previous melting. To effect this change it must be heated within closed vessels, as when the heated metal is exposed to air it absorbs oxygen, burns with a pale blue flame, and is converted into white fumes of arsenious acid. We know, as yet, little or nothing accurately as to the extent of its evaporation at common temperatures in dry or moist air; in the latter there are grounds for supposing that it would be considerable in amount.

Arsenious acid, when heated to 380° , softens, and is sublimed without undergoing previous fusion; its volatility at ordinary temperatures appears to be slight, but we know little or nothing definitely on the subject.

Arsenic acid when strongly heated becomes anhydrous, and at nearly a red heat is decomposed, yielding arsenious acid and oxygen; it is one of the most fixed of the arsenical compounds.

Arsenuretted hydrogen is permanently gaseous at ordinary temperatures, and very poisonous; when exposed to a cold of 40° it condenses into a limpid, colourless fluid. This gas might readily be formed from arsenical papers hanging on damp walls, with a layer of sour paste underneath.

Terchloride of arsenic is a heavy fluid boiling at 270° , but forming fumes in the air at ordinary temperatures, and escaping into vapour when the bottle containing it is unstopped.

Kakodyl, $\text{C}_4 \text{H}, \text{As}$, when pure, is a heavy oily liquid, which emits offensive fumes, and takes fire spontaneously in the air, although it does not boil under 338° . It can be obtained with ease by fusing together minute fragments of arsenious acid and acetate of potash, when its disagreeable odour becomes perceptible. It is not at all impossible that this or an analogous compound may be generated in some instances from arsenical wall-papers.

Oxide of kakodyl is procured in an impure state in Cadet's fuming liquid or Alkarsin. It is likewise inflammable, and evolves fumes in the air, requiring when pure a temperature of 248° for boiling.

Chloride of kakodyl boils at 212° ; it also is inflammable and fuming in the air. Several other easily

¹ See CHEMICAL NEWS, No. LXX. p. 210.

volatile arsenical compounds are already known, but the above will suffice to show that arsenic does escape from what we might consider a very permanent state of combination, which is further proved by an experiment of Dr. Davy's. On making superphosphate of lime with arsenicated sulphuric acid the resulting compound is at first largely charged with arsenic, but in the course of time, when freely exposed to the air, the arsenic gradually passes off, so that Dr. Davy has detected the arsenic thus escaping by suitable apparatus.

As a further proof that arsenical compounds become disseminated from wall-papers throughout the rooms where they are hung, we have the evidence of many who have examined the dust collected in these rooms from the tops of presses, picture-frames, philosophical instruments, &c.; thus I might mention Professor Taylor and Dr. Davy, as well as others, who have obtained arsenical sublimates on testing these dusty deposits, and even by the eye the light fibres of green flock have been recognised in this same dust, in some instances giving it a distinct yellow colour. Thus we perceive that the flock must adhere but slightly to the paper, and we are not without evidence that the colouring matter also adheres only loosely to tissues on which it is deposited, which might be expected from these colours being insoluble mineral substances not mordanted to the stuffs on which they are laid rather as a coating than a true dye. It may be worth while to recall one of Dr. Davy's experiments; he took a piece of green tartan about nine inches square, which weighed twenty grains, and by simply washing in water he removed five grains of colouring matter, the greater portion of which consisted of arsenite of copper, and if we merely shake this flimsy tissue enough of the colouring will be detached to satisfy any observer as to its dangerous character.

In many of these rooms in which persons have complained of injurious effects from exposure to arsenical papers or colourings, a peculiar odour has been described as perceptible to visitors, and sometimes to the inhabitants themselves, and a circumstance which I became acquainted with a few years since may throw light on the source of some of these odours. In old houses it often happens that wall-papers are hung again and again upon the walls without previous removal of the dirty or old paper underneath. This sometimes occurs from the laziness or carelessness of the workmen, but it may also take place from a desire to avoid the disturbance of old rotten partitions, cracks in walls, loose plastering, &c. We have in this way a succession of layers of old paper covered with all kinds of paints which have been agglutinated by size originally, and alternating with the paper of course are layers of paste, the entire being ready for putrefactive change, and should any damp attack the wall, it is little wonder that offensive odours will be developed, and if arsenic be present, it might, under such circumstances, be readily vapourised. A painter informed me that in these old rooms covered with successive strata of paste and paper, disagreeable odours were not uncommon, and he ascribed them altogether to the decomposing state of the wall-paper, and also asserted that the only way of getting rid of the annoyance was by the total removal of the paper which caused it.

In rooms in which gas is extensively burned we have a plausible explanation of one manner in which arsenic may be volatilized. In all the coal gas supplied to our houses there is an abundance of ammonia present, as any one can satisfy himself by examining the water in

this gas-meter after being a short time in use. In addition to this we have more than traces of sulphuretted hydrogen, or some sulphur compound, which has in some instances been in such injurious excess as to destroy the gilding on books, corrode the binding, and injure the paper (as, for example, in the library of the London Reform Club in London), and also to tarnish curtains, hangings, etc. Now a sulphide of arsenic might be readily formed by the sulphuretted hydrogen, and as it is freely soluble in ammonia we can understand how it might become disseminated through a room and prove injurious; but still I freely admit that we have no evidence as to the mode in which arsenic does become volatilized from wall-papers. There is strong probability for thinking that two or more different reactions occur, and it would be well worth studying the subject thoroughly; but even without this information as to the manner in which the poisoning occurs, we have, I think, fair evidence that these arsenical papers do in certain instances affect the health, and therefore the comfort of persons exposed to their influence; that they are further dangerous to children, who may ignorantly suck or swallow fragments of such paper, and above all, that their preparation must expose the workmen employed in their fabrication to serious risks and illness of a character which we have the means and the opportunity of preventing, and which, if not so obviated, we have good grounds for concluding must often prove fatal, suffering and even death being the exorbitant price demanded for our indulgence in a gaudy and highly dangerous colour.—*Dublin Hospital Gazette.*

On Some Metallic Phosphides, by M. P. VIGIER.

CERTAIN defined and crystallised metallic phosphides can be obtained by making the vapour of phosphorus react on the metal in a state of fusion in a hydrogen or nitrogen current. The following is an easy method for combining phosphorus and zinc:—

Introduce two vessels—one containing zinc, the other phosphorus,—in a porcelain tube, furnished at each extremity with two small tubulated globes. The zinc should be placed about the centre of the furnace; the phosphorus beyond, on the side whence the dry hydrogen arrives. Heat that part of the tube where the vessel of zinc is situated, and when the zinc begins to distil (which is very easy to perceive, on account of the two globes, which expose all that passes in the tube), boil the phosphorus by means of a spirit-lamp or hot charcoal; the two vapours combine with explosion, and the excess of phosphorus condenses in the second globe. By the disengaging tube of this receiver, the phosphuretted hydrogen is seen to burn at the moment of the reaction. Does this proceed from the direct combination of phosphorus with hydrogen, or from the partial decomposition of the phosphide formed? This important fact I shall explain further on.

When the cooled porcelain tube is broken, there will be found in it beautiful and well-defined prismatic crystals, one to two centimètres long, presenting a play of colours similar to bismuth crystals, an inflated and friable gray substance, and against the sides of the tube prismatic needles and a fused substance with very brilliant fracture. Analysis proves that these three substances have the same composition, PZn_3 ,—that is to say, pure phosphide of zinc.

This body can be decomposed in the air only at a temperature sufficiently elevated to form phosphate of

zinc. It is less fusible than zinc, and is attackable, with the aid of heat, by hydrochloric and sulphuric acids, disengaging phosphuretted hydrogen, PH_3 . Nitric acid dissolves it completely,—a circumstance which has helped me in analysing it. When pulverised it emits a slight odour of phosphorus. It is this phosphide which I gave to M. Cahours, who, by making it react on iodide of ethyle, obtained a compound of iodide of zinc and iodide of tetraethylphosphonium.

During the preparation of phosphide of zinc, small, red, transparent needles, of the same crystalline form as the phosphide, are almost always formed at the extremity of the porcelain tube.

Marggraf, of Berlin, wishing, in 1740, to combine phosphorus and zinc, mixed the two substances and submitted them to the action of heat in an earthen alembic. The phosphorus distilled off, the surface only of the zinc being attacked. In the cover of the alembic he found some little tufts of reddish needles, the composition of which he was unable to discover. Chemists since then have given them the name of *Marggraf's flowers*. I believe I have determined their composition and method of formation.

Phosphide of zinc crystals become phosphates by contact with a little oxygen without change of shape, and retain the red phosphorus produced during the reaction. Repeated experiments have assured me that Marggraf's flowers are crystallised phosphate of zinc, coloured by red phosphorus. I have been able to obtain far larger quantities by operating with sand-stone tubes, which are porous and allow air to pass through them.

If, in the above-mentioned process, the zinc is replaced by cadmium, and the temperature a little lowered, fused and crystallised gray phosphide of cadmium is obtained, breaking like phosphide of zinc, but in smaller needles. When the air is allowed to penetrate the tube we obtain needles of phosphate of cadmium and red phosphorus, identical with Marggraf's flowers.

Phosphide of tin is formed in the same manner as the two preceding. It preserves the form of the vessel, and on its surface can be traced the disposition of its crystals, which are very brilliant when they are separated from each other by fracturing the mass.

Phosphide of Sodium and its Employment in the Preparation of Phosphorised Bases.—Phosphorus combines energetically with alkaline metals. When phosphorus is thrown on sodium, melted in an iron crucible, placed in a receiver full of carbonic acid, phosphide of sodium is the result; but this body is not definite, and its preparation is dangerous. The following is the method for obtaining a definite compound:—Pour some rectified oil of naphtha or coal oil, boiling at about 120° , into a tubulated retort, furnished with a receiver. Introduce a globule of sodium and a piece of phosphorus, and when the two bodies are melted they combine, producing a glimmering light, and sufficient heat to distil the liquid rapidly. Then the sodium is seen to float, surrounded with black pellicles, which detach themselves and fall to the bottom of the retort. With a glass rod crush this substance, so that it may retain no sodium globules, and the combining process continues. Add the phosphorus, little by little, until there is an excess of it, which will be evidenced by the phosphorus crystallising on the sides of the cooled retort. Treat the phosphide thus formed with sulphide of carbon, and when it is perfectly free from phosphorus dry it in a water-bath in a current of carbonic acid. Black, pulverisable phosphide of sodium is thus obtained.

This compound can be preserved in dry air, but humid

air destroys it promptly. A considerable disengagement of phosphuretted hydrogen results from the contact of water and acids.

Chlorine readily attacks it, transforming it into chloride of sodium and perchloride of phosphorus.

Potassium and calcium behave in the same manner as sodium, and, doubtless, it is the same with the other alkali metals.

Phosphate of sodium placed in a retort in contact with hydriodic ethers speedily transforms them into iodide of sodium and phosphorised radicals.

I have thus obtained the trimethylphosphine and triethylphosphine with their iodides, and that in a quantity sufficiently large to allow me to hope I may complete the study of these interesting bodies.—*Société Chimique de Paris*.

On Some Applications of the Ferrocyanide of Potassium in Chemical Analysis, by EDMUND W. DAVY, A.B., M.B., M.R.I.A., Professor of Agriculture and Agricultural Chemistry to the Royal Dublin Society.¹

I HAVE recently been engaged in making some experiments on the ferrocyanide of potassium or yellow prussiate of potash, with a view to extend its applications in chemical analysis; for though this important salt has already been applied to a number of useful purposes in analytical research, still my experiments have shown me that its use might be advantageously extended, particularly as a re-agent in volumetric analysis, a form of analysis which has of late come into very general adoption, especially for technical purposes, on account of the great quickness, and at the same time, accuracy with which different substances may by its means be determined. The principles upon which volumetric analysis depend are so well known, that I need not refer to them; and though it possesses so many advantages over the older gravimetric method, in which the different substances are determined by weight instead of by volume, it yet has this drawback, that the preparation of the necessary standard solutions often takes considerable time, first, in order to obtain the substance to be used for this purpose in a sufficiently pure and dry state, and secondly, to form a solution of it the exact strength of which may be known: for though it may appear a very simple operation to dissolve a known weight of a certain substance in a given bulk of water or other solvent, yet, when this has to be done with such great precision as is necessary in these cases, it is a tedious and troublesome operation, and any inaccuracy in the graduation of the standard solution will render all determinations made with it more or less inaccurate. It is obvious, therefore, that it would be most desirable that the substances which are intended to be used as re-agents in volumetric analysis should be easily obtained in a pure state, and that where considerable time and trouble have been expended in graduating solutions of those substances, they should not be liable to undergo changes whereby their strength would be more or less altered, but that when standard solutions have once been made, they might be kept and used for a great number of determinations.

The ferrocyanide of potassium fulfils both these conditions; for it is in general met with in commerce almost chemically pure, and in a state in which it can at once be employed as a volumetric re-agent; and if at

¹ From the *Philosophical Magazine*, being part of a paper read before the Royal Dublin Society, December 27, 1860.

any time it should happen to occur not quite so pure, it can readily be purified by re-crystallisation; and, in addition to these important considerations, its solution is not prone to change, especially if it be not left exposed to the action of the light. In this latter respect it has a decided advantage over several of our most useful volumetric re-agents, viz., the permanganate of potash, the protosalts of iron, sulphurous acid, &c., which, from their being so prone to undergo spontaneous decomposition, must be either freshly prepared, or the strength of their solutions accurately ascertained every time they are used, if a day or so has elapsed between each determination.

The employment of the ferrocyanide of potassium as a volumetric re-agent depends on the following circumstances: viz., that it is readily converted into the ferridcyanide of potassium (red prussiate of potash) under different circumstances, and that the point where the whole of the former salt has been changed into the latter may easily be known, either by the use of a diluted solution of a persalt of iron (which gives with a drop of the mixture a blue or green colouration as long as any of the ferrocyanide remains unchanged) or by some other simple indication. Thus, for example, when chlorine is brought in contact with the ferrocyanide of potassium, this change, as is well known, takes place, which is expressed by the following symbols:—



The same occurs, as far as the conversion of the ferrocyanide into ferridcyanide, when an acidified solution of the former salt is brought in contact with a solution of the permanganate of potash, which is instantly decolorized by the reducing action of the ferrocyanide of potassium, which is thereby converted into the ferridcyanide, and this decoloration of the permanganate continues as long as any of the ferrocyanide remains in the mixture.

Again, if a solution of the ferrocyanide of potassium, acidified strongly with either hydrochloric or sulphuric acid, be brought in contact with a solution of the bichromate of potash, the same change of the ferrocyanide into the ferridcyanide immediately takes place.

The first reaction has been long known, and is the means employed at present for obtaining the ferridcyanide or red prussiate of potash for manufacturing and other purposes; the second reaction has been more recently discovered; but I am not aware that the third, in the case of the bichromate, is generally known, or that the changes which occur in the reaction have been previously studied.

From experiments which I made, it would appear that when a solution of ferrocyanide of potassium, acidified with hydrochloric acid, was mixed with one of the bichromate of potash, the following reaction was produced, viz. $6(K_4FeCy_6) + KO_3 + 7HCl = 3(K_3Fe_2Cy_6) + 4KCl + Cr_2Cl_3 + 7HO$; for, amongst other facts, I may observe, that when I mixed together solutions of the two salts in the proportions corresponding to 6 equivalents of the ferrocyanide of potassium to 1 of the bichromate of potash (as indicated in the above formula), acidifying the mixture with hydrochloric acid, I found that the whole of the ferrocyanide was converted into the ferridcyanide, and that any quantity less than that proportion of the bichromate of potash left more or less of the ferrocyanide unchanged. The same results followed the use of sulphuric acid; and it appears that a similar reaction occurs with this acid as with hydrochloric acid, with the exception that in this case

the 4 equivalents of chloride of potassium and the 1 equivalent of sesquichloride of chromium are replaced by 4 equivalents of sulphate of potash and 1 of the sesquisulphate of chromium.

The proportion of either acid used, provided there is enough to strongly acidify the mixture, does not appear to affect the reaction; for I obtained precisely the same results where a very large amount of acid was employed as where the quantity necessary only to strongly acidify the mixture had been added.

On these three reactions which I have noticed, may be based the means of employing the ferrocyanide of potassium in several useful determinations, the first, and one of the most important, of which is the ascertaining the amount of available chlorine in the chloride of lime or bleaching powder, which is a matter of much importance in many of the chemical arts, but particularly in bleaching; for not only does the commercial value of this substance depend on the quantity of available chlorine that it contains, which is subject to great variation from exposure to the air and other causes, but likewise it is of the greatest importance that the bleacher should readily be able to determine from time to time the strength of the bleaching liquor which he employs: for if it be too strong, he knows that the fabric which he bleaches will be injured; and if too weak, it will not be sufficiently bleached, and the process must be repeated, which incurs much additional expenditure of time.

Various methods have from time to time been proposed for the determination of the value of chloride of lime; but the greater number of them, from the trouble required to make the test-solutions, and their not keeping when made, as well as the skill required in their use, render them inapplicable for general purposes.

I shall, therefore, merely refer to the two methods which are chiefly used at present to determine the value of this important substance. The first is Gay-Lussac's, in which the amount of chlorine is ascertained by seeing how much chloride of lime is necessary to convert a given quantity of arsenious into arsenic acid; the second is Otto's, in which protosulphate of iron is substituted for arsenious acid, and the determination of chlorine is made by seeing how much of the bleaching powder is required to change a given weight of the protosulphate of iron into a persalt of that metal: these processes are so well known that I need not describe them.

In both these methods I find that more or less chlorine is always lost, which, however, may be reduced to a minute quantity by very carefully adding the solution of chloride of lime either to that of arsenious acid, or of protosalt of iron; but in ordinary hands they (especially the latter process) will yield results in which too small a proportion of chlorine will be indicated, from the loss of that substance which will invariably take place.

The ferrocyanide of potassium answers admirably for the estimation of available chlorine in the chloride of lime, when used in the manner I shall presently explain, and, according to my experiments, will give in ordinary hands far more accurate results than either Gay-Lussac's or Otto's method. I am aware, indeed, that this salt was proposed by Mr. Mercer some years ago for this purpose; but the way which he recommended it to be used (which consisted in dissolving a certain weight of the ferrocyanide in water, acidifying it, and then adding the solution of bleaching powder from a burette till all the ferrocyanide was converted into ferridcyanide) is, I find, not a good manner of employing the ferrocyanide in this estimation, and, like the other methods, will lead to a loss of

chlorine; for when the solution of chloride of lime is added to the acidified ferrocyanide, a portion of the chlorine is separated, especially if the bleaching liquor be added too quickly, or is not greatly diluted. But the way I propose of using the ferrocyanide of potassium in this important valuation is to mix together a certain quantity of a standard solution of ferrocyanide with a given amount of a graduated solution of the chloride of lime, using more of the former salt than the latter can convert into ferridcyanide; then adding hydrochloric acid to dissolve the precipitate formed and render the mixture strongly acid, and finally ascertain, by means of a standard solution of bichromate of potash, how much of the ferrocyanide remained unconverted into the ferridcyanide by the action of the chlorine of the chloride of lime,—which is effected by adding slowly from a graduated burette the standard solution of bichromate, till a minute drop taken from the well-stirred mixture by means of a glass rod, ceases to give, with a small drop of a very dilute solution of perchloride of iron, placed on a white plate, a blue or greenish colour, but produces instead a yellowish-brown.¹ When this latter effect is observed, it indicates that all the ferrocyanide has been converted into ferridcyanide; and as 147.59 (one equivalent) of bichromate of potash is capable of converting 1267.32 (six equivalents) of crystallised ferrocyanide of potassium into ferridcyanide, and as 422.44 (two equivalents) of the ferrocyanide are converted into the same substance by 35.5 (one equivalent) of chlorine, as is seen by the formulæ already given, knowing the amount of chloride of lime employed, we have all the data necessary to calculate the per-centage of chlorine.

(To be continued.)

TECHNICAL CHEMISTRY.

On the Composition of Cast Iron and Steel, by M. E. FREMY.

I HAVE already shown that iron, cast iron, and steel do not appear to me to be allied, in relation to their composition, as is generally admitted; and it is not correct to say that steel is simply a combination of iron and carbon less carburated than that forming cast iron.

Without absolutely denying the influence exercised by carbon on the properties of steel and cast iron, I purpose nevertheless to show that several other metalloids possess the property of greatly modifying the characteristics of cast iron and steel, and that these bodies do not exist accidentally in these compounds; and that all the uncertainty connected more especially with the manufacture of steel may probably be traced to the action, hitherto little studied, of foreign bodies.

The object of the experiments which I now make known is to determine the conditions under which nitrogen can combine with iron.

Every chemist is aware that to M. Despréztz we owe the important discovery of nitride of iron; to him we owe the demonstration that iron, at red heat, decomposes ammoniacal gas, fixes the nitrogen, becoming itself white and brittle, and augmenting in weight, sometimes to the extent of 11.5 per cent. Submitted to the action

of acids, this body produces a salt of iron and an ammoniacal compound.

These clear results have, however, been doubted by some chemists. Some have thought the increase in the metal's weight was owing to an oxidation produced by the water or air which ammoniacal gas is capable of retaining; others, that the modifications of the physical properties of the iron were due to an alternate phenomenon of the oxidation of the metal and the reduction of the oxide by the hydrogen of the ammonia.

Though convinced that the experiments of M. Despréztz have no need of confirmation, I wished, however, to repeat the contested experiments in eliminating all sources of error which could arise from impurity in the ammonia or the humidity of the gas. I feel it incumbent upon me to state that every trial has completely confirmed M. Despréztz's position. In numerous experiments I have invariably seen red-hot iron decompose ammoniacal gas, producing the brilliant white body called "nitride of iron" by M. Despréztz.

The opinion has been advanced that the product of the decomposition of ammonia by iron might be a combination of metal with a hydride of nitrogen less hydrogenated than ammonia.

This question must be decided by experience. In fact, under the influence of oxygen, the compound studied by M. Despréztz produces peroxide of iron. In conducting this decomposition in a porcelain tube, communicating with the tubes designed to fix the water produced by the reaction, it is easy to distinguish whether the compound was a nitride or an amide of iron.

This experiment has been made with the greatest care. A known weight of the nitrogen compound was made red-hot in a current of oxygen, the metal was transformed into pure peroxide of iron, nitrogen was disengaged, and the tubes for absorbing the water underwent no alteration in weight. This experiment appears to me conclusive, and proves that the body produced by the action of ammoniacal gas on iron is actually nitride of iron, and that it contains no hydrogen.

This essential point being once established, it became necessary to determine under what other circumstances iron would combine with nitrogen.

I have first examined the action of pure nitrogen on metallic iron. The nitrogen was obtained either by decomposing nitrite of ammonia, or by the action of copper on atmospheric air, and the gas was purified and dried by the most efficacious means. It results from these experiments that nitrogen combines with great difficulty with the iron prepared by the processes usually employed in manufacture, but that it unites with the metal when presented to it in a nascent state. Thus, I have obtained nitrogenised iron by making nitrogen impinge on the oxide of iron at the moment of its reduction by either hydrogen or charcoal.

Cyanogen also modifies the properties of iron; but I have reserved the study of this reaction for another paper, to be devoted to the study of the phenomena resulting from the combined action of nitrogen and carbon on iron.

The processes to which I am about to refer doubtless yield nitride of iron, but the reactions are slow and incomplete. Thus, completely to nitrogenise small fragments of iron wire by ammonia, I have been obliged to pass the current of gas over the red-hot metal for three entire days.

In order to submit nitride of iron to a thorough chemical examination, and, above all, to study what influence this body is capable of exercising on the con-

¹ The yellowish-brown colouration which is at first produced when enough of the bichromate has been added, quickly changes to a greenish colour by some secondary reactions which take place when the persalt of iron is left in contact with the mixture. But this does not interfere with the test; for it is the first effect which is produced which indicates the completion of the reaction, and not the after changes which may result.

stitution and properties of steel, I have been obliged to devise a new method, which enables me to prepare this metallic nitride with facility. I have been so fortunate as to arrive at this result by decomposing at red heat protochloride of iron by dry ammoniacal gas. I introduced into a porcelain tube about 200 grammes of anhydrous protochloride of iron. This tube I made red-hot, and passed over the salt a current of ammoniacal gas, furnished by ordinary liquid ammonia, slightly heated, the gas being dried by passing through long tubes filled with caustic potash.

Under the influence of ammoniacal gas the metallic chloride decomposes rapidly; chloride of ammonium and a curious amide salt are disengaged. This salt is instantly decomposed by water, producing ammonia and oxide of iron. After the operation an inflated and partly melted mass is found in the tube; it is sometimes gray and often also metallic, white, and brilliant. This body is nitride of iron.

I submit to the inspection of the Academy about 200 grammes of nitride of iron obtained in this way. This body, till now hardly known to chemists, can henceforth be prepared with the greatest facility. It will, I have no doubt, become a new and valuable adjunct to our researches, by furnishing nitrogen to mineral substances or organic bodies. This process for the production of nitride of iron applies to the preparation of other metallic nitrides. By the same method I have obtained combinations of nitrogen with the metals of the iron group. A special paper will be devoted to these compounds.

I have proved that nitride of iron, proceeding from the decomposition of protochloride of iron by ammoniacal gas, possesses all the properties of that obtained by passing ammoniacal gas on red-hot iron.

This nitride is easily reduced to powder; it is less oxidisable than pure iron; it is very slowly attacked by nitric acid, but very speedily by sulphuric and by hydrochloric acids.

Nitride of iron when dissolved in acids produces ammoniacal and ferruginous salts.

According to the experiments made at my request by M. E. Becquerel, nitride of iron becomes magnetised readily and permanently like steel, only this property appears less developed than in ordinary steel.

Nitride of iron is remarkable for its fixity, and, in this respect, is allied to nitride of titanium, which has been so carefully studied by MM. Wöhler and H. Deville; in fact, it may be made red-hot without undergoing decomposition. Oxygen attacks it only at a high temperature, and then transforms it into peroxide of iron.

Nitride of iron, when heated in a charcoal fire, undergoes an important modification, to which I must revert when treating of the chemical constitution of steel. It is transformed, in this case, into a metallic mass analogous to steel, and, like it, becoming very hard by tempering. If nitrogen is present in this new compound it does not exist in the same state as in nitride of iron; for when the hardened product is heated in a hydrogen current no trace of ammonia is disengaged.

The most remarkable reaction of nitride of iron is that which it exercises on hydrogen. When slightly heated in this gas it decomposes immediately, yielding ammonia, and leaving a residue of pure iron.

This direct combination of hydrogen with the nitrogen contained in a metallic nitride appears to me a very curious fact. It, however, proves that nitride of iron can be employed to yield nitrogen to other compounds. The easy decomposition of nitride of iron by dry

hydrogen has enabled me to determine the different circumstances in which iron can unite with nitrogen. This experiment, in fact, removes all doubt.

It would be otherwise were the nitride attacked by an acid and the liquid then decomposed by potash. Reagents, and especially potash, often contain nitrates which, under the influence of protoxide of iron, produce ammonia.

The action also of hydrogen on nitride of iron has enabled me to analyse this compound readily. To determine the composition of nitride of iron it is necessary only to estimate the loss this body undergoes when heated in dry hydrogen.

It results from my analyses that nitride of iron obtained by means of protochloride of iron contains 9.3 per cent. of nitrogen, this composition corresponding to a nitride represented by the formula Fe_3N . By making ammoniacal gas react on iron, M. Despréts has proved that the weight of the metal augments sometimes as much as 11.5 per cent.; the nitride formed in this case would be represented by Fe_2N .

I shall not now dwell upon the formula of nitride of iron, for there is no proof that this compound has yet been obtained in a state of absolute purity; the temperature at which it is formed, and the hydrogen atmosphere then surrounding it are capable of varying its composition. It is, however, to be presumed that iron can unite with nitrogen in several proportions, as is shown by the following experiment:—Submit to the action of ammoniacal gas for twenty hours some small, red-hot cylinders of pure iron, sufficiently large to render the chemical action incomplete, and the weight of the metal, under these circumstances, does not augment more than 6 per cent.

After this experiment, the metallic cylinders, when examined, will be found to be composed of two very different parts: the external one almost melted, exceedingly friable, and may be detached by the slightest blow; the other, internal, rather hard, and still metallic. The external part is formed of 9.8 per cent. of nitrogen and 90.2 per cent. of iron, corresponding to the formula Fe_3N . Thus, the nitride produced by the action of ammoniacal gas in excess on iron has the same composition as that resulting from the decomposition of protochloride of iron by ammonia. The internal and still metallic portion can be cut by a file; it is, nevertheless, very brittle and contains nitrogen, but in much smaller proportion than the preceding. It presents in its general aspect a certain analogy with metal called in the foundries burnt iron.

It would be a curious circumstance to ascertain whether this accident of its manufacture which deprives iron of all its useful properties is not owing to a combination of iron and nitrogen. This is a point not to be neglected in my future researches.

Such are the new facts relative to the history of nitride of iron which I wish to make known to the Academy. I will sum them up in a few words:—

1. The object of my first experiments was to reproduce and prove the exactness of the experiments M. Despréts has described in his paper on nitride of iron.
2. I then established the fact that the body produced by the re-action of ammoniacal gas on red-hot iron is really nitride of iron and not of amidide: it contains no hydrogen.
3. It results from my experiments that the direct combination of nitrogen and iron takes place chiefly when the metal is in a nascent state.
4. I have proved that nitride of iron is formed with

the greatest facility by decomposing anhydrous protochloride of iron by ammoniacal gas. This method applies to the preparation of other metallic nitrides.

5. The composition of nitride of iron prepared by the action of ammoniacal gas either on iron or on protochloride of iron, is the same; it contains about 9.5 per cent. of nitrogen, and may be represented by the formula Fe_3N .

6. Nitride of iron is completely modified when heated in a charcoal fire; is no longer decomposed by hydrogen; and appears to resemble steel.

In another Memoir I shall ascertain whether nitride of iron can be employed in the preparation of steel.—*Comptes-Rendus*.

PHARMACY, TOXICOLOGY, &c.

'Supposed Case of Poisoning.'

THAT medical men are sometimes extremely rash in expressing opinions, even when they involve a charge of murder, is a fact unfortunately too well known to need additional illustration. A case, however, has just occurred at Cheltenham to which we feel bound to allude, not only on account of the want of chemical knowledge and skill displayed, but because it serves to justify the opinion we expressed a short time ago that the generality of medical men are not possessed of the necessary qualifications for the office of public analysts. We may relate the case, as we find it detailed in the *Cheltenham Chronicle*, in a few words. An illegitimate child died rather suddenly in the Cheltenham Workhouse, and a post-mortem examination of the body was made by the medical officers. The stomach was found perforated, and the mucous membrane was of a dark colour, with a few minute white spots upon its surface. This was held to be occasioned by arsenic, and accordingly an inquest was held, at which the following evidence (which we, of course, condense) was given:—

Dr. BIRD, of Cheltenham, said he took some mucus from the duodenum and stomach, mixed it with distilled water and hydrochloric acid, and boiled a piece of clean copper wire in the mixture. The copper wire became coated with an arsenical deposit. He next cut portions of the stomach, colon, and duodenum into small pieces, and boiled these with distilled water and potash. He then added some hydrochloric acid, and boiled this mixture with another piece of copper wire, which also showed the presence of arsenic. He then took a portion of the same fluid, and added some ammonio-sulphate of copper, which gave a bright green colour. To another portion of the same fluid he added ammonio-nitrate of silver, which gave a precipitate of a "slight lemon-yellow colour." He then boiled another portion of the duodenum with water and hydrochloric acid, and placed in the mixture a piece of bright copper wire, which quickly became coated with a white coating, indicating arsenic. After drying the wire he placed it in a tube, and on heating the tube the smell of arsenic was very distinct. The Doctor added that he had no hesitation in saying that the stomach contained a quantity of arsenic.

Mr. ALFRED SANDERS, surgeon, said he had tested a piece of the stomach by Reinsch's test, and the copper soon became covered with a coating of arsenic. He also tried other tests, all of which gave unmistakable evidences of the presence of arsenic. He had not the

slightest doubt that the body contained a quantity of arsenic.

At this stage of the proceedings a gentleman suggested that some portions of the stomach which remained should be handed to a competent analytical chemist and the inquest adjourned. The Coroner assented, and the remains were placed in the hands of Mr. Horsley, the recently-appointed analyst at Cheltenham. When the inquest was resumed, Mr. Horsley detailed the experiments he had made, all of which went to prove that not a trace of arsenic was discoverable in any of the matters handed over to him. Upon this Dr. Bird and Mr. Sanders recalled the evidence they had previously given, and expressed themselves perfectly satisfied that no arsenic was present in the body, and that the child died from natural causes.

It would be a waste of space to comment at any length upon Dr. Bird's evidence, so manifestly absurd is it to the merest tyro in chemical and toxicological science. The idea of adding ammoniated sulphate of copper in a trial for arsenic to hydrochloric acid and potash (we are not told which was in excess), and, still worse, adding nitrate of silver to some of the same fluid, would never have occurred to any one having the smallest knowledge of chemistry. We may point out, however, one source of error which might possibly have serious consequences when a medical man without special means sets about testing for arsenic. The hydrochloric acid ordinarily supplied to surgeons and pharmacists is notoriously contaminated with the poison. We saw recently a specimen in use in the dispensary of one of our large metropolitan hospitals, which contained nearly 0.1 of a grain per cent. Common sulphuric acid also contains it, and in this case Mr. Sanders ingeniously confessed that, supposing he really did find arsenic, he might have added it himself from this very source, although we are not told how he used the sulphuric acid in Reinsch's test. The remainder of the evidence of these gentlemen we may leave with all its absurdities, only remarking that copper boiled with animal matters almost invariably suffers some change of colour, which by an inexperienced man may be taken for a metallic deposit, and that the apparent sublimate which Dr. Bird procured was no doubt due to his neglect of the common precaution of washing his copper with ether to free it from fatty matter, which always adheres.

Mr. Horsley's evidence calls for no remark, except to point out his error of ascribing the method of detecting arsenic by distilling off the chloride to Dr. Taylor, when it has been shown to belong exclusively to Dr. Odling. In a general examination we think Mr. Horsley would be needlessly inconvenienced by precipitating the antimony with tannin; but as in this case his attention was solely directed to the detection of arsenic, it was of no moment.

In conclusion, we can only express a hope that medical men will abstain from undertaking examinations of this kind, and in all cases hand the suspected matter over to those who are specially qualified for the task. Such mistakes as that reported generally entail a severe penalty on the perpetrators by the want of confidence which they inspire; but when we reflect on the possible consequences to the unfortunate mother in this case—with already one sin to answer for before a world which has little charity—if Dr. Bird's and Mr. Sanders' evidence had passed uncontradicted, we cannot help thinking that the formal censure of a coroner's jury is but a small punishment for "originating a charge of so serious a nature, and which had no foundation in fact."

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Two Lectures on Musical Acoustics, and on the Physiological and Psychological Causes of Musical Harmony and Discord, by Professor HERMANN L. F. HELMHOLTZ, of Heidelberg.

LECTURE I.

In the Lectures which I shall have the honour of delivering before you I will try to explain why there is difference between consonant musical intervals and dissonant intervals. The law which rules this difference has been known, partially, at least, for a long time. The old Greek musicians knew what was the proportionate length of strings of the same materials, make, and tension, adapted for giving consonant intervals. The discovery of this law is ascribed to Pythagoras. He found out that if you wish to get the octave of the sound which any string gives, you must shorten the string to half its length; if you wish to get the fifth of the fundamental sound of the string you must shorten the string so that its length becomes two-thirds of the former length, and so on.

The Greek musicians knew this law only in relation to strings. The modern natural philosophers found out that if the nature and tension of the strings are alike, the length of a string is in inverse proportion to the number of vibrations. They, therefore, found out the real cause of this law, which was at first adopted for strings. They concluded from the old Greek law of consonance that there was a definite proportion and a simple proportion of the numbers of vibrations between two sounds which give consonantal intervals. The sound of the higher octave is produced by twice as many vibrations as the lower octave, or fundamental sound. A sound which is higher by one-fifth makes three vibrations in the same time where the fundamental sound makes two vibrations. A sound which is higher by one-fourth makes four vibrations in the same time that the fundamental sound makes three vibrations; and in the same proportion for the major fifth and minor third. In this way the proportions for all simple consonant musical intervals can be given by the first six numbers of the series of the whole numbers. This is a curious law. We do not understand at first what the series of the whole numbers has to do with the consonantal intervals. But you see also that this law is a fundamental law for the whole construction of the musical scale, for the musical scale must contain those consonant intervals; and, therefore, the series of sounds which are used in our musical scale is given by these proportions of the whole numbers. There has been much discussion about this law. Several natural philosophers have tried to find an explanation of it. The common hypothesis was that the human soul found a peculiar pleasure in simple ratios and a dislike of complex ratios, —ratios which could not be expressed by the simple number. But you see this explanation is no explanation at all; because it may be, if you have not studied natural philosophy, you do not know that a sound is produced by vibrations, you do not know that higher sounds have therefore more vibrations in the same time than lower sounds, and you do not know anything about these simple proportions of consonant intervals. Therefore this explanation cannot be considered as sufficient, and I will try in these Lectures to describe those physiological and psychological phenomena which explain this fundamental law of the musical scale.

In this first Lecture I will explain the different qualities, the physical differences in the sound, and the physiological functions and faculties of the human ear, which are necessary for distinguishing and perceiving those differences of sound which are really distinguished and perceived. I

will suppose that all of you know by common experience that every sound is produced by the vibrating movement of the air, or by the vibrating motion of any elastic body by which sound is transported by the air to the ear. In many cases you can see these vibrations, if they are vibrations of solid elastic bodies; in other cases you can, if the sounds are strong enough, feel the vibrations of the air; this refers principally to the vibrations of the lower sounds, which are the most easily felt.

These vibrations are produced, as you know, in many different ways. The common law for the production of musical sounds is that the motion of the air which is performed during every single vibration is repeated regularly a great number of times. We distinguish, as you know, between such sounds as are called noises and musical sounds. The motion of the air which corresponds to noise may be quite regular, but in such cases there is no regular repetition of the same motion. In musical sounds we have a regular repetition of the motion which was executed during the first vibration; this motion is repeated in equal periods of time, and quite in the same way. That constitutes the principal and fundamental difference between a musical sound and a regular sound or noise. Therefore, the motion of the air which corresponds to musical sounds is commonly a motion which goes backward and forward; it is not constant and continuous, but a vibrating and oscillating motion.

These oscillations of the air, and of all elastic bodies, are produced in different musical instruments in very different ways. You have, first, instruments constructed with strings, as the monochord, the piano, the harp, the guitar, or the violin. In those instruments a certain string is moved and put into vibration, and the vibrations of such a string continue during a certain time, and give regular shakes to the ends of the string, which are fastened upon the sounding-board, and from thence transmitted by the air to the ear. In other cases you have vibrations of the air produced, as in organ pipes. A thin current of air is moved against the lip of the pipe and there broken, and by this breaking the vibrations are produced. In other cases you have elastic plates, which alternately open and close apertures for the air. In this way all reed pipes are constructed, the air is blown into a box with openings. An elastic plate closes the openings, and when vibrating sometimes opens the hole by which the air escapes, and then closes it again, so that the current of air is interrupted, and becomes separated into single pulses of air. In this way the sound is produced in the harmonium, the reed pipes of the organ, the oboes, the clarionets, and so on. In the trumpet there is a similar mode of interrupting the current of the air, which comes out of the mouth by the lips, which are vibrating themselves, so that they close and open the mouth, and the current of the air coming in small pulses into the instrument, the sound is strengthened by its resonance.

So you see that the manner in which such vibrations of the air are produced is very different, and, indeed, the form of the motion of the air itself which is produced by these different musical instruments, is itself very different. For explaining these facts which I have brought before you we have a very convenient instrument called a siren. A siren in its simple form is composed of a brass box into which air is blown from beneath. The covering of this box is perforated by a series of holes; and above this covering a brass disc is rotating, which has a corresponding series of holes. [In explanation, the Lecturer exhibited two cardboard discs perforated with corresponding holes at regular distances; so that when the one disc rotated the holes in both would be alternately opened and closed.] If you move the upper disc slightly the box will be closed and the current of air passing out is interrupted; moving it a little further you have again the opening. If the motion is very swift we can have a great number of interruptions to the current of air during a very small time, by

which it is divided into single pulses which give the vibrations to the outer air,—these are transported to the ear, and you hear the sound. Now I will use a more complicated instrument. In this one [pointing] the disc and the covering of the box have four series of holes; the interior series has eight openings, and the exterior one has sixteen openings. Then you see that if the disc turns, in a given time the interior series or holes will give eight vibrations to the air during the time of one revolution of the disc, and the exterior series of holes will give sixteen revolutions, and therefore will give the higher octave. And you see that in this case the number of pulses which the air receives from the instrument are fixed by the number of the holes, so that the proportion between the sounds which correspond to the different series of these holes, corresponds exactly and accurately to whole numbers. In this more complicated instrument I have two such boxes, and two discs. You cannot see the discs as you have seen in the other instrument, because I have put some resounding boxes above the discs that they may give a better quality of sound. There is another peculiarity in the construction of this instrument. I can turn by this axis [pointing] the upper box so that it rotates itself. And by this rotation the number of the pulses of air can be altered a little. You see if the disc turns in one direction and the box itself turns in the opposite direction, then because the holes go in an opposite direction they will reach each other more swiftly than they do if the box is reposing. And if I move the box in the same direction as the discs are revolving they will not reach each other with quite so much rapidity. So I can get small alterations of the tone, and can alter the pitch of the higher box a little. [The Lecturer proceeded to make the experiment; the air being forced into the siren by a pair of bellows.]

I make this experiment principally to show you that the consonant intervals correspond quite accurately to these simple proportions of the vibrations. I can alter the pitch; I can get consonant tones by the apparatus, and the proportion of the whole numbers is kept quite accurately as long as our box is standing. As soon as I move it the pitch is altered, and you will hear the alteration of the consonance. [Experiment.] You hear that the sounds become higher and higher as the velocity of the revolving discs is increased by the effort of the current of air. Now, opening the series of twelve holes, we get a fundamental note. Opening in the same box the series of eighteen holes, we get the fifth, a perfect fifth going on quite continuously and without roughness. Taking the fundamental note again, and shifting the box in the manner I have described, you can perceive the alteration in tone, higher and lower, by this means I can alter the tone in a very small degree. The proportion between the sounds is given and fixed by the number of holes in these boxes which number cannot be altered. You heard the sound become higher at first by the increased velocity of the disc and by the increased number of vibrations. Then I obtained the pure and actual consonants by these intervals, and you saw that if I moved the upper box so that I altered the higher tone by a very small amount, then the consonance was damaged, it was neither so good nor so beautiful as before.

Now, with regard to the differences of the sounds, the first thing we have to speak on is the intensity of the sound. The intensity of the sound depends on the amplitude of the vibrations. If we cause a string or any elastic body to vibrate slowly, we have sound of small intensity; if we make the vibrations more rapid, then we have a sound of greater intensity. Therefore, the amplitude of the vibrations corresponds with the intensity of the sound. Then we have to distinguish the pitch of the sound. I have just said that the pitch depends only on the number of the vibrations made during a certain time, and we express it by the number of vibrations made during one second. The lowest sounds are produced by the fewest

vibrations, and the lowest number of vibrations which we can hear as real musical sounds, is about thirteen or fourteen in the second. It was erroneous, as stated by some one, that sound could be produced by eight vibrations in the second, and that it was wrong as afterwards shown by Despréts. Those tones of thirty vibrations have no good musical body, one begins to hear the single beats. Good musical sound is only produced by forty vibrations in the second. This corresponds to the lowest *F* on the piano. Those of you who play that instrument will know that the lowest notes are too low to give the ear the good impression of a musical sound. This depends not upon the construction of the instrument; it is the same with the lowest sounds of organs and all other instruments. The makers only go down to the sound of forty vibrations on the violoncello and basso. We can go to an extremely high number of vibrations to produce high tones. In the scale which we commonly use for music, as for example, the compass of the piano-forte, the highest note corresponds to nearly 4000 vibrations. But we can get higher sounds still by small tuning-forks, and Despréts found out that he could get sounds of 30,000 vibrations or more by their use. But if you go only a small interval beyond the limits of the piano, then these sounds become very disagreeable and their musical pitch is not very distinct. You can hear that they become higher and higher, and it is very difficult to say whether they become higher by an octave or a fifth and so on, and therefore they cannot well be used in music.

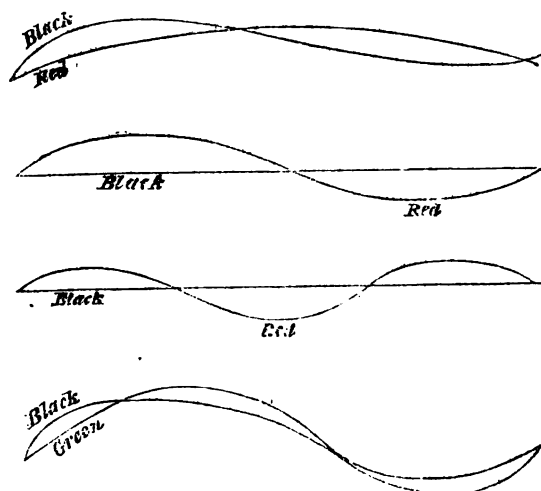
The next difference is in the *quantity* of sound. You know that the same note formed by the violin or the trumpet or the human voice has quite a different quality of sound. The French call this the *timbre*. Now, this difference of the quality must depend on the form of the vibrations. We have seen that the amplitude of the vibrations corresponds to the intensity of the sound; that the number of the vibrations corresponds to the pitch; therefore there remains nothing else than the form of the vibrations for explaining the different quality of sounds.

And, indeed, the form of the vibrations can be extremely different even if we take such a simple instrument as a single string. For showing this I have fastened before the black board a string, the motions of which are the same as those of a musical string, only slower; and I will show you the different forms of motion which such a string can produce. At first I can move the string in such a way that every point of it moves according to the same law, moves in the same way as the point of the pendulum would move if the pendulum was put into vibration. This motion [causing the string to vibrate] would give a very soft and simple sound. Then I can strike the string in the same way as it is done in the piano, and you perceive the wave runs up and down along the string. In giving it the motion it would have in the guitar or in the harp, I draw it to the side and let it go when you see the motion of the waves. Then I can put the string in another kind of motion, which is the motion it receives on a violin. There, one end of the string is going slowly across, and it falls down with great velocity, and the other end of the string goes down slowly and jumps up swiftly.

We know that if we excite the same string by striking against it, or by drawing it up with the finger or with the violin bow, we have always the same musical sound; but the sound is different as to its quality. Therefore, we see that it is necessary to consider the form of motion given to elastic vibrating bodies to find out on what the quality of the sound depends. That is the main subject on which we have to treat. Now, the number of forms in which a string can vibrate is infinitely great; but however complicated they are, we can consider them as a sum of motions of a very similar kind. Mathematicians distinguish a sort of vibration which they call simple vibrations, or you may also call them pendulum vibrations, because every point

of the string moves according to the same law which rules the motion of the pendulum. Such a string as we have here can execute very different pendulum or simple vibrations. Vibrations in which every point of the string does not move according to the law of the pendulum we may call complex vibrations. [The Lecturer proceeded to show the different forms of simple and complex vibrations that could be produced from a single slack wire.] If I touch any point of the monochord string with the hairs of a painting-brush, that point is obliged to repose; and when I strike the string, the sound beyond that point ceases, and is abolished by the friction of the hairs. The number of vibrations of these simple sounds of the string is very different. You saw that I could bring the string into different forms of vibration by altering the mode of striking it and the position in which it was struck. When the string is divided into two parts it has twice as many vibrations as before, when it is divided into three parts it has three times as many, and so on. And in this way we get a number of sounds which can be given by the same string. If I touch one part of the string with the brush and then strike it, you hear the sound, and I would have you observe that I can take away the brush after I have excited the string without altering the sound. Now, such vibrations of the string can be compound; one string can execute two such systems of motions in the same time. As, for instance, I can move this string as before, so that it makes simple vibrations, and whilst it is doing so I can strike it in the centre and it will have the up and down motion combined with the first swinging motion. In this way I can combine two systems of simple vibrations. You may compare the motions of the string with the waves on the surface of water. If you look down on to the sea from any high point of view you will commonly see long systems of straight waves coming on from the horizon to the shore; then they are reflected from the shore, and go back into another direction. You may throw a stone into the water, and you will have the circular waves going out from the point where the stone fell down and spread over the wavy surface, quite in the same way as they would spread on the plain surface of reposing water. If you consider this, you see that the circular waves are added to the wavy surface in the same way as to the plain surface. Every point of surface, indeed, becomes as high as the same circular movement of the same plain waves would make, and if you consider the points between the waves as the level, then you may see that the height of every point of the surface above the level is like the sum of the heights, which would be brought on by the single systems of waves. Now, in the same way the vibrations can be composed on the string. If we execute such different kinds of such composition of vibrations then we can bring forth and produce every possible form of vibration. And every form of vibration of the string which is possible can be composed by such simple vibrations only in one way. I have a diagram here to explain this. The first waves are such forms of the string as would be made by simple vibrations. Beneath you have the second simple vibration given by the red line. If you put all the heights of this curve above the other you get the black curve there above which is composed. To that black curve, which is represented in the lowest diagram as a green line, the third vibration is added, and you see the black line is as high above the green line as the red line is above the black line in the second diagram. In this way we get the second black curve, and by putting together those vibrations we get the red at the bottom. The vibrations can also be shown by the aid of the electric light. Here the electric light is reflected by a little mirror on one of the points of the branches of this tuning-fork [pointing] on to the screen. If the tuning-fork is put into vibration then its vibrations are reflected on to the screen, but in a very increased manner. [The Lecturer proceeded to show the vibrations by the aid of the electric light and

be screen. He first showed the vibrations of a large tuning-fork producing fewer vibrations, and then of one



an octave higher. He then reflected the vibrations from both tuning-forks at the same time producing a composed system of vibrations.] There are a great variety of forms, and if we take a number of simple oscillations it is possible to get every possible form of composed vibrations.

Now, we have seen that a great number of simple vibrations can exist together on the surface of water. They can exist together in the same way in a room filled with air; as, for instance, if you take a room where a dancing party goes on, there you have the sounds of instruments, the sounds of voices, the rustling of dresses, and so on. You have a great number of sounds which are confused altogether in the same mass of air, but you can perceive them all separately. Even by your ear you can perceive that all these sounds co-exist at the same time in the same place. Now, the motion of the air in such a room must be a very complicated one. You must suppose that from the mouths of the gentlemen there are going out waves of the length of four to eight feet, and from the mouths of the ladies waves of the length of two to four feet. Then the rustling of the dresses and silk gives a little crisping in the air, like the crisping on the water produced by the wind blowing on its surface. These air-waves are reflected by the walls, return and pass through each other, and although the whole system is so complicated, the human ear can distinguish all the single sounds which are combined. This is a very wonderful fact; but more so because the ear distinguishes the sounds under difficult circumstances. If you look down upon the water you see the whole surface at the same time,—you overlook the whole of the system,—and you can see the motion and compare the motion of all parts of the surface. But if you hear the sounds in a room you can perceive only the motion of that air which is next to your ear, you perceive only the motion of a single point in the whole mass. It would correspond to what would happen if, instead of looking on the water itself, you put a stick on the surface, so that the stick would be moved, and only watched the motions of the stick. If you did that, I do not believe you would be able to separate them to the single systems of waves which go and come to this point. Therefore this separation of different undulations is far more difficult to the ear than the same is to the eye. Now, we have seen that the ear separates such a mass of sound co-existing in the room, which is really composed by different voices and instruments. But if it composes such a mass of sound, it must also analyse sounds which are not composed, from their original source; because every quantity can be considered

as the sum of parts. The number five can be considered as the sum of three and two, or as the sum of three and one and one, and so on. So, as there exists such a number of sounds produced really by different instruments, the ear can also decompose sounds into their simple elements. So, for instance, we have seen the motion of the string can be considered as a composition of different oscillations. We can distinguish these simple oscillations also by the ear. If you listen, I will first touch the string with the brush, so that we have only the second harmonic of the string. If I cause the fundamental sound to come out, you will hear the harmonic and the fundamental sound, and at last you will have the natural sound of the string. So much about the compositions of the musical sounds. In the next Lecture I will try to make the application.

CORRESPONDENCE.

Adulterated Milk of Sulphur.

To the Editor of the CHEMICAL NEWS.

SIR,—A friend of mine recently handed over to me a small quantity of Milk of Sulphur which he more than suspected had been "doctored."

I submitted it to heat in a covered crucible, and was surprised to find that only 48.92 per cent. of sulphur was driven off, leaving 51.08 per cent. of a white pulverulent substance, which, on analysis, proved to be sulphate of lime. An instance of this sort needs no comment, and the fact that it was sold at 1½d. per oz. and only costs to the shopkeepers about 3d. per lb. does not improve it.

By publishing this, you may lead people to be on the look out when purchasing it for the future.—I am, &c.

W. CROSLBY, Analytical Chemist.

Ormesby Iron Works, Middlesbrough on Tees.

P.S. I send you a sample in the very paper in which I received it.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Action of Sulphurous Acid on Metals and Metallic Oxides at High Temperatures.—Schiff records (*Annal. der Chem. und Pharm.*, bd. cxvii. s. 392) the following results of his experiments with sulphurous acid and some of the metals and metallic oxides at high temperatures:—

Cupric oxide when heated in the dry acid is only partially changed to cuprous oxide; a third of it passes into sulphate of cupric oxide: $3\text{Cu}_2\text{O}_2 + \text{S}_2\text{O}_4 = \text{S}_2\text{Cu}_2\text{O}_8 + 2\text{Cu}_2\text{O}$.

Zinc Oxide undergoes no alteration.

Lead Oxide.—The greater part is changed into sulphide, and some into lead sulphate. Quantitative analysis of the products gave widely different results. The carbonate of lead behaves like the oxide.

Tungstic acid could only be reduced to the blue compound of tungstic acid and tungstic oxide.

Iron (finely divided) undergoes combustion more or less actively even at a low temperature, and forms sulphide and a small amount of sulphate of the protoxide.

Lead (precipitated from the acetate by means of zinc) does not undergo combustion, but by longer action is changed mostly into the sulphide.

Tin (reduced from the chloride by zinc) undergoes active combustion below the melting-point, and the metal is changed into a yellowish mass, which is a mixture of stannic oxide and bisulphide. If this product is heated in the sulphurous acid until the stannic oxide becomes insoluble in dilute acid, the bisulphide may be extracted.

Antimony is attacked with difficulty: it is converted to the red amorphous sulphide.

Arsenicum is only acted on by sulphurous acid when in the form of vapour, and then but slowly. Two sublimate are obtained; that nearest to the heat is in most part sulphide of arsenic, the furthest is arsenious acid. Both have some sublimed arsenicum with them.

Metallic copper, mercury, and bismuth undergo no change.

Potassium burns rapidly when heated in sulphurous acid, and leaves a yellow residuum which contains little or no sulphurous acid, but consists of sulphate, hypsulphite, and polysulphuret of potassium—the last being the principal ingredient.

II. ORGANIC CHEMISTRY.

Compounds of Sesquistannethyl.—These—a very short notice of which, by Cahours, appeared in the CHEMICAL NEWS, Vol. i. p. 157,—have been further investigated by Kulmiz (*Journal für Prakt. Chem.*, bd. lxxx. p. 60), who describes more minutely the salts of this radical.

Chloride of Sesquistannethyl is prepared from the sulphate by the addition of the chloride of barium; the bromide by the action of hydrobromic acid on the hydrated oxide of sesquistannethyl. The iodide may be prepared either by acting on the sulphate of sesquistannethyl by iodide of potassium, or by treating an alcoholic solution of the oxide with iodine. It forms a clear mobile liquid, which has a very irritating odour; it boils between 180° and 200°, and has the specific gravity 1.850.

Sulphide of Sesquistannethyl.—Sulphuretted hydrogen is passed to excess through one-half of an alcoholic solution of oxide of sesquistannethyl; the excess of HS is then driven off by heat, and the other half of the solution is added. At the end of twenty-four hours a heavy oil deposits, which contains an amount of sulphur corresponding to the formula $\text{Sn}_2(\text{C}_4\text{H}_5)_3\text{S}$. When the alcoholic solution of the oxide of sesquistannethyl is saturated with HS, a crystalline mass is formed, which probably contains hydrosulphuret or a higher sulphide. The liquor decanted from this, deposits other crystals having a repulsive odour somewhat resembling that of mercaptan.

Carbonate of Sesquistannethyl.—The alcoholic solution of the hydrated oxide of sesquistannethyl, when evaporated slowly in the air, absorbs carbonic acid and deposits small brilliant crystals of the carbonate. It can be made more easily by the double decomposition of the sulphate of the radical and carbonate of ammonia, and then is obtained in the form of a white powder, which crystallizes from an ethereal solution in oblique prisms.

Phosphate of Sesquistannethyl is formed by the action of phosphoric acid on the carbonate. It is a crystalline powder soluble in dilute alcohol. The arseniate may be prepared in the same way. The iodate and bromate are deposited in small crystals when the iodide or bromide is prepared by the action of iodine or bromine on the oxide of sesquistannethyl. The author has also prepared the formate, acetate, butyrate, and oxalate of sesquistannethyl. Two tartrates are obtained by the action of tartaric acid on the carbonate,—one a bibasic salt which crystallizes in cubes, the other an acid salt which separates in rhombic crystals after the former has deposited. The chloride of sesquistannethyl forms double salts with the chlorides of platinum, gold, and mercury. With the chloride of platinum it forms two compounds: the first is obtained by beautiful crystals of a deep red colour, and has the formula $\text{Sn}_2(\text{C}_4\text{H}_5)_3\text{Cl} \cdot 2\text{PtCl}_2$; the solution which yielded this, afterwards deposits yellow crystals, which have a disagreeable odour, and the composition $\text{Sn}_2(\text{C}_4\text{H}_5)_3\text{Cl} \cdot \text{PtCl}_2$.

Cyanate of Sesquistannethyl is formed by the action of cyanate of silver on the iodide in alcoholic solution. The action of ammonia on an alcoholic solution of the cyanate

forms crystals of a body which the author regards as urea, in which an atom of hydrogen is replaced by sesquistanethyl. The crystals contain $N_2 \cdot H_3 [Sn_2 (C_4 H_5)_3] C_2 O_3$. Kulmiz has also formed acicular crystals of what he supposes to be the iodide of sesquistanethyl-ammonium, $N [Sn_2 (C_4 H_5)_3] H_3 I$, by heating in a sealed tube an alcoholic solution of the iodide saturated with ammonia.

MISCELLANEOUS.

INTERNATIONAL EXHIBITION OF 1862.

AMENDED AND ADDITIONAL DECISIONS OF HER MAJESTY'S COMMISSIONERS ON POINTS RELATING TO THE EXHIBITION.

April, 1861.

1. Her Majesty's Commissioners have fixed upon Thursday, the 1st day of May, 1862, for opening the Exhibition.
2. The Exhibition building will be erected, on a site adjoining the gardens of the Royal Horticultural Society, and in the immediate neighbourhood of the ground occupied in 1851, on the occasion of the first International Exhibition.

3. The portion of the building to be devoted to the exhibition of Pictures will be erected in brick, and will occupy the entire front towards Cromwell Road; the portion in which Machinery will be exhibited will extend along Prince Albert's Road, on the west side of the gardens.

4. All works of industry to be exhibited should have been produced since 1850. The decision whether goods, proposed to be exhibited, are admissible or not, must, in each case, eventually rest with Her Majesty's Commissioners.

5. Subject to the necessary limitation of space, all persons, whether designers, inventors, manufacturers, or producers of articles will be allowed to exhibit; but they must state the character in which they do so.

6. Her Majesty's Commissioners will communicate with Foreign and Colonial exhibitors only through the Commission which the Government of each Foreign Country or Colony may appoint for that purpose; and no article will be admitted from any Foreign Country or Colony without the sanction of such Commission.

7. No rent will be charged to exhibitors.

8. Every article produced or obtained by human industry, whether of

Raw materials,
Machinery,
Manufactures, or
Fine Arts,

will be admitted to the Exhibition, with the exception of,

1. Living animals and plants.

2. Fresh vegetable and animal substances, liable to spoil by keeping.

3. Detonating or dangerous substances.

Copper caps, or other articles of a similar nature, may be exhibited, provided the detonating powder be not inserted; also Lucifer Matches, with imitation tops.

9. Spirits or alcohols, oils, acids, corrosive salts, and substances of a highly inflammable nature, will only be admitted by special written permission, and in well-secured glass vessels.

10. The articles exhibited will be divided into the following classes:—

SECTION I.

Class 1. Mining, Metallurgy, and Mineral Products.

„ 2. Chemical Substances and Products, and Pharmaceutical Processes.

„ 3. Substances used for Food, including Wines.

„ 4. Animal and Vegetable Substances used in Manufactures.

SECTION II.

Class 5. Railway Plant, including Locomotive Engines and Carriages.

„ 6. Carriages not connected with Rail or Tram Roads.

„ 7. Manufacturing Machines and Tools.

„ 8. Machinery in general.

„ 9. Agricultural and Horticultural Machines and Implements.

„ 10. Civil Engineering, Architectural, and Building Contrivances.

„ 11. Military Engineering, Armour and Accoutrements, Ordnance, and Small Arms.

„ 12. Naval Architecture, Ship's Tackle.

„ 13. Philosophical Instruments and Processes depending upon their use.

„ 14. Photographic Apparatus and Photography.

„ 15. Horological Instruments.

„ 16. Musical Instruments.

„ 17. Surgical Instruments and Appliances.

SECTION III.

Class 18. Cotton.

„ 19. Flax and Hemp.

„ 20. Silk and Velvet.

„ 21. Woollen and Worsted, including Mixed Fabrics generally.

„ 22. Carpets.

„ 23. Woven, Spun, Felted, and Laid Fabrics, when shown as specimens of Printing or Dyeing.

„ 24. Tapestry, Lace, and Embroidery.

„ 25. Skins, Fur, Feathers, and Hair.

„ 26. Leather, including Saddlery and Harness.

„ 27. Articles of Clothing.

„ 28. Paper, Stationery, Printing, and Bookbinding.

„ 29. Educational Works and Appliances.

„ 30. Furniture and Upholstery, including Paperhangings, and Papier-maché.

„ 31. Iron and General Hardware.

„ 32. Steel and Cutlery.

„ 33. Works in Precious Metals, and their imitations, and Jewellery.

„ 34. Glass.

„ 35. Pottery.

„ 36. Manufactures not included in previous classes.

SECTION IV.—MODERN FINE ARTS.

Class 37. Architecture.

„ 38. Paintings in Oil and Water Colours, and Drawings.

„ 39. Sculpture, Models, Die-sinking, and Intaglios.

„ 40. Etchings and Engravings.

11. Prizes, or rewards for merit, in the form of medals, will be given in Sections I. II. III.

12. Prices may be affixed to the articles exhibited in Sections I. II. III.

13. Her Majesty's Commissioners will be prepared to receive all articles which may be sent to them, on or after Wednesday the 12th of February, and will continue to receive goods until Monday, the 31st of March, 1862, inclusive.

14. Articles of great size or weight, the placing of which will require considerable labour, must be sent before Saturday, the 1st of March, 1862; and manufacturers wishing to exhibit machinery, or other objects that will require foundations or special constructions, must make a declaration to that effect on their demands for space.

15. Any exhibitor whose goods can be properly placed together, will be at liberty to arrange such goods in his own way, provided his arrangement is compatible with the general scheme of the Exhibition and the convenience of other exhibitors.

16. Where it is desired to exhibit processes of manufacture, a sufficient number of articles, however dissimilar, will be admitted for the purpose of illustrating the pro-

cess; but they must not exceed the number actually required.

26. Exhibitors will be required to deliver their goods at such part of the building as shall be indicated to them, with the freight, carriage, portage, and all charges and dues upon them paid.

27. The vans will be unloaded, and the articles and packages taken to the places appointed in the building, by the officers of Her Majesty's Commissioners.

28. Upon receipt of notice from Her Majesty's Commissioners, that the articles are deposited in the building, exhibitors, or their representatives, or agents, must themselves unpack, put together, and arrange their goods.

29. Packing cases must be removed at the cost of the exhibitors or their agents, as soon as their goods are examined and deposited in charge of the Commissioners. If not removed within three days of notice being given, they will be disposed of, and the proceeds, if any, applied to the funds of the Exhibition.

35. No counters, or fittings, will be provided by Her Majesty's Commissioners. Exhibitors will be permitted, subject only to the necessary general regulations, to erect, according to their own taste, all the counters, stands, glass frames, brackets, awnings, hangings, or similar contrivances which they may consider best calculated for the display of their goods.

36. Exhibitors, or their representatives, should provide whatever light temporary covering may be requisite (such as sheets of oiled calico), to protect their goods from dust; and, in the case of machinery and polished goods, should make the requisite arrangements for keeping the articles free from rust during the time of the Exhibition.

43. Exhibitors must be at the charge of insuring their own goods, should they desire this security. Every precaution will be taken to prevent fire, theft, or other losses, and Her Majesty's Commissioners will give all the aid in their power for the legal prosecution of any persons guilty of robbery or wilful injury in the Exhibition, but they will not be responsible for losses or damages of any kind which may be occasioned by fire or theft, or in any other manner.

44. Exhibitors may employ assistants (male or female) to keep in order the articles they exhibit, or to explain them to visitors, after obtaining written permission from Her Majesty's Commissioners; but such assistants will be forbidden to invite visitors to purchase the goods of their employers.

50. Articles once deposited in the Building will not be permitted to be removed without written permission from Her Majesty's Commissioners.

55. Her Majesty's Commissioners will provide shafting, steam (not exceeding 30 lbs. per inch), and water, at high pressure, for machines in motion.

56. Persons who may wish to exhibit Machines, or trains of Machinery, in motion, will be allowed to have them worked, as far as practicable, under their own superintendence, and by their own men.

70. Intending exhibitors in the United Kingdom are requested to apply, without delay, to the Secretary to Her Majesty's Commissioners, for a *Form of Demand for Space*, stating, at the same time, in which of the four Sections they wish to exhibit.

100. Foreign and Colonial exhibitors should apply to the Commission, or other Central Authority appointed by the Foreign or Colonial Government, as soon as notice has been given of its appointment.

101. Her Majesty's Commissioners will consider that to be the Central Authority in each case which is stated to be so by the Government of its country, and will only communicate with Exhibitors through such Central Body.

102. No articles of foreign manufacture, to whomsoever they may belong, or whosoever they may be, can be admitted for exhibition, *except with the sanction of the Central Authority of the country of which they are the pro-*

duce. Her Majesty's Commissioners will communicate to such Central Authority the amount of space which can be allowed to the productions of the country for which it acts, and will also state the further conditions and limitations which may from time to time be decided on with respect to the admission of articles. All articles forwarded by such Central Authority will be admitted, provided they do not require a greater aggregate amount of space than that assigned to the country from which they come; and, provided also, that they do not violate the general conditions and limitations. It will rest with the Central Authority in each country to decide upon the merits of the several articles presented for exhibition, and to take care that those which are sent are such as fairly represent the industry of their fellow-countrymen.

103. Separate space will be allotted to each foreign country, within which the Commissioners for that country will be at liberty to arrange the productions entrusted to them in such manner as they think best, subject to the condition that all machinery shall be exhibited in the portion of the building specially devoted to that purpose, and all pictures in the fine art galleries, and to the observance of any general rules that may be laid down by Her Majesty's Commissioners for public convenience.

104. By arrangements made with Her Majesty's Government, all Foreign or Colonial goods intended for exhibition, sent and addressed in accordance with regulations hereafter to be issued, will be admitted into the country, and transmitted to the Exhibition building without being previously opened, and without payment of any duty. But all goods which shall not be re-exported at the termination of the Exhibition will be charged with the proper duties, under the ordinary Customs' Regulations.

109. It is not the intention of Her Majesty's Commissioners to take any steps in reference to the protection of inventions or designs, by patent or registration, the law on these points having been materially simplified since 1851.

ANSWERS TO CORRESPONDENTS.

* * In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

* * All Editorial Communications are to be addressed to Mr. CROOKER, and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

VOL. II. of the *CHEMICAL NEWS*, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

O. A. P.—The article you refer to was given three weeks ago in our pages.

W. H. W.—The diagrams will be given. Chevreul's "Circles Chromatiques" can be obtained at Baillière's, in Regent Street.

F. R. S. A.—Hypochlorous acid is best prepared by passing well-washed and dry chlorine gas over-precipitated and dried red oxide of mercury in a glass tube. The tube must be kept cool, and the gas passed slowly, when chloride of mercury and hypochlorous acid are produced; the latter is to be collected by displacement. Precipitated oxide of mercury must be used, as the crystalline oxide will not answer.

W. P.—We will try and give you the desired information next week.

A Lover of Science.—1. Use benzol instead of camphine. The whole of the gum will not dissolve; the solution must be strained from the residue. 2. Chloride of lead is precipitated as a white powder upon the addition of chloride of sodium to acetate of lead—both in solution. 3. Aldehyde is prepared by carefully distilling 6 parts of oil of vitriol, 4 parts of rectified spirits of wine, and 4 parts of water, with 6 parts of powdered binoxide of manganese. Distil at a gentle heat till 6 parts have come over. Collect in a receiver cooled with ice.

THE CHEMICAL NEWS.

VOL. III. No. 75.—May 11, 1861.

WATER ANALYSIS.

STATEMENT OF THE RESULTS.

No branch of analytical chemistry is, perhaps, better defined or less liable to error than the methods usually employed to detect the presence, and determine the amounts respectively, of the several bases and acids likely to occur in the examination of a sample of spring water; but no sooner is an accurate estimate of the individual constituents arrived at, than difficulties are experienced in drawing up a statement of the results which shall correctly represent the mode of combination and arrangement of the several saline constituents held together in solution by the water. Much diversity of opinion appears yet to exist with regard to the system to be adopted in these cases; some Analysts following the recommendation of Fresenius, whilst others, working according to systems peculiarly their own, have arrived certainly at very anomalous conclusions in reference to the co-existence of salts in solution.

We remember to have heard, many years since, an anecdote bearing so directly on the subject in hand that we may be excused for mentioning it here. It is related of one of our early but distinguished Chemists, who, on reporting the analysis of an ordinary spring water sent to him for examination, had combined the sulphuric acid with the magnesia, sodium with chlorine, lime with carbonic acid, &c., and thus represented the water to contain not only common salt and chalk, but also a large proportion of Epsom salt; whereupon his client became alarmed at the highly medicinal character of the spring, which, in his neighbourhood, had acquired some celebrity as an agreeable drinking water. This led to a reconsideration of the analytical results, and it was found that by substituting *chloride* of magnesium for the objectionable sulphate, and gypsum for chalk, that the amended report became then completely satisfactory to all parties.

A recent example of discrepancy in stating the results of the chemical examination of samples of water from the same locality is presented in the April issue of the *Quarterly Journal of the Chemical Society*. The saline water of Purton, near Swindon, has been analysed independently by Dr. Noad and by Dr. Voelcker, of Cirencester. Their results were communicated to the Chemical Society on the same evening, and the tabulated statements are brought together on contiguous pages of the Society's *Journal* and in the columns of the *CHEMICAL NEWS*.¹ It will be seen that there are many points of general agreement in these analyses, but there is also a notable discrepancy as regards the state of combination of the lime. Dr. Noad advances experiments to prove that nearly half the amount of lime is present in the water as carbonate, since this proportion is precipitated on boiling. Dr. Voelcker maintains, on the contrary, that *potash* is present as carbonate, and the lime *all* as sulphate. This latter supposition may not, perhaps, be

directly contradicted by the known reaction of carbonate of potash upon sulphate of lime, but involves at least the necessity of assuming this to be the more probable state of combination. In regard also to that frequent constituent of saline waters, chloride of magnesium, Dr. Noad's analysis exhibits a fair proportion, but in Dr. Voelcker's Table this item is altogether unrepresented.

What has been stated above in reference to the satisfactory nature of the tests for inorganic constituents in water does not, unfortunately, equally apply to the determination of organic matter. It were much to be desired that better methods could be found for determining with greater accuracy the amount of dissolved organic impurities contained in samples of water. The process usually followed is difficult of execution, and takes no account of the particular *kind* of organic matter which may be present. Whether peaty or nitrogenous in its nature, harmless or injurious, all come under the general category "organic matter." Neither can it be said that the division into *crenic* and *apocrenic* acids has, since the first introduction of these names, served to convey any clearer idea of the nature of these organic bodies.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Chemical Researches on Combustible Minerals,
by M. E. FREMY.

MY long-pursued studies on vegetable tissues, of which the Academy already knows the principal results, have naturally led me to wish to determine the chemical characteristics of the combustible minerals, and to try to discover whether their constituent materials present any analogy with those forming unaltered vegetable tissues.

Admitting, as do all geologists, that turf, lignite, coal, and anthracite, are formed under different circumstances, and that they appertain to the strata of different epochs, my desire is to discover in these combustibles the degree of alteration of the organic tissue.

In studying turf, I have discovered no really new fact. By the side of the unaltered elementary organs, found in such large quantities in fibrous turf, I have found, according to the degree of alteration the combustible has undergone, various proportions of those brown compounds, neutral or acid, nitrogenised or not, which, in our ignorance, we designate by the general term of ulmic compounds. The presence of these bodies, which M. Payen has already investigated, establishes a very clear distinction between turfs and unaltered organic tissues.

The chemical examination of lignites was likely to prove more interesting. In this examination I have taken care to distinguish those specimens still presenting

¹ See *CHEMICAL NEWS*, Vol. III. p. 77.

a ligneous organisation from those which have the appearance and compactness of coal. The former constitute the xyloid lignite, or fossil-wood; the latter, compact and perfect lignite. Regarding their chemical characteristics, all the varieties of lignite I have examined are included in the two preceding species.

Although the fossil-wood lignite has often the tenacity and appearance of ordinary wood, I have come to the conclusion that in this combustible the ligneous tissue undergoes a great change; trituration reduces it to fine powder; submitted to the action of a weak solution of potash, it yields to the alkali a considerable quantity of ulmic acid.

The following two reactions establish a very marked difference between ordinary wood and fossil-wood lignite:—

When nitric acid, by the aid of heat, reacts on wood, it dissolves a portion only of its fibres and medullary rays, and leaves untouched the cellulose matter, which dissolves without colouration in concentrated sulphuric acid, and possesses all the properties ascribed to it by M. Payen.

Under the same circumstances, fossil-wood lignite is attacked with great energy and completely transformed into a yellow resin, soluble in alkalies and in excess of nitric acid.

By submitting wood and fossil-wood lignite comparatively to the action of hypochlorites there is a marked difference in their effects on the two substances.

Hypochlorites re-act on wood in a manner which may be compared to that of nitric acid. They dissolve rapidly a portion of the fibres and medullary rays, leaving the cellulose untouched.

Alkaline hypochlorites attack fossil-wood lignite, almost entirely dissolving it, leaving only imponderable colourless traces of the fibres and medullary rays.

It results, then, from the preceding facts, that when ligneous tissues arrive at that state of modification constituting fossil-wood lignite, and while preserving the appearance of wood, their substance has undergone a very material modification, and then contains new proximate principles, characterised by their perfect solubility in nitric acid and in hypochlorites.

After determining the chemical characteristics of fossil-wood lignite, it becomes interesting to ascertain whether the compact lignite, in which the texture of the ligneous tissues is no longer observable, and which is black and brilliant like coal, to which it bears so much analogy as to mystify the most experienced, preserves the chemical characteristics of fossil-wood lignite or whether it resembles coal.

In a geological point of view, the comparative study of fossil-wood lignite, of compact lignite, and of coal appears to me to be also of great importance. If, in fact, there is a positive connection between the state of alteration of combustible minerals and the age of the strata containing them, it becomes apparent how much geology would benefit by possessing a chemical character, by means of which it would be possible to determine accurately the extent of the modification of an organic body, and of determining the age of the stratum by the state of alteration of the combustible mineral found therein. Therefore, I have applied myself to find a series of chemical re-agents acting differently on combustibles, minerals, and to arrange their varieties conformably to the degree of their modifying action and the chemical characters they would thus present. The re-agents I have employed are potash, hypochlo-

rites, sulphuric and nitric acids. I also take into account the excellent characteristic given by M. Cordier.

I have previously shown that it is impossible to confound ligneous tissue with fossil-wood lignite, the latter being soluble in hypochlorites and nitric acid. Compact lignite presenting no longer the appearance of organisation could be confounded only with certain varieties of coal. The mode of combustion, the reaction of the volatile products on litmus, and the colour of the dust, constitute already distinctive and very important characteristics. To this subject chemical re-agents will give a yet greater degree of certainty. When compact lignite is submitted to the action of concentrated potash, the liquid sometimes becomes coloured brown, dissolving a small quantity of ulmic acid; but alkaline liquid does not generally re-act on the combustible. Thus a distinction is immediately established between fossil-wood and compact lignite.

I have always observed that the lignites which resist the action of potash are those which, by their bearing, approach nearest to the coal series.

Compact lignites, black and brilliant like coal, dissolve entirely in alkaline hypochlorites, are rapidly attacked by nitric acid, and produce the yellow resin which I have already mentioned while treating of fossil-wood lignite.

The two characters just indicated prevent, then, the confounding lignites with coals. Coal, in fact, is not dissolved by hypochlorites, and is attacked but feebly by nitric acid. I have submitted to the test of the hypochlorites almost all the important coals belonging to different strata, and have found that these combustibles always resist the action of these chemical re-agents. To me this characteristic appears so valuable that I think were a sample of coal met with slightly attackable by hypochlorites, it would be advisable to examine whether the combustible possessing this exceptional property were really coal; for it may well be imagined that in coal-fields there may exist vegetable matters unequally decomposed.

Coal and anthracite which resist the action of alkaline and hypochlorite solutions dissolve completely in a mixture of monohydrated sulphuric acid and nitric acid; the liquid takes a very deep brown colour, and holds in solution an ulmic compound, which water completely precipitates.

It is not my intention in this paper to consider the influences by which organic tissues are transformed into combustible minerals. I ought, however, here to put on record what seems to me an interesting observation. I have ascertained that ligneous tissue, exposed for several days to a temperature of 200°, undergoes successive modifications, and yields bodies very similar to those found in lignites. The first are soluble in alkalies, and correspond to fossil-wood lignite; the second are insoluble in alkalies, but dissolve entirely in hypochlorites, like compact lignite. These are the new facts which it is my wish to submit to the Academy. Their evident object is to introduce chemical characteristics into the study of combustible minerals, and they appear to me to lead to the following deductions:—

1. By treating combustible minerals by the above-mentioned re-agents, it is ascertained that in proportion to the age of the lignite so the chemical characteristics of the tissues gradually disappear, and the organic matter more resembles graphite the older the stratum whence it is derived. One exception I make in respect of the strata which have been modified under the influence of metamorphism. My researches accord completely with

those of M. Regnault, who has already arrived at the same conclusions as myself in his important researches on combustible minerals.

2. The first degree of modification of ligneous tissues represented by turf is characterised by the presence of ulmic acid, and also by the ligneous tissues or cellules of the medullary rays, which can be extracted and purified in considerable quantities by means of nitric acid or of hypochlorites.

3. The second degree of modification corresponds to fossil-wood, or xyloid lignite. It is partly soluble in alkali, like the preceding body; but it undergoes a greater alteration, for it dissolves almost entirely in nitric acid and hypochlorites.

4. The third state of modification is represented by compact or perfect lignite. Re-agents indicate in this substance a transition from organic matter to coal: thus, alkaline solutions have generally no effect on perfect lignite; this combustible is characterised by its complete solubility in hypochlorites and nitric acid.

5. The fourth degree of modification corresponds to coal, which is insoluble in alkaline solutions and in hypochlorites.

6. The fifth degree of alteration is anthracite, which is obviously allied to graphite, which resists the re-agents capable of modifying the preceding combustibles, and which is but very feebly attacked by nitric acid.

It is, then, evident that chemical reactions confirm the classification of the combustible minerals admitted by geologists. I am, however, far from thinking that lignite, coal, and anthracite, which are now characterised by their elementary composition and chemical reactions, constitute the only modifications which organic matters undergo while changing into combustible minerals. There doubtless are intermediate transformations of the organic tissues corresponding to the differences established by commercial practice between the different species of coal and lignites. But are the re-agents sufficiently sensitive to be capable of characterising these different varieties of the same combustible mineral in glance-coal or smith-coal, or even in the different layers of the same coal-bed? I shall investigate this question in another communication.—*Comptes-Rendus.*

On Some Applications of the Ferrocyanide of Potassium in Chemical Analysis, by EDMUND W. DAVY, A.B., M.B., M.R.I.A., Professor of Agriculture and Agricultural Chemistry to the Royal Dublin Society.¹

(Continued from page 276.)

Having made two standard solutions, the first containing 21.122 grammes of ferrocyanide of potassium in a litre of the solution, and the second 14.759 grammes of bichromate of potash in the same quantity of solution (weights which are to each other as their atomic equivalents), I made several estimations of chloride of lime with them, adopting the method I have just described, and found that it gave the most consistent results, and which agreed very closely with those obtained by Gay-Lussac's and Otto's methods when the latter were performed with the greatest care, the only difference being that the results obtained by my method indicated a few hundredths of a part more of chlorine than either of those methods did, which may be accounted for by the unavoidable loss of a minute quantity of chlorine which takes place in those processes.

¹ From the *Philosophical Magazine*, being part of a paper read before the Royal Dublin Society, December 17, 1860.

In order to simplify the process and render the calculation as short as possible, I would recommend for commercial valuations the following way of carrying out this principle:—Having obtained a flat-bottomed flask or bottle, which will contain 10,000 grains of distilled water when filled up to a certain mark in the neck, make two standard solutions, the first by placing in the flask or bottle 1190 (or exactly 1189.97²) grains of the purest crystallised ferrocyanide of potassium (yellow prussiate of potash), reduced to powder, adding distilled water to dissolve the salt, and when this is effected, filling up with water to the mark; and having mixed the solution thoroughly, place it in a well-stoppered bottle. The second standard solution is made in the same manner, substituting for the ferrocyanide 138.6 (or exactly 138.58) grains of bichromate of potash which has been purified by re-crystallisation and fused in a crucible at as low a heat as possible. Both these solutions will keep unchanged, and will answer for a number of determinations if they are preserved in well-stoppered bottles, and the ferrocyanide solution be kept, when not in use, excluded from the light. Get a burette or alkalimeter capable of holding or delivering 1000 grains of distilled water, and divided into 100 equal divisions; also two small bottles, one capable of delivering 1000 grains, and the other 500 grains of distilled water when filled up to a certain mark on the neck of each,³ which may both be readily made by filling them with water, emptying them, and, after they have drained for a minute or two, weighing into each the above weights of distilled water; or, what will be sufficiently accurate for most purposes, pour from the burette into one 100 divisions of distilled water, and into the other 50, and mark with a file where the fluid stands in the neck of each bottle. Having these all ready, take an average specimen of chloride of lime, and weigh out 100 grains of it, and make in the usual way a solution of it by trituration in a mortar with some water; pour it into the flask which was used in preparing the two standard solutions, and, having filled up with water to the mark in the neck, mix the solution thoroughly; and before each time that any of the chloride of lime is taken out, shake well the contents of the flask.

Measure out into a beaker-glass, by means of the two little bottles, 100 divisions of the chloride of lime solution, and 50 of the standard solution of ferrocyanide; and, having mixed them well together, add some hydrochloric acid to dissolve the precipitate formed and acidify the mixture strongly; and having mixed the whole well, pour from the burette slowly the standard solution of bichromate, stirring well all the while, till a drop taken from the mixture and brought in contact with a drop of a very weak solution of perchloride of iron produces a yellowish-brown colour, as already noticed. Then read off the number of divisions of the standard solution of bichromate which was necessary to produce this effect, and this being deducted from 50, gives the per-centage by weight of chlorine.

For the standard solution of ferrocyanide having been made so that the 10,000-grain measures should be equivalent to 100 grains of chlorine, and as every division of

² The above numbers are obtained as follows:—35.5 parts of chlorine are capable, as before stated, of converting 422.44 parts of the crystallised ferrocyanide of potassium into ferridcyanide; therefore, 100 parts of the former will convert 1189.97 parts of the latter into the same compound. Again, as before observed, 1267.32 parts of the crystallised ferrocyanide require 147.59 parts of the bichromate of potash to convert them into the ferridcyanide; 1189.97 parts, therefore, will take 138.58 parts of that salt to produce the same effect.

³ Two small pipettes capable of delivering the above quantities would be found still more convenient.

the burette equals 10 grains, each of these divisions of the ferrocyanide solution converted into ferridcyanide will indicate 0.1 grain of chlorine. Again, the 100 divisions of the solution of chloride of lime represent 10 grains of that substance, and we want to know how many divisions of the ferrocyanide solution its chlorine has converted into ferridcyanide. This is readily ascertained by the bichromate solution, which has been so graduated that each division represents a division of the ferrocyanide solution. So that to determine the percentage of chlorine, we have only to deduct, as before stated, the number of divisions of the bichromate solution employed from the 50 of the ferrocyanide solution, and the difference gives us the percentage of chlorine by weight in the sample; thus, in four experiments 50 divisions of the ferrocyanide solution, mixed with 100 divisions of the solution of chloride of lime, required 18.5 divisions of the bichromate solution to convert the whole of the ferrocyanide employed into ferridcyanide; this number, taken from 50, leaves 31.5 divisions of ferrocyanide, which were converted into ferridcyanide by the chlorine of the chloride of lime; and as each division represents 0.1 grain of chlorine, 31.5 will be equivalent to 3.15 grains of chlorine, which is the amount contained in 10 grains of the sample; consequently, 100 grains will contain 31.5 grains of chlorine, which is the same amount as is obtained by simply deducting the number of divisions of bichromate solution employed from 50 of ferrocyanide used in the estimation.

Though this process appears a long one, from the details which are necessary to explain its principle, yet in practice it is very expeditious, and requires only a very few minutes for its performance, and is much quicker than either Gay-Lussac's or Otto's method.

Though I have as yet chiefly confined my attention to the use of the ferrocyanide of potassium in the estimation of chlorine in bleaching powder, I have no doubt that it may be advantageously employed in many other useful determinations by carrying out the principles already explained: thus, for example, it may be used as a means of determining the amount of bichromate of potash present in a sample of that salt, or the quantity of chromic acid that exists under different circumstances. Again, the same salt may be used in different determinations where a certain amount of chlorine is liberated, which represents a proportional quantity of some other substance: thus, for example, in the estimation of manganese ores for commercial purposes, where they are heated with hydrochloric acid, the quantity of chlorine disengaged will indicate a certain amount of peroxide of manganese in the ore, on the presence of which its commercial value almost entirely depends; and the chlorine evolved may be estimated by absorbing the gas in a dilute solution of caustic potash, and then determining the amount of chlorine in it by precisely the same process as that I have recommended in the valuation of chloride of lime. To test the accuracy of this method, I heated in a small flask a given quantity of pure bichromate of potash with an excess of strong hydrochloric acid, and collected the evolved chlorine by means of a dilute solution of caustic potash, employing the bulb retort and curved dropping-tube, as recommended by Bunsen in the "*Analysis of the Chromates*" (see the last edition of Fresenius's "*Quantitative Analysis*," page 234), and ascertained afterwards, by the use of the ferrocyanide of potassium, the amount of chlorine evolved, which corresponded almost exactly with the calculated amount of that substance which should have been obtained by the action of the quantity

of bichromate used on the hydrochloric acid. Again, a standard solution of ferrocyanide of potassium may be used, as E. de Haen has shown, to determine the strength of the permanganate of potash in the analysis of the ferrocyanide and ferridcyanide of potassium, as an acidified solution of the ferrocyanide, as before stated, rapidly decolorises a solution of permanganate of potash, whereas the ferridcyanide has no action on that salt; and this reaction might be taken advantage of, in the valuation of chloride of lime, to determine the excess of ferrocyanide used in my process, but from my experiments I found that more precise and accurate results were obtained by the use of the bichromate of potash.

The reaction of the bichromate of potash on the ferrocyanide might be employed in the valuation of the ferrocyanide of potassium and other ferrocyanides, having previously, in the case of those which were insoluble, converted them into the ferrocyanide of potassium by boiling them with caustic potash, and separating the insoluble oxides by filtration.

It might also be employed for the valuation of the commercial red prussiate of potash, which is now to some extent employed as a bleaching agent in calico printing, and which consists of varying quantities of ferro- and ferridcyanide of potassium together with chloride of potassium. By ascertaining first how much a given quantity of the sample requires of a standard solution of bichromate of potash to convert the ferrocyanide present into ferridcyanide, the percentage of that substance would be known; and then by taking another portion of the sample and converting the ferridcyanide it contained, by reducing agents, such as the sulphites of soda and potash, &c., into the ferrocyanide, and finally determining the amount of bichromate necessary to bring the whole of the ferrocyanide then present into the state of ferridcyanide, the difference in the two results would indicate the proportion of ferridcyanide originally present in the sample.

The last application of ferrocyanide of potassium which I shall notice in the present communication is its employment as a reducing agent. It has long been known that the cyanide of potassium possesses most powerful reducing properties, and has been very usefully employed for that purpose in the reduction of different metallic salts under various circumstances; but I am not aware that the ferrocyanide of potassium has been proposed or used for similar purposes; at least, I have referred to a great number of analytical and general chemical works, and in none of them is this salt recommended as a reducing agent, though the cyanide is so much extolled for that purpose. According to my experiments, the ferrocyanide is a far more convenient reducing agent than the cyanide, and may be substituted for it in many cases of reduction with the best results, as it possesses many unquestionable advantages over that salt for this purpose. Thus, the ferrocyanide does not deliquesce and decompose when exposed to the air, whereas the cyanide rapidly absorbs moisture, and, unless kept in very well-stoppered bottles, becomes quite wet, and in this state quickly decomposes; and this deliquescence on the part of the cyanide is often a source of much inconvenience in its use as a reducing agent, owing to the almost unavoidable absorption of more or less moisture which takes place in mixing it with the substance to be reduced, and during the introduction of the mixture into the reducing-tube. The ferrocyanide, on the other hand, in a thoroughly dried and finely-powdered state, can be intimately mixed with the substance without any appreciable absorption of moisture.

I made the following comparative experiment to ascertain the relative absorptive properties for moisture of the two salts under the same circumstances:—Having thoroughly dried in a water-oven, till it ceased to vary in weight, some finely-powdered ferrocyanide, I placed 50 grains of it in a counterpoised watch-glass, and powdering in a warm mortar some fresh cyanide of potassium, I placed the same quantity of it in a similar counterpoised watch-glass, and left them both exposed to the air. On examining them after four hours' exposure, I found that the former had only gained $\frac{1}{10}$ th parts of a grain of moisture, whereas the latter had taken up 3.6 grains, or sixty times as much moisture under the same circumstances. After two days' exposure I found that nearly all the cyanide had passed into the liquid condition, having taken up 4.6 grains of water; whereas the ferrocyanide appeared perfectly dry, and had only absorbed 1.4 grain.

The great fusibility of the cyanide is sometimes rather a disadvantage, which has to be lessened by mixing it with a certain proportion of dried carbonate of soda; but the ferrocyanide not fusing at so low a temperature, does not require in most cases this admixture to lessen its fusibility. Again, the ferrocyanide is not a poisonous salt, whereas the cyanide is highly so, and must be used with great caution; and, lastly, the former salt is little more than half the price of the latter. Combined with the above advantages, I find that the ferrocyanide is equally effective in reducing metallic oxides and sulphurets, and is especially convenient for the reduction of different combinations of arsenic and mercury, which are reduced by it with the greatest ease.

I made several comparative experiments with the dried ferrocyanide and with the cyanide as reducing agents for the sulphuret of arsenic and arsenious acid, employing the same quantity of arsenical compound with each salt, under similar circumstances; and in almost every case, particularly where the quantities operated on were minute, I obtained more satisfactory results with the dried ferrocyanide than with the cyanide.

The following were amongst my experiments:—I mixed the $\frac{1}{10}$ th of a grain of sulphuret of arsenic with 3 grains of the dried ferrocyanide, and made a similar experiment, substituting the same quantity of cyanide; and, on heating the mixtures in similar glass tubes, obtained almost identically fine and characteristic rings of metallic arsenic.

I then intimately mixed the same quantity of sulphuret of arsenic with 49.9 grains of very finely-powdered glass, and taking 5 grains of this mixture, containing the $\frac{1}{10}$ th part of a grain of the sulphuret, mixed it with 5 grains of the dried ferrocyanide, and made a comparative experiment with another 5 grains of the mixture, substituting the same quantity of cyanide: on heating both these mixtures in small reduction-tubes, I got the characteristic metallic rings in both, but better defined in the case of the ferrocyanide.

I finally took 2.5 grains of the mixture of sulphuret and glass, containing about $\frac{1}{1000}$ th parts of a grain of sulphuret of arsenic, and treated them in the same manner, using in one case 2.5 grains of ferrocyanide, and in the other 2.5 grains of cyanide, and obtained in each case a minute metallic ring, which, however, was much more distinct and satisfactory where the ferrocyanide had been used as the reducing agent.

The same comparative experiments were made with arsenious acid, when results similar to those in the case of the sulphuret of arsenic were obtained.

The ferrocyanide, therefore, is a most delicate reducing

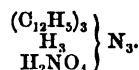
agent in the case of arsenical compounds, and where very minute quantities have to be detected, appears from my experiments to give more satisfactory results than the cyanide.

Whether the addition of dried carbonate of soda would improve the ferrocyanide for some cases of reduction, I am not at present able to say; but in one experiment which I made with the sulphuret of arsenic, I obtained as good results, using the ferrocyanide alone, as where it was mixed previously with its own weight of dried carbonate of soda. In many cases the ferrocyanide may be used as a reducing agent in a state of powder without separating its water of crystallisation; but, in most cases, it will be rendered a far better reducing agent by being previously dried at 212° in a water-bath or oven; and in this dried condition it may be kept for any length of time in a good-stoppered or well-corked bottle.

Though as yet my experiments have been chiefly confined to the reduction of different compounds of arsenic and mercury, I entertain no doubt that the ferrocyanide of potassium will be found an equally effective reducing agent in the case of the combinations of other metals, and that it may with great advantage be substituted for the cyanide of potassium in many cases where the latter salt is used as a reducing agent.

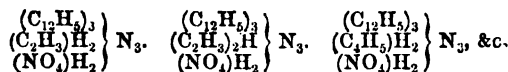
On Aniline Red, by M. E. KOPP.

I FIND that aniline red, produced by the action of nitric acid or of nitrates of an easily-reducible base, is a triamine, representing mononitro-trianiline,—



The existence of binitro-trianiline seems to me almost certain, as, by treating aniline with a mixture of sulphuric and nitric acid, we obtain a yellowish-brown body, possessing in the highest degree the properties of a nitro compound; but I have not yet succeeded in purifying it sufficiently to make the analysis coincide exactly with the formula.

In trianilines a portion of the hydrogen may be substituted by methyl, ethyl, and amyl, along with nitrous gas. These are very probably the compounds:—



I specified in my paper these substances only as hypothesis, but they really exist. I have already obtained several of them, and it is very remarkable that the red tint disappears, becoming from violet more and more blue, according as the hydrogen is replaced by the hydrocarbons.

The latter are not yet sufficiently pure for analysis, though very beautiful as shades of colour. They are generally almost insoluble in water, but easily soluble in alcohol or acetic acid. When the trianiline, instead of being nitrated, is chlorated, bromated, or iodised, the compound is equally a red matter, but presenting slight differences of tint.

I venture to hope that my work will serve to throw some light on the mode of formation, the composition, and the constitution of the colouring matters, so remarkable and so important, which are derived from aniline.—*Comptes-Rendus.*

On the Properties of Liquid Carbonic Acid,
by GEORGE GORE, Esq.

In this communication the author shows how a small quantity of liquid carbonic acid may be readily and safely prepared in glass tubes closed by stoppers of gutta percha, and be brought in a pure state into contact with any solid substance upon which it may be desired to ascertain its chemical or solvent action, or be submitted to the action of electricity by means of wires introduced through the stoppers.

By immersing about fifty substances in the liquid acid for various periods of time, he has found that it is comparatively a chemically inert substance, and not deoxidised by any ordinary deoxidising agent except the alkali metals. Its solvent power is extremely limited. It dissolves camphor freely, iodine sparingly, and a few other bodies in small quantities. It does not dissolve oxygen salts, and it does not redden solid extract of litmus. It penetrates gutta percha, dissolves out the dark brown colouring matter, and leaves the gutta percha undissolved, and much more white. It also acts in a singular and somewhat similar manner upon india rubber. The india rubber while in the liquid acid exhibits no change, but immediately on being taken out it swells to at least six or eight times its original dimensions, and then slowly contracts to its original volume, evidently from expansion and liberation of absorbed carbonic acid, and it is found to be perfectly white throughout its substance. These effects upon gutta percha and india rubber may prove useful for practical purposes.

The liquid acid is a strong insulator of electricity; sparks (from a Ruhmkorff's coil) which would pass readily through $\frac{1}{2}$ inch of cold air, would with difficulty pass through about $\frac{1}{16}$ th of an inch of the liquid acid.

In its general properties it is somewhat analogous to bisulphide of carbon, but it possesses much less solvent power over fatty substances.—*Proceedings of the Royal Society.*

TECHNICAL CHEMISTRY.

Cyanogen Steel.

It is curious to observe how frequently several inventors are engaged at the same time upon similar discoveries, although each of them may be pursuing his labours altogether independently of the others, and although the discovery may be unquestionably new. At the time when the pneumatic process of refining iron was creating so much attention, it will be remembered that both Mr. Martien and Mr. Bessemer had lodged provisional specifications within three months of each other, consequently the provisional protection accorded to each was running simultaneously, and before the public or any other person but themselves had had an opportunity of ascertaining the details of the respective inventions. About the same time the presence of nitrogen in steel was positively declared by Mr. Christopher Binks, and his project for employing cyanogen compounds in the manufacture of that metal was brought prominently forward. The question, however, of improving the mode of manufacturing steel had at that period been discussed to repletion, and Mr. Binks' assertion was regarded with suspicion, or, at least, as the project of an enthusiastic patent seller. After long remaining in abeyance (the

fact of the discovery being only kept from sinking into oblivion by the occasional lodging of a specification, in which the use of substances capable of yielding cyanogen was incidentally mentioned), the subject has been revived, and is now restored to more than its pristine vigour, by the publication of M. Fremy's communication to the French Academy of Sciences; and it seems probable that cyanogen compounds will ere long be acknowledged to be as necessary to the manufacture of steel as carbon has hitherto been.

In Mr. Fleury's process of manufacturing steel with electricity cyanogen is regarded as a necessary constituent of steel; and several other processes, which have in practice proved to give excellent results, may owe their success to the unintentional introduction of cyanogen compounds. Amongst the specifications of patents issued during the past week, is one relating to an invention by Messrs. Duhesme, De Ruolz, and De Fontenay, for improvements in the manufacture of cast-steel, and in the re-manufacture of old steel, in which the use of cyanogen is again the principal ingredient employed. In making cast-steel according to this invention, certain mixtures of wrought-iron, cast-iron, oxide of iron, old steel, and ferrocyanide of potassium, or ferricyanide of potassium, or some of them are to be employed. The ferrocyanide of potassium is commonly known as yellow prussiate of potash, while the ferricyanide of potassium is commonly known as red prussiate of potash. The corresponding sodium salts may be substituted for the potassium salts. Were the sodium salts used, the process would be almost identical with that of Mr. Charles Sanderson, of Sheffield, also made public several years since. The papers read before the Society of Arts by Mr. Binks, advocating cyanogen, and by Mr. Sanderson, for employing substances capable of yielding cyanogen, for improving the quality of iron and the allied metals, having been presented within a short time of each other. But whether the salts of sodium or those of potassium be used, it is evident that Messrs. Duhesme, De Ruolz, and De Fontenay have been long anticipated, the cyanogen being incontestably the ingredient upon which they rely for success. In the manufacture of cast-steel, they continue, one mixture employed consists of—Wrought-iron, 700 parts by weight; white cast-iron, 200 parts; oxide of iron, 100 parts; red prussiate of potash, 8 parts. A second mixture consists of—Old steel (old files, tools, or springs), 500 parts by weight; wrought-iron, 500 parts; red prussiate of potash, 14 parts. A third mixture consists of—Old steel, 350 parts by weight; wrought-iron, 350 parts; white cast-iron, 200 parts; oxide of iron, 100 parts; red prussiate of potash, 7 parts. A fourth mixture consists of—Wrought-iron, 1000 parts by weight; oxide of iron, 75 parts; red prussiate of potash, 28 parts. From the small proportion of cyanogen added in the several instances, the astonishment that its existence did not long previously attract attention will at once disappear, the quantity of ferricyanide being only such as would cause a very small proportion of cyanogen to remain in the iron at the completion of the process.

If the ferrocyanide be employed instead of the ferricyanide of potash in the mixtures described, double the quantity indicated for the red prussiate of potash is used; but the latter is preferred as giving a more decided reaction, and also on account of its containing no water of crystallisation. Refined iron, or fine metal, may be advantageously substituted for the cast-iron in the first and third mixtures, in which case the quantity

of fine metal employed must be double that indicated for the cast iron, without altering the proportions of red prussiate of potash. The cast-iron employed should be white, and of at least two qualities, one of which should contain manganese. It is to be first broken up into small fragments. The wrought-iron should be in the smallest possible fragments, and it should be avoided to use granular with fibrous iron. Granular iron of a good quality is preferred. The old steel should be of a good quality, and introduced into the mixture in small pieces. For the re-manufacture of old steel one mixture employed consists of old steel, such as old files, tools, or springs, 1000 parts by weight; red prussiate of potash, 10 parts. Another mixture consists of old steel, 1000 parts by weight; yellow prussiate of potash, 20 parts. In carrying out the manufacture of cast-steel, as also the re-manufacture of old steel, according to this invention, the above mixtures have to be melted down in crucibles, the ingredients having been previously mixed as completely as possible. The old steel, as also the wrought and cast-iron, should first be broken into small pieces, and the prussiate of potash should be well distributed throughout the mass. The operations take place in a common air furnace, for which a very pure coke is employed, mixed with a tenth part of wood charcoal. The crucibles must be kept hermetically closed until complete fusion of the metals ensue, and the fire must be managed with the usual precautions. The same description of crucible is employed as is generally used for the smelting of steel. Those of plumbago are preferred; but, if these are used, it is well to increase slightly the proportion of prussiate of potash after the crucibles have been employed for two successive melt-ings.—*Mining Journal*.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE VII.

Nitrogen — Preparation and Properties — Compounds of Nitrogen with Oxygen and with Hydrogen. — Carbon — its Properties in the Form of Diamond, Graphite, and Charcoal.

We have to-day to consider an element which, in many respects, presents striking contrasts to the two elements we have hitherto studied. This element is nitrogen, a body which is rather distinguished for the sluggishness of its affinities than for the facility with which it unites with other bodies. On this account, nitrogen exists in nature chiefly in the free or elementary condition. It is contained in this state in the atmosphere, and constitutes about four-fifths of the total volume of atmospheric air, and more than three-fourths of the weight of the atmosphere. It exists, however, also in combination, although in comparison with the quantity contained in the atmosphere this amount in combination should be regarded almost as a vanishing quantity. It exists, however, in the tissues of plants and animals, and is in fact essential to the existence of animal and vegetable life. It is found also in the remains of plant life of former ages; consequently it is a constituent of our coal formations, and exists there to the extent of from one to one and a-half, and even, in some cases, up to two per cent. It is also a constituent of nearly all those highly poisonous vegetable

substances, such as strychnine, brucine, narcotine, and bodies of that kind. It is also an essential element in hydrocyanic acid, that very poisonous prussic acid, as it is sometimes termed, with the properties of which we are all acquainted. There is scarcely any vegetable poisonous substance which does not contain nitrogen. That singular vegetable extract with which the Indians poison their arrows, is, so far as we know at the present time, entirely free from nitrogen. That and the poison of *cocculus Indicus* are nearly the sole exceptions to this general rule that these poisonous vegetable substances invariably contain nitrogen.

We can prepare this element either by abstracting it from the atmosphere, or, in other words, by removing the element with which it is associated in the air, namely, oxygen; or we can extract it from some of its compounds. The first is the most simple mode of proceeding. As nitrogen is remarkable for its want of affinity, and as oxygen is distinguished for its powerful affinities, there are many means by which we can withdraw the oxygen from the nitrogen in the atmosphere, thus leaving the nitrogen in a free state. A common candle will do this, to a certain extent, by burning it in a closed vessel filled with atmospheric air. Under such circumstances, after a certain time, the candle goes out. Now, it goes out when it no longer finds a sufficient quantity of oxygen to continue its combustion; but, unfortunately, a candle will not continue to burn until all the oxygen has been removed. Besides this, it also produces, at first, carbonic acid, and when the candle is about to be extinguished for want of more oxygen it produces carbonic oxide. From the carbonic oxide it is very difficult afterwards to free the nitrogen; and consequently this process of burning a candle in the air would not answer our purpose for procuring free nitrogen. Phosphorus, however, is a substance which is more combustible than a candle is, and which does not generate, on its combination with oxygen, any volatile matter—any gaseous substance—which we should have difficulty in subsequently removing from the nitrogen. Further, the phosphorus is capable of continuing to burn until every trace of oxygen has been removed from the air, and phosphorus, consequently, is well adapted for the preparation of nitrogen, leaving it in a state of comparative purity.

Here is the apparatus by which we effect this. We place a little piece of phosphorus on a small capsule floating on water, and then ignite it by means of a heated wire, and invert over it a bell-jar filled with air. At first we have an expansion of the air contained in the jar, owing to the heat generated by the combustion of the phosphorus, but this expansion will soon cease, and give place to a contraction as the combustion goes on; and we shall notice, I think, when the phosphorus has become extinguished that the gas remaining in the receiver will be considerably less than the bulk of air which the vessel contained at first. Those of you who can see over the edge of this vessel will perceive that even at the present moment the water is gradually rising in the interior. This rising of the water will increase as the temperature of the enclosed nitrogen diminishes. You see the phosphorus burns more and more dimly, and it will finally be extinguished. At the moment of its extinction, the last particle of oxygen will have been removed from the atmospheric air. Now, as we shall subsequently find, atmospheric air consists of particles of oxygen and nitrogen, the nitrogen only remaining after the extinction of the phosphorus. The gas which remains may be regarded as pure nitrogen, or, at all events, nitrogen very nearly pure.

That is one mode of preparing the element. Another plan consists in causing two compounds of nitrogen to decompose each other. If we mix together a compound of nitrogen and hydrogen, which we term ammonia, and a compound of nitrogen and oxygen, which we call hypnitrous acid, we have a solution containing just a sufficient amount of oxygen in the one compound to combine with the hydrogen in the other compound so as to produce

water, leaving the two quantities of nitrogen that were contained in those two compounds in the elementary form. Now, nitrogen, prepared by either of these processes, possesses the following properties: It is a perfectly colourless and transparent gas. Here we have a jar of it, and just as we saw in the case of the oxygen and hydrogen, so here with nitrogen; you cannot distinguish a vessel filled with this gas from another vessel filled with atmospheric air. Here [in the bell-jar in which the phosphorus had just been burnt], at present, we have a quantity of white fumes mixed with our nitrogen. These white fumes consist of phosphoric acid produced by the combustion of phosphorus at the expense of the atmospheric oxygen. This phosphoric acid will gradually subside and dissolve, leaving the gas, after a few minutes, as clear and transparent as that which we have in the other jar, and which was prepared, in fact, by this process. Nitrogen possesses nearly the same specific gravity as atmospheric air. Taking atmospheric air as unity it is slightly more than '97. It is very slightly soluble in water—far less so than oxygen. One hundred volumes of water dissolve only about one and a-half volumes of nitrogen, whilst about double this quantity of oxygen is dissolved. This greater solubility of oxygen than atmospheric nitrogen in water is a most important point connected with the existence of animal life beneath the surface of water. Fishes live in water by means of the atmospheric oxygen which is dissolved in that liquid. They have no power whatever of decomposing the water, and separating the oxygen from the hydrogen; but they require for their respiration free oxygen, just in the same way as animals living in the air; and they receive this oxygen from the quantity of air dissolved in water. If you deprive water of air by placing it under the exhausted receiver of an air-pump for a few moments, fishes placed in that water will very soon die, because they cannot obtain the supply of oxygen which is necessary to continue their respiratory functions. In consequence of water dissolving more oxygen than nitrogen, these animals have, so to speak, a dissolved atmosphere, far richer in oxygen than our ordinary air; and, therefore, by a comparatively much smaller bulk of air than land animals require they can continue their respiration, and obtain quite a sufficient amount of oxygen for their wants.

—In ordinary language we generally describe nitrogen as being incombustible in itself, and incapable of supporting combustion. If we take a jar of it—let us take this jar here, for instance—and plunge into it a lighted taper, we shall see, on the one hand, that when the paper comes in contact with the gas the latter will not inflame, and, on the other hand, that the paper is extinguished when it is lowered into the jar; so that we gather from this experiment that nitrogen is incombustible; but these matters are not to be decided upon the dictum of a single experiment of this kind. It is quite possible that nitrogen may be very combustible at higher temperatures, and indeed we shall find that it is combustible to a certain extent at all events. Different combustibles require very different degrees of heat to set them a burning—to “ignite” them, as we call it. I have here a substance which is so easily ignited that the moment it comes in contact with the air, even at ordinary temperatures, it bursts into flame. I will expel some of it out of this tube—it is a liquid body—into the dish there; and you see this substance, as soon as it comes in contact with the air, ignites and burns brilliantly. Now phosphorus is another combustible body which will furnish us with a second link in this chain. The body which we have just expelled from the tube has its igniting point below the temperature of the atmosphere. The igniting point of phosphorus is higher than the ordinary temperature of the atmosphere, and if we bring a piece of this phosphorus into the air, you see it does not take fire; but it will take fire at a temperature not very much higher than that of the

atmosphere, and if we float it in a copper capsule upon hot water, bring it in contact with the heated surface, our phosphorus will probably then ignite. [A piece of phosphorus was ignited in the manner described.] There you see at that temperature our phosphorus ignites and enters into combustion. Then, again, we have a substance, the bisulphide of carbon, which has an igniting point a little higher than phosphorus, but still far below an ordinary flame. We will warm this glass rod. I need not have it so hot that I cannot bear to draw it through my fingers. With this warm rod I dare say I shall be able to ignite this bisulphide of carbon. Yes; you see it ignites at this temperature, far below any of our usual igniting points. The rod is sufficiently hot, but I can draw it in this way through my fingers, although it possesses power to ignite this inflammable substance. Now, proceeding upwards, the ordinary gas flame which we burn has an igniting point still higher than this bisulphide of carbon, but lower, again, than some other bodies. The ordinary coal gas, if exposed to very bright red heat, ignites; but if it be exposed to a temperature below this very bright redness, that is, if you take an iron bar heated only to dull redness, it will fail to ignite this coal gas. Now, we have here another material which is still less combustible than coal gas; this material is a compound of nitrogen which I shall presently have to explain to you. It is called ammonia, or ammoniacal gas. We are generating it here in this vessel, and passing it through this iron tube which is heated to redness by a gas furnace. I dare say by the aid of that external heat applied to the ammonia we shall get it to burn at the extremity of the tube; but the ammoniacal gas, as we shall presently see, is not ordinarily combustible, but only when previously heated in this way can it continue its combustion. If we allow a jet of ammoniacal gas to impinge on a candle-flame it will burn around the flame, but will not continue its combustion when the flame has been removed; so that if we were to pass this ammoniacal gas simply through a cold tube and endeavour to ignite it at the end we should get no flame from it. Probably, our tube will now be heated sufficiently high. [A light was applied to the end of the tube from which the gas was issuing.] Yes, there our ammoniacal gas burns at the extremity of the tube quite readily. Now we will turn off the gas of the apparatus by which the tube is heated, and you will see that soon after that gas has been extinguished the flame of the ammoniacal gas will gradually withdraw itself from the end of the tube and finally be extinguished. It can no longer retain its ignition, because the temperature produced by the combustion of ammonia is by no means so high as its igniting point. We can only keep the temperature at the igniting point by previously passing the gas through this hot tube. It thus produces a higher temperature, just as you get a higher temperature in a furnace fed by heated air than you do in a furnace fed by ordinary air; and the difference between these five or six hundred degrees of heat is sufficient to raise the temperature of the burning ammoniacal gas beyond its igniting point, and therefore to render it capable of burning in the air. The element nitrogen, to which all these experiments lead us up, has an igniting point still higher than that of any of these substances. It is higher even than that of ammoniacal gas; and we require to expose it to a still higher temperature in order to bring about its combustion with oxygen. Further, like ammoniacal gas, nitrogen on its combustion with oxygen, that is, when you get it to burn by heating it to the proper temperature to render it combustible, produces, in burning, a less amount of heat—I mean to say a lower temperature is produced—than is equal to its igniting point. Its igniting point is higher than the temperature produced on its combustion. On this account the combustion cannot be propagated from particle to particle, or from mass to mass of the mixture of oxygen and nitrogen gases. If this were not the case

it is obvious that all the nitrogen in the air would long ago have been burnt up. The first time that a philosopher obtained the heat of the oxyhydrogen blow-pipe he would inevitably have ignited the atmospheric nitrogen and burnt up the whole of the atmospheric oxygen. The combustion would have been propagated from particle to particle, had not the igniting point been higher than the temperature produced by its combustion. The union of the two gases which compose the air must have resulted from this ignition at a single point; but as the igniting point is higher than the temperature produced by the combustion of the nitrogen, the combustion of atmospheric air cannot be communicated from one point to another, but can only go on where the temperature is sufficiently high. The temperature of an ordinary fire is not sufficiently high for this purpose. It does not exceed about 4000° Fahr. The temperature of our ordinary oil, gas, and candle flames does not exceed about 3500° Fahr. The igniting point of nitrogen is considerably above that. It is impossible to measure these high temperatures, but I have calculated from an experiment with a mixture of oxygen and nitrogen, that a temperature of about 5400° is necessary to ignite nitrogen. Now, you see the temperature we have in a common fire, and that which we have in common gas and candle flames, are not sufficient for the ignition of nitrogen, and therefore we get no combustion of nitrogen in these flames—a circumstance which, as we shall presently find, is of great value to us, and contributes considerably to the comfort of our lives. We should have exceedingly noxious effects produced in the atmosphere by the ignition and combustion of nitrogen with oxygen. We have, however, some sources of heat sufficient to ignite nitrogen. One of these sources is the combustion of oxygen and hydrogen gases in the proportions to form water, or nearly so. If we mix these gases with nitrogen, and then ignite them, and add at the same time a certain excess of oxygen, the nitrogen burns,—to a limited extent it is true,—but it undergoes very definite combustion.

The temperature of the electric spark is also high enough to ignite nitrogen, being above the igniting point of that gas; and consequently if we pass a stream of electric sparks through a jar of atmospheric air, such as we have here, we shall have an ignition of the nitrogen which it contains, and its combustion at the expense of the atmospheric oxygen. You see at the present moment [exhibiting a glass flask containing air] this is quite colourless. If the nitrogen burns on the passage of the electric sparks, it will produce ruddy vapours in the flask. You will see, in the first place, a yellowish colour, and, finally, a reddish colour, produced by the action of the stream of electric sparks, as they pass between these two points. [A series of electric sparks from a Ruhmkorff's coil was then passed through the flask of atmospheric air.] We shall only get our colour after some time; but it is an experiment which is worthy our seeing, we shall therefore wait. Oxidation of the nitrogen is caused by the passage of the electric sparks through the jar, and consequently such an oxidation must take place every time a flash of lightning passes through the atmosphere.

Nitrogen in thus uniting with oxygen, forms an extensive series of compounds. Here we have a list of them in this table:—

Compounds of Nitrogen with Oxygen.

Protoxide of Nitrogen . . .	N O
Binoxide of Nitrogen . . .	N O ₂
Hyponitrous Acid . . .	N O ₂
Nitrous Acid . . .	N O ₃
Nitric Acid . . .	N O ₅

In any proportions in which the oxygen does not exceed two equivalents to one of nitrogen, the compound obtained is neutral; when there is a larger proportion of oxygen than this, the compound becomes acid.

We have already a very perceptible yellow colour-

ing produced in that flask [through which the electric sparks were still being passed]; and in the course of a minute or two more, I dare say it will be more perceptible. The compounds produced here are the three last mentioned in the table to which I have just directed your attention. They consist partly of hyponitrous acid, partly of nitrous acid, and partly of nitric acid. All these three substances are liable to be formed in this way; and in the action of lightning in thunderstorms these three compounds are produced, but the two compounds containing the smaller proportions of oxygen gradually pass into the last containing the highest proportion of oxygen; and therefore it is that we usually find present in the rain that descends after a thunder-storm a certain proportion of nitric acid.

We have now a very decided brown [again referring to the flask through which the sparks had been passing]. The operation is a very slow one, but the formation of these oxides of nitrogen continues to increase.

This is one source, then, of the fifth combination of nitrogen with oxygen—nitric acid, which we have next to consider. It is produced by the combustion of atmospheric nitrogen, and it is also found in a state of combination with bases. It forms nitrates with bases. Some chemists are of opinion that all the nitric acid which we meet with in combination with potash or soda, in the so-called "nitre districts," is due entirely to the putrefaction and decay of animal and vegetable substances; while others contend that it is due to the combustion of nitrogen by atmospheric electricity. Probably the proportion of nitric acid generated in the atmosphere, and present in the rain in this way, is comparatively small; and we find our chief supplies of this substance from the oxidation of nitrogen contained in animal and vegetable substances, but especially in animal substances. Now, nitrogen which is already combined is capable of undergoing this kind of combustion at the ordinary temperature of the atmosphere. It is going on around us, in fact, in the soil of London. Permeated as it is by the sewerage, we have there animal matters containing nitrogen undergoing this process of combustion with oxygen without any elevation of temperature whatever, producing immense quantities of this substance, nitric acid, which may be detected in all the shallow wells in and around London. It is especially abundant in the neighbourhood of burial grounds, where animal matter is oxidizing to a greater extent than it is in other localities; and the wells in those neighbourhoods have been found to contain a large proportion of the salts of nitric acid.

Nitric acid may be made by a process such as we have proceeding here. It is here being obtained by displacing it from its compounds with bases by means of sulphuric acid. We have added a quantity of sulphuric acid to this nitrate of potash, or common nitre, which is one of the best known salts of this body. Sulphuric acid is capable, under the influence of heat, of displacing the nitric acid. After the reaction we shall have left sulphate of potash in the retort, whilst nitric acid distils over.

Nitric acid thus obtained is a colourless, fuming, limpid, highly corrosive liquid, and contains one equivalent of nitrogen and five equivalents of oxygen, which we have represented on the diagram by the symbol NO₅, but our nitric acid contains also one equivalent of water. It is distinguished for its powerful oxidizing properties. It oxidizes metals with very great facility, converting them, first, into oxides, and, subsequently, into nitrates. Here is some of this acid which is used commercially on a large scale, being formed by this decomposition which we have going on here. The operation is performed in large cylinders which are lined with fire bricks in order to protect them from the action of the acid. If nitric acid is brought into contact with copper a very powerful oxidation of the copper takes place, with the production of

these red fumes. These red fumes are the same as those which we have been forming in this flask, only you see here they have a greater intensity. Here is a greater amount of this red compound produced by the deoxidation of this nitric acid. It is from this deoxidation of the final compound of nitrogen and oxygen that we usually obtain our supplies of the other compounds of nitrogen and oxygen. We might perform the same experiment, also, with iron, which is oxidized in the same way and gives rise to the same red fumes. Nitric acid, however, is incapable of oxidizing all the metals in this way. If we bring it in contact with gold, for instance, no such oxidation occurs. [A piece of gold leaf was placed in a glass vessel containing nitric acid.] The gold floats about in the nitric acid with its brilliancy unaffected. It is also incapable of oxidizing platinum and a few other metals; but the great majority of metallic substances are capable of being oxidized by it. These oxides, being taken up by the remaining nitric acid present, form nitrates, and thus we produce our nitrate of copper, nitrate of iron, nitrate of zinc, and nitrates of other metals which we wish to obtain.

Nitric acid may be very readily detected by this very process that we have been performing here,—namely, by deoxidizing it by means of copper turnings. If we first set the acid free from any combination in which it may previously have existed, by the addition of sulphuric acid, as we have done here, and then bring copper turnings into contact with the liquid in which the nitric acid is thus set free, we have these red fumes produced which are quite characteristic of the presence of nitric acid. When the quantity operated upon is small, we may not, perhaps, perceive any of these fumes: in this case we can detect the presence of the nitric acid by those papers we employed on the last occasion for another purpose. Here are some papers which have been immersed in a mixture of starch and iodide of potassium, and you see they are powerfully acted upon by this gas; the action is, indeed, too powerful for you to see it properly. They are browned very strongly, but with a smaller quantity the paper is rendered blue. You see this bluing action better when the paper is moistened a little.

Now, you will remember that this is precisely the test which is used for the detection of ozone in the atmosphere. We mentioned it in the last Lecture, as being used to show the presence of ozone in the atmosphere, and we have seen that every electric spark that passes through the air produces this acid. How, then, can this be a proper test for the presence of ozone in the atmosphere? If we know that this nitrous acid produces the same effect upon the papers as was produced by what is supposed to be ozone, how can such papers be said to be a test for ozone? It is obviously quite unreliable. The effect would be produced, and I think probably is produced, by the nitrous acid derived from the passage of electric discharges through the air, and not by ozone at all.

From nitric acid we can obtain, by graduated deoxidation, the remaining compounds of nitrogen with oxygen. The first compound resulting from the gradual deoxidation, is nitrous acid, and the next is hyponitrous acid, which contains three equivalents of oxygen and one of nitrogen; the next compound which we shall come to, will be the so-called "binoxide of nitrogen," which contains two equivalents of oxygen and one equivalent of nitrogen. This is the substance which is produced when nitric acid is acted upon by copper turnings; in fact, we had it produced in the first experiment. In order to obtain this substance, NO_2 , it is necessary to exclude the air; and here is an apparatus by which we can perform that experiment. The operation shall be going on while we are endeavouring to make ourselves acquainted with the properties of this gas. Here we have a vessel containing copper turnings. We pour a portion of nitric acid through this funnel-shaped tube, which passes nearly to the bottom of the jar. The

gas, on being evolved, will pass out through this flexible tube, and be collected here, [referring to a jar inverted over the pneumatic trough.]

Binoxide of nitrogen is a perfectly colourless and transparent gas, like those we are already acquainted with. Here is a jar of it, for example, which has been already collected, and in which we shall be able to see some of its properties. It possesses a specific gravity only slightly greater than that of air, very slightly indeed, it being 1.04. It is slightly soluble in water, water absorbing about 5 per cent. of this gas; but still we can collect it over water without any material loss; that is, in fact, the mode in which this gas is generally collected. One of its most distinguishing characteristics is its strong affinity for a further amount of oxygen; it is, indeed, a chemical compound in a very unstable position, and it has a great desire for a further quantity of the element oxygen. If we press some of this gas up into a jar containing oxygen, we shall find that it will at once produce those ruddy fumes that we had formed here, owing to the transformation of the binoxide into the next two compounds below it on the diagram—hyponitrous acid and nitrous acid. Here, for instance, is a jar half filled with oxygen, [the jar of binoxide of nitrogen was discharged into the jar of oxygen standing in the pneumatic trough.] You see where it comes in contact with the oxygen, these dense red fumes are produced, and you will notice after a very short time, that there is a considerable absorption of the red compound in the interior of the jar. You see how the water is rising in the interior of the jar taking the place of the gas which it absorbs. These two compounds, hyponitrous acid and nitrous acid, which were produced by the addition of the oxygen to the binoxide of nitrogen, are both of them very soluble in water, and are, therefore, very rapidly removed from the remaining oxygen present in the jar.

Binoxide of nitrogen presents a rather remarkable character in regard to combustibles. It is difficult to say whether it is a supporter of combustion or not. It is not inflammable, at all events; we can affirm that of it positively; and if we plunge a taper into this gas, you see the light is immediately extinguished, so that we should be rather inclined to conclude from that experiment that binoxide of nitrogen does not support combustion; and perhaps we should be quite right in that conclusion. The fact is, that unless the burning substance we place in the binoxide has sufficient heat in it to decompose the gas, transforming it into its elements, oxygen and nitrogen, the combustion is not supported. If, however, the combustible can decompose the gas, it will continue to burn in the mixture of nitrogen and oxygen which is formed. Phosphorus, for instance, is a substance of this kind. If we put a fragment of phosphorus into this jar of binoxide of nitrogen, if it is only just ignited the temperature of the phosphorus is not enough to decompose the gas; but if actively in combustion it continues to burn in the gas with very considerable brilliancy, especially after the phosphorus vapourises, and thus comes into extensive contact with the binoxide. But it is not in the binoxide itself that the phosphorus burns, because if we introduce it in only a languidly ignited state, it is extinguished. Another substance which is capable of burning in this gas is the bisulphide of carbon, which we have already used in a former experiment. Here is our jar of binoxide of nitrogen; here is our bisulphide of carbon. We will pour a little bisulphide of carbon into the jar and agitate it a little, so that the vapour may mix pretty well with the gas. [A light was applied to the mouth of the jar, and a slight explosion, accompanied with a bright flash, took place.] This substance, you see, undergoes very brilliant combustion in the binoxide. The temperature of its combustion is sufficient to decompose the gas, and then the vapour of the bisulphide burns in the mixture, which contains an equal quantity of oxygen and nitrogen, and

is, therefore, much richer in oxygen than atmospheric air. The bright light contains also a large amount of what have been called the "chemical rays," and therefore it has been used in photography. It enables you—in the dark, I was going to say—at all events, without sunlight, to take photographic pictures.

Now, the remaining compound of nitrogen with oxygen—namely, the protoxide—is obtained by a still further reduction of nitric acid, by which we remove a further quantity of its oxygen. This protoxide is a compound in itself unimportant, but it is rather singular in its effects. It is remarkable for its peculiar effect upon animals, and on this account it is called "laughing gas." If I could exhibit some of its effects before you, I should then consider myself justified in speaking longer about it; but as we cannot very well do that, I think we had better pass it by simply with the remark that it resembles the binoxide with regard to combustibles. Some substances will not burn in it. Sulphur, for instance, is at once extinguished; therefore we can say of all these gases that they do not support combustion when the oxygen is in combination; but if they are decomposed by the heat of the combustible, the products of their decomposition, nitrogen and oxygen, do exert a supporting effect upon combustion.

We now come to the compounds of nitrogen with hydrogen,—namely, ammonium and ammonia,—the respective compositions of which are represented here on the diagram:—

Compounds of Nitrogen with Hydrogen.

Ammonia, or ammoniacal gas	NH_3
Ammonium	NH_4

These substances can neither of them be formed by direct combination with their elements. It requires indirect means in order to bring about these combinations. Both of these bodies are produced by the gradual decay or putrefaction of organic substances. Ammonia uniting with acids, or even with water, give rise to ammonium. But this ammonium is a somewhat hypothetical body. We have never as yet been able to get it in a free state. It is contained in chloride of ammonium, which is obtained by neutralising hydrochloric acid by ammonia. Chloride of ammonium contains one equivalent of ammonium united with one equivalent of chlorine. We might go a little farther than that, and form a kind of alloy, or amalgam, with this hypothetical substance, ammonium, by making it combine with mercury. Such an amalgam is produced when we act upon a solution of chloride of ammonium in water by an amalgam of sodium and mercury. The two metals, potassium and sodium, are capable of extracting the chlorine and leaving the ammonium disengaged from the chlorine. But the ammonium does not appear in a free state. It combines, upon the moment of its liberation, with mercury, and forms a kind of pasty amalgam, which reminds us of the amalgams formed by dissolving tin, zinc, lead, and other metals, in mercury. Here we have some amalgam formed with sodium and mercury, and I will pour some of it into this vessel [a shallow white dish], which contains a solution of chloride of ammonium. Now, you see this mass which I poured into that was not very large, but it is growing very considerably. It has nearly filled the lower part of the dish, and it has now changed its character. It is so light that it is floating upon the surface of the liquid here; and you see it forms a kind of butter-like mass which is capable of being moulded into any form, and taking a very high polish. Here we have some of this body forming in a tall jar, and you may there see it grow more clearly than in this basin. You see from that comparatively small portion of material what an immense mass of this butteraceous substance we have produced.

The other body, ammonia, is readily produced from chloride of ammonium, which we obtain from the destruc-

tive distillation of animal and vegetable substances. Gas liquor, obtained in the manufacture of coal gas, and bone liquor, furnish us with a great bulk of the ammonia contained in this chloride of ammonium. If we mix chloride of ammonium with quick lime or, better, with hydrate of lime—slaked lime—we remove from it the chlorine, and also one equivalent of the hydrogen which it contains, and we have then evolved, in the form of gas, ammonia or ammoniacal gas as it has been called with greater propriety. Here is some of it coming off; we will collect it at once, into this vessel containing water where it will be absorbed, and we shall obtain a solution of it. We have some of this gas already collected here in these jars; here is a jar of it, so that from this we can ascertain sufficient of its properties for our present purposes. It possesses a specific weight much less than that of the atmosphere; it is, bulk for bulk, about half the weight of atmospheric air, its specific gravity being about .539. It cannot be respired in its concentrated form; if diluted with air and then respired it has rather exhilarating and pleasant effects than otherwise. It is the volatile gaseous matter which comes out of those Preston salts bottles which are pretty generally known, and which are found to be sometimes refreshing. Under these circumstances it is of course highly diluted with air. Ammonia is slightly inflammable, but you saw that it was necessary, in the experiment where we burnt ammonia in the previous part of the Lecture, first to heat it above its igniting point in order to have a continuous combustion. It is one of those gases which can be rendered liquid by pressure. It is a highly alkaline substance, and this is at once rendered evident if we dissolve some of it in a reddened solution of litmus. Here, for instance, is a solution of reddened litmus in water, by means of which we will absorb this jar of ammoniacal gas. [The mouth of the jar containing the ammonia was placed in the reddened solution, and upon the removal of the stopper beneath the surface of the liquid the water rushed up and filled the jar.] You see how rapidly it is absorbed by water and how the red colour of the litmus solution, which we employed for the purpose of showing that ammonia is alkaline, was transformed into blue when the liquid came into contact with the gas. Water in this way is capable of absorbing no less than 780 times its volume of this gas.

We must now rapidly pass on to commence, at all events, the history of another element, namely, Carbon, which exists in three forms in nature; namely in the form of diamond with which we are all acquainted; of graphite, which we know in those lead pencils used for marking upon paper; and in the form of charcoal as it is obtained by heating animal and vegetable substances to redness. These substances are all of them essentially the same element, and they produce on their combustion precisely the same compound. We will endeavour to burn three specimens of these substances, namely, diamond, graphite, and charcoal, and ascertain whether the substance produced is really the same in each case, and by that means establish the identity of the three substances. We will first endeavour to burn a diamond. This small piece of diamond is embedded in plaster of Paris in order to hold it conveniently. In order to burn it it is necessary to heat it to a very high temperature, and as its igniting point is very high, I shall require to light it in a jet of hydrogen in contact with oxygen. You see we have here a jar of oxygen, and when we have ignited the diamond to a sufficient temperature, we will plunge it into this jar, and then I hope it will continue its combustion. It will certainly do so unless we have not heated it sufficiently; but the experiment is rather difficult, unless we take a large diamond, and then it becomes rather a costly experiment. [The diamond was ignited by means of the oxy-hydrogen blow-pipe.] There, our diamond has got up to a white heat. [The diamond was then introduced into the jar of gas.] There is our diamond, burning

as a bright point in the jar of oxygen, and when it has burned a little longer so as to give us an appreciable quantity of carbonic acid—for it burns very slowly—we will test whether we have really produced carbonic acid by this combustion of the diamond. Now, we will take, in the same way, a piece of graphite; in fact, a piece of a Mordan's pencil, and burn it in another similar jar of oxygen. In the meantime we will make sure that this is the jar in which the diamond was burned. Now, I think we shall not find so much difficulty with our graphite in getting it up to the proper temperature. You see that burns more readily than the diamond does; we have not to heat it to so high a temperature, and now it is introduced into the oxygen combustion goes on much more rapidly. The combustion will be very much sooner extinguished, although we have a quantity of graphite, five or six times the weight we had of the diamond. Sufficient carbonic acid has now been produced to make its presence manifest. Finally, we will take a piece of charcoal, which will not require to be heated much. [A piece of charcoal was burned in a jar of oxygen.] You see what a wonderful effect cohesion has upon this chemical process. There we had such difficulty in igniting our hard and compact diamond; we had less difficulty in igniting our graphite, which was less dense than the diamond; and, finally, we had no difficulty in igniting the charcoal. Now, let us see whether we have precisely the same product produced in all these instances. Here is some lime-water which we shall find to be a most delicate test for carbonic acid, and I will pour a little of it into each of these jars. [A quantity of lime-water was poured into the three jars in which the diamond, graphite, and charcoal had been burned.] There, you see, we have produced a quantity of chalk; we have formed carbonic acid from the combustion of that diamond; the carbonic acid has united with the lime, forming the carbonate of lime or chalk. Here is the result of the combustion of the graphite which is identically the same; and in the jar in which the charcoal was burned we have also carbonate of lime precipitated, showing the production of carbonic acid in that jar; so that here we have precisely the same result in all three cases.

NOTICES OF BOOKS.

On the Composition of Oxen, Sheep, and Pigs, and of their Increase whilst Fattening. By J. B. LAWES, F.R.S., and Dr. J. H. GILBERT, F.R.S.

THE pamphlet which has recently appeared under this title sets forth the results of an extended series of analytical experiments in continuation of those already communicated by the same authors to the Royal Agricultural Society. The object has been, in the present instance, to investigate the nature of the increase, and the relative development of the different organs or parts of fattening animals under various conditions of feeding. As a process of "meat manufacture," therefore, where the value of raw materials and the proportionate yield of saleable carcasses have to be considered, the question directly addresses itself to the agriculturist, constituting, indeed, a highly important branch of the business of the farmer; and to the chemist opens up a wide field for investigation in reference to the disposal in the animal economy of the several constituents of food,—to what extent they contribute directly to the formation of flesh,—and what proportion is necessarily lost in the form of offal and waste, or as products of inferior commercial value.

A diligent study of the numerous Tables in the original treatise is requisite to place the inquirer in the full possession of the details of an investigation

so laborious as that undertaken by Messrs. Lawes and Gilbert. The experimental results are drawn up for comparison, and exhibited in twenty-one tabular statements. In these columns the quantity and nature of the food consumed, with the gross increase resulting therefrom,—the proportion subsisting between the several parts of the animal,—the distinctions of offal and carcass,—and (in pigs) the relation of fat to lean, are conspicuously stated. Then, in regard to the chemical composition of the parts of the animal, the distinctions of fatty, nitrogenous, and mineral constituents and water, are deduced from means of several individuals in each class. And, lastly, the increase of the animal whilst fattening is referred, in like manner, to the several constituents of the food consumed.

The main conclusions from the whole inquiry may be briefly enumerated as follows:—

I. Food and Increase.

Fattening oxen fed liberally upon good food, composed of a moderate proportion of "cake" or corn, some hay or straw chaff, with roots, or other succulent food, and, well managed, will, on the average, consume from 12lbs. to 13lbs. of the dry substance of such mixed food per 100lbs. live-weight per week; and should give 1lb. of increase for 12lbs. to 13lbs. of dry substance so consumed.

Sheep fattening, under somewhat similar circumstances, but with a less proportion of hay or straw, will consume about 15lbs. of the dry substance of the mixed foods per 100lbs. live-weight per week, and should yield, over a considerable period of time, 1 part of increase in live-weight for about 9 parts of the dry substance of their food. If the food be of good quality, oxen and sheep may give a maximum amount of increase for a given amount of total dry substance of food, even provided the latter contain as much as 5 parts of total non-nitrogenous to 1 of nitrogenous compounds.

Pigs fed liberally upon food composed chiefly of corn, will consume from 26lbs. to 30lbs. per 100lbs. live-weight per week of the dry substance of such food. They should yield 1 part of increase in live-weight for 4 to 5 parts of the dry substance of the food. They may give a maximum amount of increase for a given amount of dry substance of such food, if it contain as much as 5 or even 6 parts of total non-nitrogenous to 1 of nitrogenous compounds.

Taking into consideration the cost of the foods and the higher value of the manure from those which are rich in nitrogen, it is frequently the most profitable for the farmer to employ,—even up to the end of the feeding process,—a higher proportion of nitrogenous constituents in his stock-foods than is necessary to yield the maximum proportion of increase in live-weight for a given amount of dry substance of food.

II. Proportion of Parts.

In proportion to their weight, sheep yield rather more internal loose fat than oxen, and pigs very much less than either.

As oxen, sheep, and pigs mature and fatten, the internal organs (offal parts) increase in actual weight, but they diminish in proportion to the weight of the animal.

Well-bred and moderately-fattened oxen should yield about 60 per cent. carcass in fasted live-weight, and sheep in similar condition a like amount. Excessively fat oxen may yield from 65 to 70 per cent. carcass.

When the fattening food of oxen, sheep, and pigs contains less than about 5 parts of non-nitrogenous to 1 of nitrogenous compounds, the proportion of gross increase for a given amount of dry substance of the food will not increase with the increased proportion of nitrogenous compounds; the proportion of carcass to the live-weight will probably be somewhat less, and the carcasses themselves will be somewhat more bony and fleshy, and less fat.

III. Chemical Composition of the Animals.

Of dry fat the entire body of a fat calf contained about

14½ per cent.; of a fat ox, 30 per cent.; of a fat sheep, 35½ per cent.; and of a moderately fat pig, 42 per cent.

In the fattened condition, the entire bodies of calves and oxen will probably contain about 4 per cent. of mineral matter; those of lambs and sheep, under 3 per cent.; and those of pigs, on an average, 1½ per cent.

This mineral matter may generally be reckoned to contain nearly 40 per cent. of phosphoric acid, and about 6 per cent. of potash.

The mean composition of six animals analysed in a condition fit for the butcher shows about 3 per cent. mineral matter, 13 per cent. nitrogenous compounds, and 33 per cent. fat; in all, about 49 per cent. total dry substance, and 51 per cent. water.

The proportion of mineral matter in the bodies of oxen, sheep, and pigs rises and falls with that of the nitrogenous compounds.

IV. Composition of Increase.

The increase of liberally-fed oxen over the last six months of the fattening period will probably consist of 70 to 75 per cent. total dry substance, of which 60 to 65 parts will be fat, 7 to 8 parts nitrogenous compounds, and about 1½ part mineral matter.

V. Relation of Constituents in Increase to Constituents Consumed.

Sheep fattening for the butcher on a good mixed diet will seldom carry off more than 3 per cent. of the consumed mineral matter. They will probably carry off less than 5 per cent. of the consumed nitrogen, if the food be comparatively rich, and more than 5 per cent. if it be comparatively poor in nitrogen. They should store up about 10 parts of fat for every 100 parts of non-nitrogenous substance consumed.

Pigs liberally fed on fattening food should store up 20 parts, or more, of fat for every 100 parts of non-nitrogenous substance consumed. These animals were found to store up four or five times as much fat as was supplied ready formed in their food.

If the produced fat were formed from starch, about 2½ parts would be required for the formation of 1 part of fat. In the sense here supposed, if the fat were so formed, about two-thirds of the dry substance of the food would be expired, perspired, or voided, without directly contributing to increase.

The comparative values of our current fattening food-stuffs, as a source of saleable animal increase depend more on their amount of digestible and assimilable non-nitrogenous than on that of the nitrogenous constituents; but, as a source of manure, their value will be the greater the higher their proportion of nitrogenous compounds.

Such are the main points which the authors deduce as the result of their observations. In reviewing these, we are willing to allow that many of the conclusions appear to be fully borne out by the experimental and numerical data urged in their support, and are, moreover, in general accordance with popular opinion. The relation of parts in the animal, and their average rate of increase, would, doubtless, be considered as established on sufficient basis. There are some points, however, to which we should be disposed to take exception, partly by reason of what may be termed the superfluous manner of employing and stating the results. Thus, in Table V., in the last column, are given the absolute weights of the carcass and the several internal organs of the sheep, calculated as a mean of 249 individuals; yet to furnish this mean, or average, 5 sheep in the lean (store) condition are included with 45 others excessively fat; and we find, in consequence, that the item "caul fat," which is shown to vary in the sheep from 2½ lbs. to nearly 10 lbs., according to the condition of the animal at the time of slaughter, is brought out, nevertheless, and made to give a general mean in the last column. The expediency of taking so many as 100 sheep to furnish an average for the "moderately fattened" is, we think, a little questionable.

In reference to the chemical composition of the animal bodies, in their entirety and as separate parts, it is to be regretted that no particulars are afforded explanatory of the analytical methods employed and of the general system of operating. We are not informed whether, for instance, the whole organ (and frequently no less would be satisfactory) was consumed in an examination for mineral constituents; in what manner the relative proportions of nitrogenous and non-nitrogenous matters were estimated. These and other questions suggest themselves as difficulties in the working of chemical processes on a scale sufficiently extensive to ensure trustworthy results. That the authors are themselves fully prepared to admit the difficulty of an investigation such as that attempted is evident, among numerous references, from the paragraph with which we conclude this notice:—

"The results must, indeed, be looked upon as only approximations, though we believe the data now supplied constitute the most reliable basis for estimates of this kind at present at command."

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Iron and Nitride of Iron.—By passing a galvanic current from a Daniell's battery through a pure solution of chloride of iron H. Kramer obtained (*Chemisches Centralblatt*, No. 18, April 24, 1861), on the kathode a thin coating of metal, which easily took a bright polish. On increasing the strength of the current, so that hydrogen was freely evolved, he procured the reduced iron in a spongy mass so soft and porous that it could be flattened to a thin plate by the pressure of the thumb-nail.

The mass reduced by Bottger's method from a mixed solution of protosulphate of iron and sal ammoniac is very different. It is as hard as steel and as brittle as glass, and on examination is found to be not pure iron but nitride of iron. If this mass be rubbed to powder, which its brittleness easily permits, and then is washed until the washings no longer give any precipitate with a silver solution, it still yields when heated with soda-lime, an alkaline gas, which gives a white precipitate when passed into a solution of chloride of mercury. One gramme of the mass gave 1.420, and in a second experiment 1.421, of peroxide of iron, which makes the proportion of nitrogen contained 1.49 per cent. The author thinks that this proof of the ease with which nitrogen combines with iron is confirmatory of Fremy's views of the chemical constitution of steel, and also Schafhaüt's on the composition of English white iron.

A Double Salt of Chloride of Calcium and Carbonate of Lime.—In making a solution of commercial chloride of calcium, Fritzsche observed (*Bulletin de St. Petersburg*, tome iii. p. 285) that a small quantity of sandy powder remained undissolved. As long as the solution of the chloride was concentrated, this powder had a yellowish appearance, but on diluting the solution it became white. On searching for the cause of this change in appearance, the author found that the powder was composed of crystals of a double compound of chloride of calcium and carbonate of lime, and that the alteration in appearance was owing to a decomposition effected by the water. Some of the crystals placed on a glass plate with a little water, and examined immediately by the microscope, were seen at first to be perfectly transparent; but soon the surfaces became cloudy, and a granular separation was apparent. In consequence of the solution of the chloride of calcium they soon lost all their transparency, and after a time there only remained a skeleton of carbonate of lime of the size and shape of the original crystals. Some of the sandy powder separated very carefully from the chloride of calcium was found on

analysis to have the composition $\text{Ca Cl} + 2\text{CaO CO}_2 + 6\text{HO}$.

II. ORGANIC CHEMISTRY.

A New Acid by the Oxidation of Nitro-Benzene.—When nitro-benzene is oxidised by boiling either with a solution of permanganate of potash or a mixture of nitric acid and bichromate of potash, there is found among the products a peculiar acid. The oxidation is most conveniently effected by the last-mentioned means, but care must be taken to have an excess of nitro-benzene. The new acid is soluble in hot nitro-benzene, but separates on cooling in small white crystals, which remain suspended in the solvent. This is separated by decantation, and is then shaken up with an excess of ammonia, which dissolves the new acid and another, forming a deep yellow salt much like picric acid in appearance. The solution is treated with hydrochloric acid, which causes the new acid to deposit. It is then washed with distilled water to remove chloride of ammonium and also some yellow acid which accompanies it. So purified, it has the following properties:—It is colourless, has a sharp, slightly bitter taste, and presents itself in slender needles irregularly grouped together. It fuses at a temperature little above the ordinary, and completely volatilizes, condensing in well-formed brilliant needles. Almost insoluble in cold water, and but little soluble in boiling water, it dissolves freely in alcohol, ether, and nitro-benzene. It dissolves, also, in hot acetic acid, and crystallises out again on cooling. Several analyses of specimens prepared by different methods led the authors (M.M. Cloez and Guignet, *Bulletin de la Société Chimique de Paris*, No. 1, p. 3) to the following as the formula of the new acid:— $\text{C}_{10}\text{H}_7(\text{NO}_4)\text{O}_6$, and induce them to regard it as a product of the oxidation of nitro-cinnamic acid $\text{C}_{10}\text{H}_7(\text{NO}_4)\text{O}_4$. In conclusion, they remark that they have experimented with commercial nitro-benzene, and the new acid may come of the oxidation of some foreign body in that article.

Preparation of Aniline Red by Arsenic Acid.—We may add the following (*Polytechn. Centralblatt*, 1861, s. 493) to the account we gave of this process at p. 292, vol. ii. It seems that if an excess of aniline be employed no result is obtained until this excess has distilled over. If, on separating this colour afterwards, it is found to be contaminated with tarry matter, it is best removed by mixing the whole with sand, and then adding a little hot water, and filtering. Sometimes it happens that the dyer fails in using aniline red, because the fuchsine has got combined with ammonia. In such a case the addition of a little acetic or any other acid will make the bath again available. Gerber-Keller (*Ibid.*, s. 398) has repeated Dr. Hofmann's experiments on the action of bichloride of carbon on aniline (See *CHEMICAL NEWS*, vol. ii. p. 292) with exactly the same results. He obtained an oily or doughy mass, which in time changed into a hard, brittle product, according to the temperature and strength of the action. Between 180° and 195° the author obtained in every experiment with Hofmann's process a magnificent crimson-red dye, which is without doubt identical with the Fuchsine of M.M. Verguin and Rénard, Brothers. The chloride of carbon acts on aniline, but very slowly,—even at high temperatures,—while the production of azaleine with certain easily-reducible oxygen salts takes place in a few hours. Dollfus-Galline confirms the foregoing, but remarks that the colour prepared by Hofmann's method has a yellowish tone. In general, he says, the aniline reds made with chlorine compounds have a vermilion tint, while those prepared with oxygen salts have a violet. Gerber prepares his azaleine at a temperature which never exceeds 109°C . In Hofmann's method 200° must be employed, but this need be no obstacle to its technical application. The author thinks it probable that a purer red may be obtained by this than by any other process.

MISCELLANEOUS.

SACRIFICIAL METALS.

A curious statement made in the House of Commons, a few days ago, induces us to offer a few remarks on what may be called, appropriately enough, "the sacrificial function in metals." It was stated, our readers will remember, that the leaden envelope with which the Armstrong shell is surrounded, and which is absolutely necessary as a constituent part of that missile, might be caused to adhere securely, either by "a sort of mortise" or by the intermediation of zinc. The second expedient is that indeed adopted; with what amount of success the recent practice of these guns in China would disclose, were the Government not so pertinaciously bent on holding back the communications which have reached them on this matter.

It is a fact perfectly well known to chemists, but one which mechanicians have not heeded nearly so much as it deserves, that when two metals are retained in contact, and conjointly exposed to chemical influences during long periods of time, one metal sacrifices itself to ensure the other's preservation. The history of mechanical construction furnishes numerous illustrations of this chemical fact, as will be presently seen, when we come to cite a few examples; the marvel, then, is, that Sir William Armstrong—or, rather, the laboratory branch of the War Department, before whose cognizance the proposition of zinc soldering (thus to designate it) must have come—could ever have sanctioned an expedient which must necessarily fail in the end, like every other expedient which violates a law of nature. The history of ships' copper sheathing shall be taken as our first illustration of the sacrificial metallic function. Sir Humphry Davy, as is well known, devised a method of checking, or altogether obviating, the destruction of ships' copper sheathing. He accomplished this by attaching to the ship's bottom, at suitable intervals, slabs of the metal zinc, and which he called protectors. Protectors they were, in the following manner:—The zinc rapidly corroded, and was lost; but, so long as any zinc remained for the sea-water to act upon, the copper remained untouched. So far as copper preservation is concerned, the method adopted must be pronounced efficient. Practically, it failed, indeed, to secure the advantages described, but not for any reason that concerns us here. It was found that when copper no longer slowly dissolved, it ceased to be a poisonous metal. Barnacles and sea-weed attached themselves, just as they might have done to an uncoppered wooden bottom; and, moreover, owing to a galvanic action set up, the ship's copper was rapidly fouled by a deposition of magnesia and lime, precipitated from the magnesian and calcareous soluble salts always present in sea-water. Failing, then, to achieve what was intended of it, the copper-protecting process of Davy ceased to be employed; but the failure of it is that which alone concerns us here, as illustrating what we would wish to convey by the words "sacrificial metal."

Take, as the next example, the illustration afforded by the setting of an iron paling-rail in a bed of lead. The most casual observation, as one passes along the streets of this city, may be made more pregnant with fact bearing upon this matter than the longest homily. For a time, varying as to length with locality, external influences, and—perhaps, in some degree—on the varying quality of the iron and lead brought into contact, both metals remain sound. But, after a time, decay inevitably sets in, and, when once commenced, marches to the issue of final destruction with wonderful rapidity. The remarkable fact is, that both metals do not decay simultaneously; it is the iron which corrodes, whilst lead—the softer metal—

remains intact. Let the mechanic do what he will, this result cannot be prevented—a law of nature having decreed the sacrifice, it must and will be achieved.

Our next illustration shall be ordinary tin plate—iron, coated with tin, as is well known. Well, what sort of destruction is set up when tin plate has been exposed for a long duration of time to the atmosphere? Is the tin destroyed—does it tarnish even? Never. Iron is the sacrificial metal here; and, as surely as the iron is anywhere exposed, it perishes, crumbles, and dissolves away, with destructive rapidity. The rate of destruction of iron totally unprotected is slow, by comparison with that which ensues when, owing to a flaw or disintegration of the covering tin envelope, atmospheric agencies come into play upon the underlying metal.

But the case we most particularly wish to direct attention to, is the result of bringing zinc into contact with iron, and retaining the two metals together over long periods, as may be seen in the so-called galvanized iron. Under this latter disposition, zinc becomes the sacrificial metal. Not one particle of iron decays, so long as atmospheric destructive agents can wreak their dissolution—so to speak—on the protective zinc. Nor does this protection altogether depend on a complete covering of the iron. Flaws and imperfections there are, and necessarily must be, through which, quite down to the iron, destructive agents, always present in the atmosphere, must penetrate. They do not act upon the iron, nevertheless, so long as a sufficient expanse of protective zinc surface remains; and this simply because a fundamental law of nature forbids their doing so.

Now, if our remarks be true—and we are content to appeal to chemical science for a verdict—what becomes of the efficiency claimed for the Armstrong shell-envelope, because of the attachment of lead by a process of zinc soldering? Sound, to the eye, these projectiles may at first seem; but a disruptive force will be acting upon them, nevertheless, from the very beginning; and in total ruin and destruction this force will eventuate at last; whatsoever Sir William Armstrong's advocates may assert, notwithstanding—whether at Elswick or at Woolwich, whether in Parliament or out of it.—*Mechanics' Magazine*.

Royal Institution.—The following is the arrangement of Lectures for the ensuing week:—Tuesday, May 14, at 3 o'clock, Mr. John Hullah, "On the History of Modern Music." Thursday, May 16, 3 o'clock, Mr. W. Pengelly, "On the Devonian Age of the World." Friday, May 17, 8 o'clock, Professor J. Clerk Maxwell, "On the Theory of Three Primary Colours." Saturday May 18, 3 o'clock, Professor Max Müller, "On the Science of Language."

Chemical Society.—The next meeting of this Society will be held on May 16, when there will be a discourse delivered by W. H. Perkin, Esq., "On the Colouring Matters obtained from Coal-tar."

Royal Institution of Great Britain.—At the General Monthly Meeting, held on Monday, May 6, 1861, Sir Henry Holland, Bart., M.D., F.R.S., in the Chair, the Hon. Lieut.-Gen. Thomas Ashburnham, C.B., Lieut.-Colonel W. MacGeorge, and William Reid, Esq., were elected Members of the Royal Institution; the Duke of Chartres, the Count of Paris, Stephen Goodfellow, Esq., M.A., and Alexander Mackintosh, Esq., were admitted Members of the Royal Institution. The following Professors were re-elected:—William Thomas Brande, Esq., D.C.L., F.R.S., Hon. Professor of Chemistry, John Tyndall, Esq., F.R.S., as Professor of Natural Philosophy. The presents received since the last meeting were laid on the table, and the thanks of the Members returned for the same.

To Unite Stone and Glass.—M. Bru, Curator of the Museum at Narbonne, has discovered that silicate of potash possesses, in the highest degree, the property of uniting surfaces of stone, glass, and pottery. It is applied with a brush to the surfaces which it is desired to bring into contact, and in a few days acquires a great solidity. It appears that the same material can also be successfully used in joinery, and for all the purposes to which common glue is applied. This discovery, which promises to be of considerable importance, was announced by M. Bru to M. Figuier, the Editor of *L'Année Scientifique et Industrielle*, the volume of which work for the present year contains, at p. 481, a paper on the subject, and a letter from M. Bru to the editor.—*Builder*.

Treating Iron in Japan.—A literary friend sends me the following:—"I have just stumbled on the following in 'Mandelslo's Travels in the Indies,' at p. 190, 'They (the Japanese) have, amongst others, a particular invention for the melting of iron without the using of any fire, casting it into a tun, done about on the inside with about half a foot of earth, where they kept it hot with continual blowing, and take it out by ladels full to give it what form they please, much better and more artificially than the inhabitants of Liège are able to do.'" This would really seem to describe, in an imperfect manner, the fact that Bessemer's discovery has been old knowledge for ages in the "far Cathay," and that, like printing and gunpowder, the knowledge of them has only been communicated to, or re-discovered by, his younger brother Japhet. When a boy I myself recollect well a country blacksmith, in the North of Ireland, who used char peat as fuel, showing me, as a curious experiment, that, by taking a tolerably large flat bar of iron from the fire, when at that temperature above the welding heat when its angles liquefy and begin to scintillate and burn off, and then at once blowing upon the bar strongly with a pair of hand or house bellows, the whole mass, for a few seconds, burnt and flew about like a firework, doing so until the stream of air cooled it down too low. I had little notion what fruitful truths lay hid in that little exhibition. These notices of the "heatings in the dark," as Bacon calls such heraldings of every great discovery, may be of interest thus to record for some future historian of iron metallurgy, by whom Bessemer's name will be undoubtedly honoured.—*Robert Mallet*.

Hermised India-rubber.—India-rubber has been known about a hundred years, and only within the last twenty years has it been found of much practical use. In the town of Beverly, Mass., for some years past, there was a manufacturing company than used a devulcanizing process, taking old rubber and making it up chiefly into india-rubber cloth, under a patent with which Goodyear's did not interfere. And for the last four years they have been working the raw rubber by a process of vulcanization without the use of heat. The rubber is put into a solution of chloride of sulphur and sulphuret of carbon, and the change is effected in its properties in a few minutes. This process is called "hermising," to distinguish it from vulcanizing. The patent is Mr. Parmalee's. The hermised rubber possesses substantially the same properties as the vulcanized. It has the advantage, however, of being made of a lighter colour, and therefore of receiving quite brilliant tints. The lighter colour arises from the fact of rubber being less exposed to the atmosphere during the process, which exposure in the ordinary process turns the product almost black; and to make it lighter, zinc white or some other kind of white material is required to be added. India-rubber springs are passing out of use; it being found that the jolting of the vehicle causes the india-rubber at last to lose its elasticity. Vulcanized rubber subjected to the action of steam for the space of about three hours becomes perfectly brittle.

The New Zealand Iron Sand.—The following correspondence on this subject has taken place in the columns of the *Engineer*:—Mr. Mushet writes: "Sir,—Having received from Captain Morshead, for the purpose of converting into steel, about three tons of the Taranaki metalliferous sand, nothing surprised me more than the almost perfect purity of this sand; of which 98·25 per cent was strongly attracted by the magnet, and 1·75 per cent. only of quartz sand remained unacted upon. The analysis of this ore by Messrs. Johnson and Matthey showed the ore to be composed of:—

Oxide of iron	88·45
Titanic acid	11·43
Loss	·12

100·00

Recently I have received from a party not interested in the matter, a fair sample of the Taranaki titanium sand, and which I find is composed as under:—

Magnetic iserine sand	33·60
Non-magnetic ilmenite sand	61·27
Sea sand	5·13

100·00

And according to the analyses given of magnetic iserine and non-magnetic ilmenite, the Taranaki sand as it lies on the shore is composed as under:—

Oxides of iron	61·14
Titanic oxide	29·73
Sand	5·13

100·00

And the yield from the ore in metallic iron will only amount to about 49 per cent. From another source, much nearer home, I have been supplied with a similar titaniferous sand of precisely similar quality; so that I infer that Captain Morshead has been misled as to the purity and richness in iron of the Taranaki sand, which, as supplied to me, appears to have been separated by a magnet from the non-magnetic bulk of the deposit. Or are there two deposits of titaniferous sand at Taranaki, one pure and the other impure? Perhaps Captain Morshead will be kind enough to explain. It will make a most essential difference in the success of the smelting operations which may be undertaken, whether the ore employed be the pure magnetic iserine or the mixed ore consisting chiefly of non-magnetic ilmenite." In reply to this, Captain Morshead writes as follows:—"Sir,—I can have no hesitation in affording to Mr. Mushet, through your columns, the explanation he seeks in his letter of the 20th March, and published by you on March 22. The sand I forwarded to him was, if anything, below a fair average sample, as could be vouched, if necessary, by four or five 'parties,' to use the words of Mr. Mushet, 'not interested in the matter.' There are not two deposits at Taranaki, one pure and the other impure, as suggested by Mr. Mushet. The whole is pure. I did not separate the three tons I gave Mr. Mushet (which were a portion of about ten tons sent over to me) from any other sand by a magnet. The suggestion of picking up such a quantity by such a process is a simple absurdity. If anyone will take the trouble to compare Mr. Mushet's present letter with those formerly written by him on the same subject, he will readily discover that Mr. Mushet, having failed to attain some end proposed by himself at the beginning of our acquaintance, is now seeking to destroy the effects of his former letters, and injure a project he can no longer control. For the present, I am content to rest upon the analysis of the material made (not for me, or at my request) by Messrs. Johnson and Matthey, corroborated as it is by the certificate contained in Mr. Mushet's last letter, of 'the almost perfect purity of the sand,' from which, I am glad to inform him, that a steel,

far surpassing any made by him, has been manufactured by other parties, and which will shortly be introduced, in large quantities, into the market."

Vapour from Water.—Take a glass bottle of very clear glass and well cleaned with sulphuric acid, and afterwards with clean warm water, then place the bottle on a stand or block of wood, and let the rays of a strong light through a lens fall in the bottle, so that the focus of the lens is about in the middle or near the side of the bottle where you want to observe the vapours; when this is done pour a little hot water in the bottle and close it with a loose stopper, then the vapours can be seen very distinctly with a common magnifying glass or microscope with a long focus placed in an angle of about 150 deg. to 170 deg. with the rays of the light. Should condensation take place on the sides of the bottle a little shaking will take it away. If the experiment is made in a warm room, and the bottle well cleaned, as before described, condensation will not be any annoyance. Then raise the bottle so that the rays of the light fall over the surface of the water, and, by careful watching, the vapours can be seen rising from the surface of the water, and also disappearing on the surface of the water. I have seen them to rise from the water, and falling back again, and floating a short time on the surface, then disappear, as a drop of water will do sometimes. Here the vapours are in a restless state, but if the water is poured out of the bottle and placed on the stand again then the vapours will come more to rest. I believe that any person, after observing this, will come to the conclusion that they are water, therefore they cannot be formed in the body of water. If all vapours have disappeared by lowering the temperature above the surface of water a great quantity of vapours are produced in the bottle (miniature fog). This can be done by blowing cold air, or introducing a cold body or a few drops of cold water.—*F. Hoffmann, in the Engineer.*

ANSWERS TO CORRESPONDENTS.

* * In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

* * All Editorial Communications are to be addressed to Mr. CHOCKER, and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the *CHEMICAL NEWS*, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

Metallic.—We do not know where it is to be procured. An advertisement in our columns would at once bring you the desired information.

Arseniuiretted Hydrogen.—*J. O.*—You can prepare this gas by forming an alloy of 32·1 parts by weight of zinc and 37·6 parts metallic arsenic; upon acting on this with hydrochloric acid, pure arseniuiretted hydrogen is evolved, and may be collected over water. The most scrupulous care must be taken that not the smallest quantity be inhaled as it is a deadly poison.

Pink Manganese Salts.—*Enquirer.*—The pink colour of the salts of manganese is generally due to the presence of a higher oxide; sometimes, however, a trace of cobalt is present which causes the colour. The addition of a bitter sulphurous acid at once destroys the colour, if due to a higher oxide of manganese, whilst it is without action on the colour produced by cobalt solution.

Chrome-Green.—*W. D.*—An account of this colour was given in the First Volume of the *CHEMICAL NEWS*, p. 94.

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SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Colour-tests for Strychnia and the Diagnosis of the Alkaloids, by WILLIAM A. GUY, M.B. Cantab., Fellow of the College, and Professor of Forensic Medicine, King's College, London.

THE observations which I am about to make on the colour-tests for strychnia formed part of the three Croonian Lectures on Tabular Analysis, given at the College of Physicians at the end of February and beginning of March of this year. In those Lectures, one form of tabular analysis was illustrated by a table of the alkaloids, and as the construction of the table grew out of a series of experiments on strychnia, and especially on the colour-tests for that important poison, it was necessary to enter into some details respecting those tests. The questions which I proposed to myself for solution were the following:—

1. The best form and mode of application of the colour-tests.
2. Of the colour-tests, which is to be preferred?
3. Are the colour-tests, or is the selected test open to any serious objection?
4. Is it possible, by means of the colour tests, or by any simple modification of them, to distinguish the alkaloids from each other?

Before I proceed to discuss these questions, I must premise that most of the specimens of strychnia and of the other alkaloids on which I experimented were supplied to me by the Messrs. Morson. The majority of them were presented to me by them as specimens of approved purity; the remainder were procured from them from time to time. For a few specimens I was indebted to my colleague, Professor Bentley. Several of the alkaloids in Mr. Morson's series are crystals of large size and great beauty, and the remainder are evidently of the best quality; of some of the more important alkaloids, such as strychnia, brucia, and aconitina, I have more than one specimen. When the results which I obtained did not happen to agree with the descriptions of approved authorities, I made experiments with other specimens, and endeavoured to ascertain the reason of the discrepancy. I may add, that the results stated in these papers are based upon several hundreds of experiments carefully performed, and the reactions noted down at the time. In order to guard against any possible misapprehension, it may be well to state that the term "colour-tests" is used throughout these communications in the sense of tests producing a series, or succession, of colours; and that it is further restricted to the chemical colour-tests as distinguished from the galvanic colour-test.

¹ Substance of part of the Croonian Lectures for 1861, delivered at the Royal College of Physicians; from the *Pharmaceutical Journal*.

1. Form and Mode of Application of the Colour-tests.—The chemical colour-tests for strychnia consist in the peculiar reactions of the alkaloid with sulphuric acid, plus one or other of the following substances:—The bichromate of potash and the ferricyanide of potassium, and the peroxides of manganese and lead. To these may be added, as less generally recognised, the chromate of potash and chromic acid; and it may be well to state that the chromates of soda and ammonia yield characteristic reactions.

To the four recognised colour-tests (the bichromate of potash, the ferricyanide of potassium, the peroxide of manganese, and the peroxide of lead) I propose to add the permanganate of potash.

In order fully to appreciate the action of these tests, it should be borne in mind:—

1. That sulphuric acid, of the strength and degree of purity commonly met with in the shops, when added to, and mixed with ordinary specimens of strychnia, does not produce in them any change of colour.

2. That the acid does not occasion any change of colour in the peroxide of manganese or the peroxide of lead.

3. That the acid, when mixed with a fragment of the bichromate of potash, produces a yellow solution, which passes gradually to a yellow-brown and clear orange.

4. That the acid, when mixed with a fragment of the ferricyanide of potassium, gives a lemon-yellow solution, becoming tinged with blue after long exposure.

5. That the permanganate of potash, treated in the same way, yields with the acid a light green solution, deepening in tint by degrees; then assuming a pink tint at the margin; then, after longer exposure, becoming brown at the centre and lilac at the circumference; and, finally, after still longer exposure, presenting a rich scarlet at the centre and a brown at the circumference.

Now, as the succession of colours produced when strychnia, sulphuric acid, and either of the five substances just specified are brought into contact, are entirely different from those occasioned by the action of sulphuric acid on the five substances in question, and as, moreover, the strychnia, colours develop themselves very speedily, there is no valid objection to the use of these substances as tests. The yellow tint of the acid solution of the bichromate of potash, the lemon-yellow tint of the acid solution of the ferricyanide of potassium, and the light green tint of the acid solution of the permanganate of potash, offer no impediment to the development of the characteristic strychnia colours, and cannot possibly be confounded with them. The same observation applies equally to the mixture of sulphuric acid with the solutions of these salts. When solutions of the strength of ten grains to the ounce of distilled water are added to equal quantities of strong sulphuric acid, the bichromate of potash yields an orange coloured mixture, the ferricyanide of potassium a lemon-yellow, and the permanganate of potash a red-brown. The tints vary with the quantity of the acid and of the

solutions, and they undergo some change on long exposure; but it may be safely alleged that the colours thus produced do not interfere with the production of the characteristic strychnia colours, and that they cannot possibly be confounded with them. But as for medico-legal purposes, it is well to avoid all objections, however captious, the peroxide of manganese, or the peroxide of lead, may claim to be preferred if, on further examination, they should be found to possess other properties entitling them to a preference.

Having shown that strychnia when treated with sulphuric acid undergoes no change of colour, and that the several tests (the peroxide of lead, the peroxide of manganese, the bichromate of potash, the ferricyanide of potassium, and the permanganate of potash) undergo either no change of colour, or such changes only as do not admit of being confounded with the strychnia colours, I proceed to consider in succession,—1. The best way of applying the colour-tests; 2. The best order in which to use the re-agents; and, 3. The best form in which to apply the colour-developing tests.

1. The colour-tests are best shown on slabs of glass coated with white arsenical enamel by the process technically called "flashing."³ But the lids of porcelain crucibles, or white plates, or fragments of any white ware, will answer the purpose very well. If the enamelled glass is used, the tests should be applied to the glass surface. In using the tests scrupulous cleanliness should be observed, and the sulphuric acid and colour-developing tests, if in solution, are best applied by the drop-bottle, as more delicate, and more secure from any soil than the common glass rod. For mixing the several substances together, a glass spatula is to be preferred to a glass rod.

2. The order in which the sulphuric acid and the colour-developing tests should be applied is easily determined by one or two simple considerations. Good and characteristic results may be obtained by mixing the sulphuric acid with the colour-developing tests, and then adding a crystal of strychnia. When so mixed with small quantities of the peroxides of manganese and lead, the acid solution has a neutral tint, which in no way interferes with the full development of the strychnia colours; and even the acid mixtures of the other colour-developing tests are not open to any fallacy if the alkaloid is brought into contact with them as soon as they are made, and before the colours first produced are deepened or changed by exposure to the air. But the method of procedure which begins with dissolving the alkaloid in the acid and then applying the colour-developing tests is to be preferred, inasmuch as the absence of colour in the acid solution is some advantage in itself, as giving full effect to the strychnia colours, while it has the still greater advantage of distinguishing strychnia from several alkaloids (poisonous and non-poisonous), which yield colours with the acid without the addition of the colour-developing substances.

3. The best form in which to apply the colour tests for strychnia is a subject of considerable practical importance, and it is one upon which there is at present no distinct agreement among chemists. Some authorities prescribe the application of the colour-developing substances in the solid form, some in solution, and some who prefer the state of solution use strong solutions, while others employ weak ones. The balance of authority is certainly in favour of applying the colour-developing

tests in the solid form, and my own experience is favourable to this mode of application. It is also commended by the obvious consideration that solution in water is favourable to a rapid development of the changing colours which constitute the test, such quick change of colour being among the most serious objections to the colour tests. At any rate, it must be admitted to be of the first importance to secure as slow a change of colour as possible, in order to give time for the observation of the colours actually produced.

The superiority of the solid form to the solution is easily shown by a comparative experiment. If we take two minute fragments of bichromate of potash of equal size, place them on a slab of white enamelled glass or porcelain, add to one of the fragments a drop of distilled water, and dissolve it in the water, and leave the other untouched; then place on the slab two equal drops of the same solution of strychnia in sulphuric acid, we shall find that on bringing the fragment of bichromate of potash into contact with the acid solution of strychnia, its colour changes from yellow to blue, and when stirred into the liquid with a glass rod or glass spatula yields a rich blue solution, which retains this colour long enough for easy and complete identification, and then gradually assumes the other tints to be presently described, retaining the final tint for hours or days. While, on the other hand, the solution of the bichromate of potash, when brought into contact with the acid solution of strychnia, yields a blue colour at the point of contact, which colour quickly disappears on mixing the two liquids with each other. When, therefore, we make use of the colour-developing tests in substance, we obtain highly characteristic and lasting colours, but when we use them in solution one evanescent colour only. It is true that if to the mixture of the acid solution of strychnia with the watery solution of bichromate of potash we add a drop or two of strong sulphuric acid we obtain a blue liquid, which undergoes the characteristic changes of colour; but the important fact remains that, other things being equal, solution in water greatly impairs the delicacy of the colour tests. If in this comparative experiment we substitute for the bichromate of potash the other soluble colour-developing substances—the ferricyanide of potassium and the permanganate of potash—we obtain the same results.

The injurious effect of dilution with water upon the colour tests is differently shown in the following comparative experiment:—I placed on a porcelain slab four equal drops of the same solution of strychnia in strong sulphuric acid. To two of the four drops I added a drop of distilled water, and then to all the drops, diluted and undiluted, equal minute fragments of the bichromate of potash. The undiluted drops gave highly characteristic results—the usual succession of rich strychnia colours; while the diluted spots gave no indication of the presence of strychnia, but merely a yellow solution. The same comparative experiment gave the same results with the ferricyanide of potassium, the permanganate of potash, and the peroxides of manganese and lead. The ferricyanide gave a lemon-yellow, and the permanganate of potash a pink, solution; while the peroxides of manganese and lead imparted a neutral tint. In multiplying this class of experiments I have now and then obtained a characteristic reaction, and occasionally an evanescent streak of blue; but these were exceptional cases.

The strength of the solution of strychnia in sulphuric acid used in both these series of experiments was one grain in 200 drops; and as the same pipette was used in

³ I have been in the habit of using this enamelled glass for colour-tests for several years. Oblong slabs of the kind described in the text may be obtained at Powell's Glass Works, Whitefriars.

preparing the solution and in the experiments, each drop contained the two-hundredth of a grain of strychnia.

Now, these experiments, though they cannot be taken to prove that the dilution with water of the acid solution of the alkaloid, or the application of the colour-developing substance in solution, must be fatal to the success of the colour tests, whatever the quantity of the alkaloid or of the acid, they do certainly demonstrate the great advantage attending the use of the colour-developing substances in the solid state. The experiments, it will be observed, are strictly logical and comparative. The distilled water added to the acid solution of the alkaloid, or to the colour-developing substance, was the only element of variation in the two series of experiments.

That the indistinct, or wholly negative, results obtained when distilled water is added are not due to mere dilution, or increase in the quantity of liquid present, is shown by the following experiment:—I placed five equal drops of the same solution of strychnia in sulphuric acid (1-200th grain in the drop) on each of three porcelain slabs. I made no addition to the first drop, but added to the second drop one drop of strong sulphuric acid; to the third, two drops; to the fourth, three drops; and to the fifth, four drops. I then added to each spot on the first slab an equal fragment of bichromate of potash; to each spot on the second slab, an equal fragment of peroxide of manganese; and to each spot on the third slab, an equal fragment of the permanganate of potash. The reactions were perfect and characteristic in each case, the colours differing only in depth of tint, but not in proportion to the degree of dilution. The peroxide of manganese was remarkable for the duration of the colours, and the permanganate of potash for their brilliancy.

Having now, as I believe, shown the superiority of the colour-developing tests in substance to the same tests in solution, and demonstrated the disadvantage of introducing into the colour tests the element of dilution with water, I shall reserve for another communication the question of the effect of the quantity of the colour-developing substances on the success of the colour tests, as well as the answer to the three remaining questions.

(To be continued.)

Further Remarks on the Supposed New Metalloid,
by WILLIAM CROOKES.

ASSUMING that further researches on this subject will confirm the correctness of the opinion which I expressed in a former paper,¹ and that the body there introduced to the notice of chemists will prove to be a new member of the large and increasing family of elementary bodies, I have thought it best to give in the present article a few additional observations which I have since made, and also to propose for it the provisional name of *Thallium*, from the Greek *θαλλός*, or Latin *thallus*, a budding twig, — a word which is frequently employed to express the beautiful green tint of young vegetation; and which I have chosen as the green line which it communicates to the spectrum recalls with peculiar vividness the fresh colour of vegetation at the present time.

I have not succeeded in finding this body in any selenium or tellurium ores which I have examined; but two or three specimens of native sulphur, especially some from Lipari, have yielded it, the latter in such

abundance that if the mineralogical specimen which I examined is a fair sample of what this sulphur is like on the large scale, it could easily be employed as a source of thallium in quantity. I have also found small traces of it in some crude sulphur sublimed from Spanish pyrites, for which I am indebted to the kindness of Mr. Thornthwaite.

The plan which I have found most effectual for separating thallium from the associated bodies is the following:—

Finely powder the ore (selenium deposit or native sulphur, &c.) and mix it with its own weight of dried carbonate of soda and half its weight of nitrate of potash; mix thoroughly, and project in small quantities at a time into a red-hot earthen crucible. When it has all been added, keep at a red heat until it fuses quietly to a liquid, when pour it out on to an iron plate. When cool, powder it and exhaust with boiling water until all the soluble portions are extracted. Filter, and add to the filtrate excess of NH_4O and NH_4S ; boil and filter. The precipitated sulphides are now to be boiled in a solution of one part commercial KCy to eight of water, until no more dissolves. Collect the residue on a filter, and wash well. Dissolve this residue, insoluble in KCy , in hot NO_2Cl ; dilute with water and filter if necessary. Add NH_4O in excess, then $\text{NH}_4\text{O.O}$ and boil. Filter, and add HS . Boil for some time, keeping the solution alkaline with NH_4O and smelling of HS . A dark brown precipitate will be produced, which gradually settles to a heavy black powder. Filter this off, and wash it well.

A portion of this, so minute as to be almost imperceptible to the naked eye, introduced into the blue gas-flame of the spectrum apparatus gives rise to a green line of extraordinary purity and intensity, a piece the size of a small pin's head being most dazzling, and quite equal to the yellow line of sodium in brilliancy. From the purity of this spectral appearance, I am inclined to think that the precipitate is thallium itself, uncombined with other bodies, and reduced by the HS from the state of oxide. This, however, is only conjecture. The quantity of the precipitate which I now have is far too small to settle the point by direct experiment. I am at present endeavouring to find a source of thallium which will yield it in quantity, and if, as I hope to be, successful, I propose giving further particulars at a future time.

PHYSICAL SCIENCE.

Early Researches on the Spectra of Artificial Light.

WE print below a most important paper connected with the above subject. We cannot, of course, give the coloured diagrams with which it was originally illustrated; but we can assure our readers that, after making allowance for the imperfect state of chromolithography sixteen years ago, the diagrams of the spectra given by Professor Miller are *more accurate* in several respects than the coloured spectra figured in recent numbers of the scientific periodicals.

In corroboration of this we need only refer to the drawing of the calcium spectrum as given by Professor Miller. In it is prominently given the dark blue or indigo line, which has not been noticed by Bunsen and Kirchhoff, and the omission of which from their coloured representations gave rise, a few months ago, to the announcement of the existence of a new metal of the

¹ On the Existence of a New Element, probably of the Sulphur Group. By William Crookes. CHEMICAL NEWS, Vol. III, p. 193, and *Philosophical Magazine*, for April, 1861.

calcium group.¹ In the description of the calcium spectrum given below it will likewise be seen that Professor Miller states that there is a bright streak in the indigo.

*Experiments and Observations on some Cases of Lines in the Prismatic Spectrum Produced by the Passage of Light through Coloured Vapours and Gases, and from Certain Coloured Flames, by W. A. MILLER, M.D., F.R.S., Professor of Chemistry in King's College, London.*²

The inquiry into the cause of the dark lines in the solar spectrum is one involved in so much difficulty and obscurity, and the interest which attaches to its solution, in connection with some of the most refined questions relating to the nature of light is so great, that I trust I shall be pardoned for bringing forward some additional facts bearing upon the subject, however little I may have to say in explanation of them. These facts may, however, at a future period, assist us in arriving at some general conclusion.

In examining the prismatic spectrum of light that had been transmitted through the deep red vapours of the nitrous acid (NO₂), Sir D. Brewster made the remarkable discovery, that the absorption of the rays by this medium differs from that of coloured liquids and solids in general, in the production of a multitude of lines crossing the spectrum in the same direction as those observed by Fraunhofer; these lines being broadest, darkest, and most abundant towards the more refrangible extremity of the spectrum. He found them to occur equally, whether solar or artificial light were employed. Shortly after, Professor Miller, of Cambridge, and the late Professor Daniell, of King's College, extended this inquiry to other coloured vapours,—viz., to chlorine, iodine, bromine, euchlorine, and indigo. Between the spectra of iodine and bromine they discovered a very exact parallelism, the coloured image in each case being crossed through a great portion of its extent by numerous equidistant lines, corresponding both in number and position in the two substances.

This remarkable similarity of action upon light, between bodies whose chemical properties are in many respects so closely allied, induced me, as the subject appeared to have been completely laid aside by the philosophers to whom these observations are due, to examine a variety of other coloured compounds. I was in hopes of discovering amongst other bodies, and particularly amongst compounds of similar nature and properties, a correspondence, if not in the colorific position, at least in the general arrangement of the lines (if any were produced), which are rendered visible by a prismatic analysis of the light that has been transmitted through the different coloured media. This hope, however, I have not been able to realise to any considerable extent. But though the results obtained are many of them negative, as the cases in which no lines occur are much more numerous than those in which they are produced, I have met with some facts which I believe are not generally known, and which may, therefore, possess sufficient interest to warrant my submitting them to the consideration of men of science. The mere enumeration of the substances in which I have failed to

discover them may, besides saving others some fruitless labour, be of some value.

My observations may be referred principally to two heads,—those which relate to the effect of coloured gases and vapours upon transmitted light, and those which arise from experiments on the spectra produced by flames of different colours.

Before detailing the mode in which the following experiments were conducted, I will briefly state the principal results which I have obtained from the first series.

1. In no case where colourless gases are employed have I detected any additional lines. Fourteen different substances were employed, varying in density from that of hydrogen, 0.0691, to that of ether, 2.586, and hydriodic acid, 4.388. Of these bodies three were simple, viz., oxygen, hydrogen, and nitrogen, and eleven were compounds, varying greatly in their mode of condensation, as in binoxide of nitrogen, where the elements unite without condensation; protoxide of nitrogen, where three volumes of the gases become two; and ammonia, in which two volumes are condensed into one; in chemical properties they differed not less widely. Five of these gases were acid, the carbonic, hydrosulphuric, hydrochloric, hydriodic, and sulphurous acid. One ammonia is alkaline, one cyanogen is usually considered an organic radical, one a compound of such a radical, as ether; the remaining one, chloride of sulphur, possesses properties referable to none of these heads.

2. The mere presence of colour is no indication of the existence of lines. Vapours which the eye cannot distinguish from each other will be found in one case, as with bromine, to give numerous lines; in another, as the chloride of tungsten, to give none at all.

3. The probable position of the lines cannot be inferred from the colour of the gas. With the green perchloride of manganese the lines are most abundant in the green, whilst with the red vapours of nitrous acid they increase in number and density as we advance towards the blue end of the spectrum.

4. Simple bodies, as well as compounds, may produce lines; and two simple bodies which singly do not produce them may in their compounds occasion them abundantly. Neither oxygen, nitrogen, nor chlorine, when uncombined, occasions lines; but some of the oxides, both of nitrogen and of chlorine, exhibit the phenomena in a most striking manner. There are, however, oxides, both of nitrogen and of chlorine, which do not occasion the appearance of lines.

5. We find, also, that lines may exist in the vapour of simple substances, as in iodine, which disappear in their compounds. This is exemplified in the hydriodic acid, a combination of equal volumes of gaseous iodine and hydrogen, united without condensation.

6. Sometimes the same lines are produced by different degrees of oxidation of the same substances, a remarkable instance of which is furnished in the oxides of chlorine. Here chemical considerations of interest may assist in explaining the cause.

7. The lines are increased in number and density by increasing the depth of the coloured stratum through which the light is transmitted, or by any cause which increases the intensity of the colour, proving that more lines exist than our instruments or eyes are capable of discerning when the vapour is dilute. This is especially exemplified with iodine and bromine.

8. These lines occur both with polarised and non-polarised light. There is little doubt, although I have not yet made the experiment, that the lines thus produced,

¹ Vide CHEMICAL NEWS, Vol. III. pp. 116 and 129.

² Read (June 21, 1845) at the Meeting of the British Association held at Cambridge, and communicated by the Author to the *London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science*, for August, 1845.

as with the ordinary solar lines, indicate an absence of chemical influence, as well as of the luminiferous portion of the ray.

In all the experiments on transmitted light, the luminous source employed was, unless otherwise specified, the diffused light of day. The different spectra represented in Plate II. were obtained by receiving the rays upon a prism of Munich flint glass, mounted upon Fraunhofer's plan and adjusted to an achromatic telescope, placed at 20 feet from a vertical slit in a brass plate, one-fortieth of an inch in width and about 4 inches long. Half of this slit admitted diffused daylight in its ordinary state, the other half of the aperture was covered by the tube or other vessel containing the gas or coloured vapour. The two spectra were thus placed in juxtaposition, and the lines of Fraunhofer could, in most cases, be traced across the vapour spectrum, furnishing exact points of comparison between the two.

By increasing the depth of the coloured stratum, we increase the strength and number of the lines visible; new and fainter lines being rendered discernible by rendering the colour of the gas more intense. A smaller quantity of some bodies is capable of rendering these lines visible than is required to produce the same effect with others; e.g., a quantity of iodine vapour, just sufficient to give a violet tinge to the air in the tube, renders the lines distinctly perceptible, whilst with a tube of bromine, the vapour of which is as full a red as that of nitrous acid, the lines, though visible, are very indistinctly defined. I have, therefore, in experimenting on permanent gases and vapours of liquids volatile at ordinary temperatures, been careful not to decide on the absence of lines unless the light had been transmitted through a body of vapour at least 9 inches deep. It is possible that in one or two instances of substances which require high temperatures for their volatilization, the employment of a stratum of greater depth might have revealed spectral lines, as, with the exception of sulphur, the experiments were, on account of the difficulty of uniformly heating larger vessels, performed in tubes three-fourths of an inch internal diameter. These exceptions, however, if they exist, will, I believe, be very few, as in no case, by using thicker strata, have I found lines, when in such tubes all indications of their existence were wanting. The vessels I have found convenient in operating on the larger quantities of gases are rectangular boxes of colourless plate-glass, usually cemented with marine glue.

In connexion with the effect of varying the depth of the coloured stratum, I may mention, that if we increase the intensity of the colour—e.g., with nitrous acid by the application of heat, the lines throughout the whole spectrum become much darker, and, where they are few in number, more sharply defined; but where they are numerous, they increase to such an extent as altogether to arrest the passage of the light, and the space from F to G, which in Fig. 4 represents several well-marked groups, becomes one continuous band of shadow. On allowing the tube to cool, the lines in this part resume their former distinctness. Analogous phenomena are observed in the case of iodine. This vapour acts principally on the green portions of the spectrum, and if the vapour be at all dense, the orange and yellow spaces appear crossed by the equidistant lines (Plate II. Fig. 2) which characterise this vapour; and they gradually are lost in the green, which seems blotted out by a uniform shadow. As the tube is allowed to cool, and the mass of vapour becomes rarer and its hue fainter and fainter, the

lines are seen by degrees extending into the green, and at length the whole shaded portion is filled with them. They do not disappear in this part until those in the orange and yellow are gone, and until the last tinge of violet in the tube has completely passed away. In most instances, where vapours, and not gases, are the subjects of experiment, it will be found advantageous to exhaust the air and seal the tube by the flame of a blow-pipe. The vapour of indigo and of several other bodies is thus experimented on without difficulty.

I need not advert to the remarkable but now well-ascertained fact, that in the light of the afternoon and evening lines become visible which at other times are not readily detected, and that those of the red and orange portions of the spectrum, in particular, become much more distinctly marked; but a singular appearance accidentally presented itself to me the other day. I was examining the spectrum of the diffused daylight towards the evening when a violent thunder-shower came on. Lines not before visible were distinctly apparent, and a group in the brightest part of the spectrum between D and E, though nearer to the former line, became very evident, increasing in distinctness with the violence of the shower; as the rain passed away they again faded and disappeared. I have had subsequent opportunities on several occasions of confirming the accuracy of this observation.

In Fig. 1, I have represented approximatively some of the principal groups of lines that I have observed in daylight after transmission through different vapours. No pretensions to accuracy are made in these sketches; they are simply intended to convey an idea of the general position and grouping of the lines.

The two first series, those of bromine and iodine, are produced by simple bodies; the remaining ones by compounds containing only two elements, it is true, but the arrangement of the molecules of which, it is all but certain, is far more complicated than such an apparent simplicity might warrant us in expecting.

In Fig. 1 some of the principal solar lines are depicted as fixed points of comparison.

Fig. 2 gives the general appearance of the lines presented by iodine. The specific gravity of this vapour is 8.707; the lines in this case are more distinct in the orange and yellow portions, and in the green becoming so numerous as to obliterate it. These lines are not sharply defined, but shadow off into the lighter spaces very gradually.² In the more refrangible part of the spectrum I did not discern them.

Fig. 3. Bromine shows lines corresponding to those of iodine. They are less distinct, however, and can scarcely be discerned in the orange portions. The violet end of the spectrum is entirely intercepted. Specific gravity 5.390.

Fig. 4. Some of the principal lines in the peroxide of nitrogen, NO_2 , specific gravity 3.183, the gas is of a deep orange. In this gas one vol. of nitrogen and two of oxygen are condensed into one.

Fig. 5 gives some of the groups exhibited by the peroxide of chlorine, Cl_2O_2 ; these are principally confined to the blue and indigo portions, the colour of the gas being a bright orange. No correspondence between the position of the principal groups in these two gases is observable.

It appeared to me of great interest now to ascertain

² I am informed by Professor Miller, that with instruments of high magnifying power and accuracy of definition, these bands are seen distinctly to be composed of very fine lines, increasing in number and strength towards the central or darkest portion of the band.

what influence the successive steps of oxidation of the same body might have upon the spectrum; and two classes of substances presented themselves as well adapted to this inquiry—viz., the oxides of chlorine and of nitrogen.

Thanks to the researches of many eminent chemists, and especially of Millon and Pelouze in the last few years, we are now in possession of means for preparing and insulating these compounds of chlorine in a state of purity. Finding the lines (Fig. 5) in the gas extricated from chlorate of potash on the addition of sulphuric acid, I next prepared some pure chlorous acid from chlorite of lead (PbO, ClO_2). This salt, on the addition of nitric acid, furnished chlorous acid (ClO_2) in a state of purity. In this gas 2 vols. of chlorine and 3 vols. of oxygen are condensed into 3 vols., and it has a specific gravity of 2.646; and on proceeding with prismatic analysis of the transmitted light, the very same series of lines was produced. Chlorous acid, in very small quantity, is capable of tinging a very large bulk of water of a bright yellow colour. As the gas had so marked an effect on the spectrum, I was induced to try this solution, but failed entirely in discovering lines; the liquid arrested the whole of the more refrangible rays entirely, no light passing beyond the mid-space between E and F.

I now prepared some pure *peroxide of chlorine* (Cl_2O_2), freeing it from chlorine and chlorous acid, by which it is always accompanied, by passing the gas through tubes surrounded with a mixture of ice and salt. The peroxide alone condenses at this temperature, and the deep red liquid thus obtained was allowed to fall into a tube and evaporate. The same series of lines were produced. In this gas, which has a specific gravity of 2.325, 1 vol. of chlorine and 2 vols. of oxygen are condensed into the space of 2 vols.

Euchlorine was next tried. The gas was disengaged from a mixture of hydrochloric acid and chlorate of potash, condensing the compound as before by a freezing mixture. To this compound Millon assigns the curious formula of Cl_2O_{13} . Here, too, I found the same series of lines. Millon's theory of the composition of these bodies is, that the peroxide of chlorine is a compound of *chloric and chlorous acids* in single equivalents, as it is immediately resolved into these two acids by the action of alkalis; and he considers the *enchlorine*, or chlorochloric acid, as he proposes to call it, a compound of 1 eq. ClO_2 with 2 ClO , the action of acids, immediately resolving it into those acids in the proportions just mentioned. Certainly the occurrence of similar lines in all three, although the condensation varies, does not militate against this view, but increases its probability.

Millon's new compound, chloro-perchloric acid, which results from the action of solar light on chlorous acid, I have not hitherto obtained in a state of sufficient purity to pronounce upon.

It is remarkable that the *hypochlorous acid* (Cl_2O), though of a bright greenish-yellow, furnishes no such lines. The density of this gas is 5.881, 2 vols. of chlorine and 1 of oxygen being condensed into 1. It is an interesting fact, that the colourless *protoxide of nitrogen*, in which 2 vols. of nitrogen and 1 vol. of oxygen are condensed into 2, having a specific gravity of 1.524, has also no sensible effect upon the spectrum.

In the series of oxides of nitrogen, the *binoxide*, NO_2 , in which 1 vol. of nitrogen is united with 1 vol. of oxygen without condensation, also a colourless gas, has no effect on the spectrum. No oxide corresponding to this in the chlorine series is as yet known.

Hyponitrous acid I have not isolated in sufficient purity to furnish trustworthy results.

Fig. 6 represents the grouping of these lines as seen in daylight transmitted through the vapours of perchloride of manganese, which is readily prepared by adding a few crystals of permanganate of potash to oil of vitriol in a test tube, and dropping in a fragment of fused chloride of sodium, a green vapour (Mn_2Cl_2) is immediately liberated, which is, however, quickly decomposed by the moisture of the air. It will be remarked, that in this vapour, the colour of which is green, the lines are most abundant in the green part of the spectrum, the green tint of the gas being due principally to the mixture of the blue and yellow rays.

The *perfluoride of manganese*, which corresponds in composition to the perchloride, does not yield any lines that I could perceive, but the moment a fragment of common salt is dropped into the mixture from which the fluoride is being disengaged, the lines of the perchloride make their appearance. Both the perchloride and perfluoride are decomposed by water, and on dropping in a little of this fluid into the tube of perchloride, all the lines instantaneously disappear. The purple vapour of the permanganic acid, in which the number of equivalents of oxygen corresponds to that of chlorine and fluorine in these compounds, is equally inactive with the fluoride.

I have examined very many other substances which yield vapours of different colours, but have not succeeded in finding any which give distinct lines besides those above mentioned, with the telescope which I employed. It was not, however, an instrument of very high power.

Simple bodies:—

Chlorine.—Greenish-yellow colour; specific gravity 2.47. This is the more remarkable, as bromine and iodine both give them.

Sulphur.—Yellow; specific gravity 6.654.

Selenium.—Yellow.

Compounds:—

Selenious acid.—Yellow (SeO_2).

Hypochlorous acid.—Yellow; specific gravity 2.993 (Cl_2O).

Oxychloride of chromium.—(CrClO_2); red vapours.

Oxychloride of tungsten.—(WClO_2); red vapours.

Terchloride of tungsten.—(WCl_3); red.

Sesquichloride of iron.—(Fe_2Cl_3); reddish brown.

Perfluoride of manganese (Mn_2F_4).—Greenish yellow. This is remarkable, as the perchloride gives a very distinct series.

Permanganic acid (Mn_2O_7).—Purple. This is beautifully shown by allowing a drop of water to fall into a tube full of the perchloride of manganese, when all the lines immediately disappear.

Indigo.—Splendid crimson vapour ($\text{C}_{16}\text{H}_5\text{NO}_2$).

Alizarine from Madder.—Orange vapour ($\text{C}_{20}\text{H}_{12}\text{O}_{10}$). Both these substances require careful management in the sublimation, as they are readily charred, and a small portion is always decomposed, whatever the amount of care employed.

In making experiments on coloured flames, I generally used an alcoholic solution of the compound on which I was experimenting. A common cotton wick, supported in a small glass tube, furnished the lamp whose flame I wished to examine. This little lamp was placed opposite the fissure, in a tin box, the side of which next the slit

* Professor Miller informs me, that by means of an instrument of superior quality, he has succeeded in satisfying himself that lines are produced by this vapour.

was permanently open, whilst the opposite side was furnished with a door, opening outwards and upwards so that by a string it could be raised to admit the light of day, and would afterwards close by its own weight when allowed to fall back. Fraunhofer's lines (Plate III., Fig. 7) thus again served as points of comparison; the comparison, though not rigidly accurate, being still very nearly so, and perfectly sufficient for my purpose.

Fig. 8 in Plate III. exhibits the remarkable spectrum given by a solution of *chloride of copper*. Several intervals of absolute darkness here occur, interrupted by bright lines of great intensity, particularly in the green and blue spaces. The general hue of the diffused light is a bluish-green.

Fig. 9 represents the spectrum from the green light of *boracic acid*, in which, besides the bright streak at D, five well-marked bright bands occur in the yellow and green, and a narrow line in the indigo.

Fig. 10 is the spectrum from *nitrate of strontia*, the red and orange portions of which are particularly developed, and are crossed by three very strong black lines; a bright line will be remarked in the blue, and another in the indigo.

Fig. 11 is that from an alcoholic solution of *chloride of calcium*; besides the lines in the orange, there is a very brilliant yellow streak at D, and two bright bands in the yellow; in addition to these is a bright streak in the indigo.

Fig. 12 represents the spectrum of a flame coloured by *chloride of barium*. It is in its features intermediate between that of strontia and lime; the most remarkable character is in a bright streak in the orange, another in the yellow, and one in the indigo.

Even *chloride of sodium*, though the intensity of the light is accumulated in a bright band near D, shows a marked tendency to the occurrence of bands in other parts; a distinct, though faint light extending far into the indigo, when a bright streak terminates it almost suddenly.

Chloride of manganese gives indications of the occurrence of bands, though but feebly. I have also tried solutions of the chlorides of iron, zinc, cobalt, nickel, mercury, and magnesia; in all a feeble band occurs in the green portion, as represented in the three last spectra; this is probably connected with the disengagement of chlorine from the salts during combustion; and the bichloride of mercury gives a bright streak near G in the indigo.

The combustion of phosphorus in the open air gives a pure spectrum without lines. I tried it also in chlorine, but did not obtain any very clear spectrum, as the deposition of chloride on the jar quickly rendered it opaque.

Another source of light was furnished by the ignition of different substances in the oxyhydrogen jet: the light thrown directly upon the prism was too intense for the eye to bear, I therefore received it on a sheet of white paper, and employed the diffused light so obtained. In the spectrum from charcoal nearly midway between D and E, a very short brilliant streak was visible, and the same streak appeared when the flame was thrown upon a fragment of dried *alum*.

Lime and strontia give results similar to those furnished by the solution of their salts when used to tinge the flame of alcohol, but not quite so well marked. *Baryta* melted too rapidly to furnish any satisfactory result.

Zinc, iron, steel, and platinum gave brilliant spectra, but no lines except that at D were visible. *Copper*,

lead, and antimony melted without giving light sufficient to make any satisfactory observation.

It may be interesting to remark, in connection with the speculations on the absorptive action of the sun's atmosphere, that if solar light be transmitted through a flame exhibiting well-marked black lines, these lines re-appear in the compound spectrum, provided the light of day be not too intense compared with that of the coloured flame; this may be seen in the red light of the nitrate of strontia, and less perfectly in the green of the chloride of copper. *It would, therefore, appear that luminous atmospheres exist in which not only certain rays are wanting, but which exercise a positive absorptive influence upon other lights.*⁵

It was my intention to have instituted a comparative series of experiments with the light obtained by ignition of the metals by the voltaic battery, but this point has already, I find, engaged the attention of my friend and colleague, Professor Wheatstone; and he has made it the subject of an investigation, characterized by his usual ingenuity and accuracy.

PROCEEDINGS OF SOCIETIES.

SOCIETY OF ARTS.

Wednesday, April 10, 1861.

Lord A. CHURCHILL, M.P., in the Chair.

At this Meeting Dr. JOSEPH MILLIGAN read his Paper on "*Tasmania: its Character, Products, and Resources*," many points of which are worthy of more than a cursory notice. We need not go into the historical and geographical portions of the Paper, as most of our readers are probably aware that Tasmania, *alias* Van Diemen's Land, was first discovered on December 18, 1642, by Abel Jansen Tasman, and was occupied as a British settlement in the autumn of 1803. Nor does the convict question come exactly within our province so we pass it over, and will quote Dr. Milligan's own words on the matters of the Trade and Population of the Colony.

"As the prosperity, wealth, and importance of a new country may be said to depend entirely upon population operating on its natural resources and capabilities, I shall briefly state its progress in Tasmania.

"In the year 1810 the number of inhabitants was	1321
" 1820 "	5400
" 1830 "	No return.
" 1840 "	46,057
" 1851 "	69,187
On Dec. 31. 1859 "	86,451

"And allowing the recent rate of increment down to the present year (1861) the number would be now nearly 90,000.

"So general was the exodus of the working population during the gold digging period of excitement, that the emigration from Tasmania in 1851-2 and '3 amounted together to 35,077; and it is shown by statistical returns that upon these three years there is a balance against the colony of emigration over immigration of more than 10,000 adult individuals, who were poorly replaced by the arrival of families amongst whom were 6000 women and children. An abstraction of so large a proportion from the adult labouring classes in the colony inflicted a considerable check, and told for some time very distinctly on the results of its productive industry. For instance, the breadth of

⁵ This paragraph (the italics of which are our own) shows that Professor Miller has anticipated, by nearly sixteen years, the remarkable discovery, ascribed to Kirchhoff, of the opacity of certain coloured flames to light of their own colour.—ED. C. N.

land in wheat crop in 1851 was 66,236 acres whereas in 1853 it amounted only to 44,123, and in 1854 to 42,920 acres; a state of depression from which it had so far recovered in 1859 as to have attained in that year to 60,314 acres.

"Wool, which in the early times of the colony was permitted, it is said, to rot in heaps on the premises of the settlers, and which from its coarse and low character at the time scarcely deserved a better fate, has been so highly improved in later times, and increased in quantity so immensely, as to have long occupied the place of the principal export and mainstay of the colony. This important end has been attained through repeated and long-continued crosses with the sheep (both ewes and rams), selected from the finest merinos and Saxon flocks in Europe by enterprising colonists, and by the agents of the Van Diemen's Land Company, the purity and excellence of whose live stock of every description long maintained a high character, and fetched the highest prices. The capabilities and suitability of Tasmania for the production of wool of the finest fibre and highest value for the purposes of the manufacturer is well known. The high prices commanded in the English market by wool from the flocks of Messrs. Kermod, Smith, Macleanachan, &c., practically attest the fact. Its character was acknowledged to be of the highest order at the London Exhibition of 1851, and at Paris in 1855. The capacity of the island for maintaining a much larger number of sheep than are at present depastured there can scarcely be doubted. For a long time sheep-owners in Tasmania cultivated fineness and softness of fibre exclusively, and the result was a delicate animal and comparatively light carcase, but a change took place with the rise in the value of animal food, and for many years past it has been the aim of flock-owners to combine length of staple with fineness of fibre to the utmost extent which they can command, a course by which they obtain a much heavier fleece, and a larger and probably hardier animal. With this view the highly improved merinos have been crossed with the Leicester, New Leicester, and South Down breeds, &c., and in many instances with great success. From the statistical tables published by Governments are extracted the following return of sheep depastured in the colony, and of the value of wool exported therefrom in the years mentioned. In 1823 wool was exported to the extent of about 1000 bales. In the year

	No. of sheep.	Value of wool.
1839	898,590	£194,647
1844	1,145,089	176,269
1849	1,712,291	202,334
1854	1,831,308	325,384
1859	1,697,199	467,968

There appears to have been a falling off in the number of sheep in the colony since 1854, but on closely examining the returns I find the weight of wool exported in 1854 stated to have been 4,419,276 lbs., while in 1859 it was 6,107,903, and as there is a nearly corresponding difference in the value, it may be concluded that small sheep of short wool and light fleece, had been replaced to some extent with sheep of larger frame and longer and heavier wool. Upon artificial pastures, along the north and north-west coast, where the climate is humid as well as warm, the Leicesters attain a size and weight little short of the prize animals exhibited at cattle shows in England, but they are not calculated to thrive on the comparatively thin natural pasturage. The Cheviots have been introduced, and as might have been expected, are found to thrive prodigiously and to increase rapidly, but it is a questionable proceeding which would replace the merino crossed with Leicester blood, yielding a fleece of fine wool of rather long staple, for a breed which, however hardy, is remarkable for the vulgarity of quality in the wool throughout its fleece, and which scarcely bears a fleece heavier than that of the animal it supersedes.

"The amount of increase realised from breeding flocks in Tasmania is dependent to a great extent upon the condition (as to flesh) and health of the ewes, upon the nature of the pastures as to the abundance of grass, or the reverse, and upon the nature of the weather at that critical season. On good, well sheltered pastures, with ewes in fair condition, I have often seen upwards of 100 per cent. saved; if at any time the yield falls much below 80 per cent. there is ground for suspecting some mismanagement; if it falls below 40 per cent. gross defects of management must have existed. One of the most ordinary causes of failure of increase in sheep husbandry is the very prevalent custom of over working the grazing, so as to produce an unnatural delicacy of constitution in the stock.

"Sheep farming, from the earliest times of the Australian settlements as the resort of free colonists, has been regarded as the most profitable mode of investing capital. In flocks where there is a fair proportion of breeding ewes of wethers, and other dry sheep, the clip of wool is considered to be more than sufficient to defray all charges of management and the value of the annual increase becomes the net measure of profit accruing. There have occurred some singular vicissitudes in the value of sheep in the Australian colonies. In 1809, prices were very high, in consequence of a large exportation for two or three years to the new colony of Port Philip, but in 1843 sheep were selling at 2s. 6d. per head in that settlement, and at the same time fetching very low prices in Tasmania. The boiling-down process was discovered, and immediately prices assumed a comparatively high figure, and small capitalists who had purchased extensively at the previous extremely low rates, found themselves suddenly converted into men of large fortune. Many Tasmanian settlers became at that period large proprietors of sheep in Victoria, and were proportionately enriched."

Next, as to whale-oil, the Lecturer said:—

"The first notice which I have met with of export of oil from the colony is an entry of 726 tons in 1822. The following quotations will give an idea of the average annual yield of the Tasmanian whale fisheries:—

	Vessels.	Tonnage.	Value of Oil and Whalebones.
1839	26	3146	£119,113
1849	34	7791	37,824
1859	No return.		63,325

"From which last entry it would seem that this branch of trade, has entirely recovered from the blow it received through the loss of crews at the time of the gold diggings."

On the subject of corn we find the following:—

"Wheat has been named as one of the staple products of the colony. In no part of the world is it raised with greater facility or of more excellent quality. There is now exhibited at the Crystal Palace, Sydenham, a sample grown by Mr. Axford, of Bothwell, Tasmania, which weighed 70lbs. per imperial bushel. Samples sent to the London Exhibition of 1851, and to Paris in 1855, were pronounced to be of first-rate quality, and obtained medals. At Circular Head, and in other districts in the island, 50 bushels to the acre have been reaped. The average return, notwithstanding, is comparatively small, as it is not believed to exceed 20 bushels to the acre. The agriculturist possesses, however, in the equability of the seasons and in their genial character,—in the mildness of winter weather and the length of the winter day—and in the certainty of favourable weather during seed-time and harvest, more than equivalent. Besides, there are the best reasons for believing that much of the wheat produced in the colony is grown on land which, from having borne one crop of wheat after another for a long series of years, has become partially exhausted, and that 20 bushels per acre would be very far below the average return under a skilful rotation and proper system of manuring. At present

it is the boast of the Tasmanian farmer that ordinarily he applies no manure to his land. A few select farms, in various parts of the colony, such as 'Clarendon,' the estate of Mr. J. Walker, on the Derwent River, and some few suburban farms, such as that managed by Mr. Shoobridge, in the close vicinity of Hobart Town, form marked exceptions to this slovenly and wasteful want of system, and make, both in cereals and root-crops, the largest and heaviest returns for the liberal and judicious course of husbandry to which they are subjected. It is therefore a reflection on the farmers of Tasmania, rather than a disparagement to its soil or climate, to say that the return of wheat to the agriculturist is generally so much less than it ought to be. There is still another mode of explaining the occurrence of light crops. The best lands are usually the most heavily-timbered, and it has not at all times been convenient to the settler to select for cultivation the land best adapted to this object in preference to that which is naturally clear of trees, or nearly so. Hence it happens that there have been brought under corn tillage much land altogether unfit for the growth of wheat, without heavy aliment in the shape of manures, which it rarely or never obtains. The great advantages which the Tasmanian farmer enjoys in the fine weather of winter for out-door labours—of spring for his seed time, and of autumn for his harvest, as it relieves him of all anxiety, may well reconcile him to a comparatively light return, but it is not right that a deficiency arising out of defective farming should be attributed to the soil; let the onus rest on the right shoulders. The price of wheat is now, and has been for some years, dependent less upon the domestic relation between the quantity produced and the number of mouths to be fed, than upon the demand which the market at Melbourne may present. Indeed the Melbourne markets, it may be said, do now at all terms most naturally modify, if not absolutely rule, the prices of provisions in the neighbouring colonies. At the same time it is matter for congratulation to Tasmania, and her wheat-producing rival, South Australia, that this Babel of the South does afford them a market for every variety of agricultural produce which they can raise. Wheat has been sent from Tasmania to the London market on more occasions than one, when very low in price within the colony, and at a period antecedent to the existence of a gold colony in its neighbourhood, but although the quality of the grain was highly appreciated, never with a result to encourage the belief that such speculations might expand into the dimensions of a regular trade. The freight and charges consequent on a long voyage, and on exposure to many casualties, have invariably absorbed the whole margin left for profits. The colonial market may therefore be said to be the only outlet at present for Tasmanian wheat; but it is a market already making large demands, and widening from day to day with every addition to the digging population, and with every fresh advance colonisation makes in the direction of those regions in Australia where tropical heats forbid the attempt to cultivate the cereals of temperate climates."

After a slight digression, Dr. Milligan returned to wheat and allied cereals, telling us that

"The amount of wheat exported from the colony,—

"In 1849, was 296,236 bushels, valued at \$51,722:

"1859, " 196,405 " " £92,861.

The flour exported in 1859 was valued at £105,128. The *Price Current* of the 23rd of January last, just received from the Colony, quotes the price of prime samples of wheat at 7s. to 7s. 3d. the bushel. The cultivation of wheat admits, it may be added, of almost boundless extension in Tasmania, every probability being in favour of the opinion that the yet uncultivated portion of the Colony contains lands of the richest character, and that as these are brought under tillage a very much higher

average yield of wheat will be obtained, altogether independently of increment from improved modes of culture.

"The usual return from ordinary soils, and in situations adapted to the growths of this grain, may be taken at about 35 bushels to the acre. There is a sample of Tasmanian oats, exhibited at the Crystal Palace, Sydenham, which weighed 49 lbs. per bushel—the yield having been 60 bushels to the acre. The value of oats exported in 1859 was £94,968. In the *Price Current* of the 23rd of January last, from Hobart Town, the price quoted is 3s. per bushel. Barley is not a favourite crop with farmers in Tasmania. The variety known as Cape barley is grown pretty generally in the autumn and winter, in order to be cut very early in the season as green-feed for cattle, and the yield is usually rather heavy. The export of barley is trifling. Of other agricultural products, the most important is the potato. This crop is chiefly cultivated along the estuaries of the rivers opening into Bass's Straits, and at Circular Head, Table Cape, and Emu Bay, on the north side of the island, and along the shores of D'Entrecasteaux Channel, the Huon River, Brown's River, and the Derwent River—near Hobart Town—on the southern side. The potato of Tasmania is far superior to that of any part of Australia or New Zealand; it is quite equal in point of dryness to the best potato grown in England, and very much to be preferred in flavour. The ordinary return obtained in its cultivation varies excessively; the diversity is due as much probably to varieties of tillage and treatment as to difference in the constitution of soils. A mean of the produce of the whole Colony produces five tons to the acre; the best farmers do not expect more than seven or eight tons, but the yield in some cases is more than ten tons to the acre. In the Statistical Table for 1859, it is stated that 7777 acres of land, cropped with potatoes, yielded 41,493 tons:—

In 1849 the price of potatoes in the colony was £6 per ton.

"1854 " " £25 to £30 "

"1859 potatoes were sold at " £3 and £4 "

In December, 1860, the last shipment from Hobart Town,

360 tons, sold at " £8 8s.

Potatoes were exported in 1857 to the value of £61,408.

" " 1859 " £42,906.

"The turnip crop is quite as hazardous in Tasmania as in England, but of far less critical importance to the farmer, live-stock being by no means so dependent on housing and artificial food there as they are in England during the winter season. Mangold-wurtzel and carrots are both well suited to many Tasmanian soils, but are not much grown.

"HAY.—English grasses are cultivated rather extensively for hay, and yield a fair return. As provender for horses it is always in demand for domestic consumption, and for the Melbourne market. The price in Hobart Town in December, 1860, was £4 and £6 per ton.

"Butter and cheese were exported in the year 1859 to the value of £13,877. The dairy is rising in estimation among farmers as a source of profit; and the butter and cheese of Tasmania has acquired a high character in the neighbouring colonies. The last quotations at Hobart Town (23rd January) give for fresh butter 1s. 3d. to 1s. 6d., and for cheese 1s. 6d. to 1s. 9d.

"LIVE STOCK.—Horses, sheep, cattle, and pigs were exported from Tasmania to the value of £60,900 in 1859.

"The feats of Tasmanian horses on the turf early established for the colony the reputation of possessing the best blood and bone in the Southern Hemisphere. The late Governor, Sir W. Denison, was a warm patron of the turf, and frequently went out with the hounds, proving himself as fearless as firm in the saddle. The gold of Victoria has now effected a transfer of much of the horseflesh of Tasmania to the opposite shores.

"The breeds of cattle which have been introduced into

the colony are the Durham or short-horn, the Hereford, the Ayrshire, the West Highland, and the Devon. These have all, at one time or other, and to a greater or less extent, been crossed with an original coarse long-horned variety, betraying Indian lineage or connection, and derived probably from New South Wales, in the very earliest times of the colony. The Durham is the favourite breed, but it carries a frame and carcass too large for the natural pastures, save in exceptional localities, and it is too delicate in constitution; the Ayrshire maintains in the colony its high reputation as a milk cow; but the Devon is by far the best adapted in frame, constitution, and kindly feeding properties, for the natural pastures of the island, and it is decidedly a comely animal. The kyloe, or West Highland breed, promises to yield a cross extremely well-suited for the wild, warm, and humid feeding grounds to the westward. The number of horned cattle in the island in 1859, appears, by Government tables, to have been 79,950. Pigs are extensively cultivated by farmers of every degree. In the early times of the colony a few were allowed to run wild in the bush, where they lived chiefly on fern roots; they have long since been extirpated. The number in the colony, as per statistical return for 1859, was 32,008.

"Fruit has within a few years acquired importance, as an article of export from the colony. Surplus apples used, in bygone times, to be converted into cider, which was given to sheep-shearers, haymakers, and harvestmen, or sold at a moderate rate. They are now sent to the Melbourne market, where the demand for them is increasing.

"There are in the colony more than 100 varieties of apple; many of them are of finer flavour than the original sorts in England, and they attain to a larger size. There are of pears more than 50 varieties, and amongst them are some of the finest quality, and some of the longest keepers. There are 25 sorts of plum, 12 kinds of cherry, 6 apricots, 1 nectarine, 30 varieties of the vine, 6 kinds of the fig-tree, mulberry, medlars, almonds, many sorts of gooseberry, currants, raspberry, &c. Most of these fruit trees bear in great profusion, and the different fruits ripen more perfectly, and are thought to possess higher flavour than the same sorts in England. Peaches and plums are still often allowed to fall, and go to utter waste under the trees in large quantities, and so also with other fruits not unfrequently.

"The value of fruits and preserves exported—

"In the year 1849, amounted to £6976."

1859, " £62,430.

We have not space for the whole of Dr. Milligan's observations upon the timber of Tasmania, but will transcribe a few particulars respecting the well known "blue gum" tree, which attains a height of 350 feet or more, and whose circumference often exceeds 100 feet.

"Planks of this wood were cut for the International Exhibitions at London in 1851, and Paris, 1855, having a width of 20 by 6 inches, which measured respectively 145 and 160 feet in length, a length which might be extended to 200 feet if specially required, as the trunk or bole of this tree often measures 220 feet and upwards to the first limb. There is, however, a practical difficulty in the shipment of such long lengths when ready, as few vessels can be found to stow them on board. The timber of the blue gum has been found nearly, if not quite, equal to oak for shipbuilding. Vessels of 400 to 500 tons burden and under, constructed of it, have from time to time, during many years, been turned out of the ship-yards of Messrs. Degraives, Watson, Ross and Macgregor, &c., at Hobart Town, and it is the proud boast of Tasmanian ship-owners that the age of the colony is not yet such as to have fairly tested the durability of this timber.

"A series of carefully conducted experiments, by Mr. Mitchell, upon scantlings of blue gum, of the same dimensions as those employed by Captain Baker, in his experi-

ment on morung saul, and of Professor Barlow, in his experiments on teak, and other timbers, have established the following comparative results:—

" Specific gravity of Blue Gum . . .	=	1090
" " Morung Saul . . .	=	1054
" " English Oak . . .	=	969
" " Teak . . .	=	745

"Elasticity remaining perfect, the deflection and weights sustained are:—

Blue Gum . . . 2625 inches deflection and weights	776 lbs.
English Oak 1590 " " "	150 "
Morung Saul 1175 " " "	450 "
Teak . . . 1151 " " "	300 "

"The breaking weights of each in pounds were:—Blue Gum, 13,330; Morung Saul, 1192; English Oak, 637; Teak, 938. The value of direct cohesion on a square-inch of timber is:—Blue Gum, 93,408; Ash, 17,337; Teak, 15,550; English Oak, 10,853. These results exhibit a superiority in favour of blue gum so overwhelming as to distance all competition. It only remains to secure for the important practical purposes of ship-building, &c., &c., timber equally well seasoned as the samples operated upon by Mr. Mitchell, to realise in its actual employment and use the full advantage of the high qualities it is found to possess. Were shippers of this timber bound by legal enactment in the colony to prove in each case that the timber has been cut and seasoned in the scantling for five years, it would, probably, be sufficient to protect the trade from the imposition of inferior sample, and the character of the timber from doubts and aspersions as to its superior value. Looking at the paramount superiority of this timber in the qualities most valuable for ship-building purposes, it may fairly be questioned whether full justice is done to its merits by the position (Class A 1, for 9 years) assigned by Lloyd's to ships built of it.

"The enormous demand which has of late years arisen in England and other countries for railway sleepers of wood has suggested to Tasmanian colonists the advisability of proposing the employment of the blue gum of the colony, on account of its hardness, strength, and durability, and they urge that, as sleepers of blue gum can be supplied at 5s. or 5s. 6d. each, which would last more than double the time of the pine sleepers now in use, which cost 3s. 6d., a vast saving of outlay in money and labour, would, by its adoption, be effected to the railway companies, &c. Several shipments of sleepers of the blue gum have recently been forwarded to London, in order to their being put upon their trial. It would be but fair to the future character of the blue gum, that the extent of seasoning to which these sleepers have been subjected previous to shipment should be made known, so that if oversight or negligence has led to the export of green wood, any injury which the reputation of the timber might thus sustain may be obviated, and the recurrence of so objectionable a practice prevented. The blue gum has been used for telegraph poles, and found to answer well. The swamp gum, the stringy bark, the iron bark, &c., will be found scarcely, if at all, inferior in any useful quality to the blue gum when properly seasoned. The blue gum and other varieties of Eucalyptus abound to such an extent along the sea-board of Tasmania as to enable the colonists to say that these timbers can be supplied for an indefinite period of time in any quantity the mechanic world may require.

"There are many varieties of trees in the forests of Tasmania, which yield woods for house-building fittings, implements, &c.; and there are others of great excellence and beauty for furniture, and for purposes of the cabinet-maker. Some fine samples were sent to the London Exhibition of 1851, and to Paris in 1855, and there are now on view, at the Crystal Palace, Sydenham, a few superb specimens of the myrtle-wood *Fagus Cunninghamii*, and the musk-wood, *Eurybia argophylla*—the Black-wood, *Acacia melanoxylon*, and Huon pine, *Dacrydium Frankii*,"

&c. Wherever seen, these ornamental timbers have been much and justly admired for their rich and harmonious tints and beautiful venation, and need only to become more extensively known in order to come into large demand in this market. The source of supply, I may add, is, to all practical intents, inexhaustible.

Building-stones of various kinds,—coal, anthracite, clay, ironstone, red hematite, and galena are all abundant in the island, while gold has been obtained from the detritus of various slate rocks, and serpentine, asbestos, and oxides of manganese have also been found in many places.

However reluctantly, we must now conclude this brief review with a few paragraphs selected from the discussion which followed the reading of the paper.

Mr. JOHN YOUL observed, in the course of his speech, that he had been travelling among the myrtle trees of which the slab upon the table was a specimen, about eighty days previously. They were very beautiful and entire; impenetrable forests extended for distances of fifty miles. The mountain-lakes of Tasmania formed perhaps the most extraordinary natural feature,—many of them were at an elevation of 3000 feet above the level of the sea, and some were of a depth of 75 fathoms.

Mr. EUGENE RIMMEL asked Dr. Milligan for some information concerning various odoriferous plants.

Dr. MILLIGAN said that the *Acacia*, *sassafras* and *musk-wood*, were found in great abundance, but that the *Elizir buccifolia* was not so common.

After various other speeches, a vote of thanks to Dr. Milligan was proposed and passed.

The Secretary has received the following letter since the meeting:—

Products and Resources of Tasmania.

"SIR,—I was unexpectedly prevented from attending the meeting on the 10th inst., or I should have given verbally the few observations upon Dr. Milligan's paper with which I now trouble you.

"It may perhaps appear somewhat ungrateful to ask for further information, after the rather lengthy paper referred to, but Dr. Milligan may possibly be able to add a few points to, and thereby increase the value of, his very instructive communication.

"In the first place, we might reasonably hope for a few more particulars relating to the food-products of Tasmania, both as regards their local preparation and consumption, and also the articles which are now, or may be hereafter, exported for this country.

"Tasmanian wheat I quite believe to be of superior quality, as a general rule, to that consumed in Britain, whether of home or Russian growth; in 1857, a small sample of the grain was placed in my hands, which gave on analysis the following results:—

Water	13.450
Ash	2.210
Potassa	.265
Soda	.390
Magnesia	.233
Lime	.072
Sesquioxide of Iron	.024
Sulphuric Acid	.154
Chlorine	.053
Phosphoric Acid	.945
Silicic Acid	.026
Nitrogenous matter, Gluten, Albumine, &c.	15.750
Nitrogen	2.500
Carbonous matter (and loss)	68.590
Fat or Oil	1.425
Starch, Sugar, &c.	67.165

100.000

"In the preceding figures the proportions of nitrogen, fat, phosphoric acid, and alkalies, will be readily noted, as

being distinctly above the average; I have frequently heard of the superior qualities of the barley of Tasmania, but have never examined samples.

"Perhaps Dr. Milligan could furnish us with some more detailed particulars respecting those food-products of Tasmanian growth, which are, or may be found, specially adapted to meet the requirements of the mother country, and also concerning those plants it would be practicable and advisable to cultivate there for a similar object. It would be interesting to learn what edible roots, if any, could be profitably grown in Tasmania for British consumption; even a non-edible plant yielding large quantities of starch of fair quality, would supply a great and very generally felt want; if I am not mistaken, some rather considerable prizes are (or have been) offered in this country and in Belgium, for the discovery of a means of obtaining starch, suitable for manufacturing and general purposes, from a source not furnishing food for man.

"Although perhaps the cost, delay, and risk, attending transportation, might prohibit the regular importation of many of the food-products of the colony which are of little comparative value, or are consumed in very large quantities, this objection could not apply to those generally somewhat costly substances from which the flavours, the perfumes, and the colouring matters of commerce are extracted.

"I believe experiments have been made with the view of cultivating the vanilla plant, but I am not aware of the result. Then, again, if I am not mistaken, the fragrant *Acacia Farnesia*, and the *Comesperma volubilis*, and several varieties of *Eurybia*, are already grown to a small extent in Tasmania, while it has been stated to me that the same country is favourable for the growth of the *Garcinia Cochinchinensis*, the *Rubia tinctorum*, the *Panax quinquefolium* ("Ginseng") the *Statice tartarica* (perhaps the most powerful tanning agent known) and many other plants of equal or greater importance.

"What is there either to prevent Tasmania from supplying us with large quantities of silk, manna, millet-sugar, and various textile fibres? And, as a final query, perhaps I may ask if any credence should be attached to the rumour—for it seems to be of no very definite character—that truffles of a fine quality have been found in the northern part of the colony.

"Anything that tends to commence or extend the practice of importing to this country the products of any of its colonies or foreign possessions, must be regarded as of no mean importance to the interests of the countries concerned. Three advantages attend the movement: viz. the supplying of home desiderata by British subjects; the more rapid development of the resources of the colony itself; and, lastly, the check that can be had upon the quality of the articles imported,—in other words, upon the practice of adulteration, should it be found to exist.

"I am, &c.

"WENTWORTH L. SCOTT.

"Bayswater, London, W., April 12, 1861."

CHEMICAL SOCIETY.

Thursday, April 12, 1861.

Professor A. W. HOFMANN, F.R.S., President, in the Chair.

Messrs. A. E. Sanson, and H. K. Bamber were elected Fellows.

Professor F. A. ABEL, F.R.S., Director of the Chemical Establishment of the War Department, delivered a discourse upon "The Application of Electricity to the Explosion of Gunpowder." The Lecturer commenced by giving a short historical account of the experiments instituted in this and foreign countries, for the purpose of firing mines and cannon by electric agency, and referred to particular methods of operation adopted by Colonel Verdu, in Spain, and by M. Savare in Paris, and to very extensive opera-

tions carried on by a system organised by M. du Moncel, at the port of Cherbourg, by MM. Dussaud and Rabattu, in 1854. The Austrian system of employing frictional electricity, and results of great magnitude obtained with it, were also briefly referred to.

In reviewing the several methods proposed in our own country for effecting this purpose, the Lecturer described the construction of Statham's fuze, and of the "galvanic fuze," which has for many years been employed in firing guns at proof. The latter consisted of a quill tube, filled with compressed meal powder adapted to a wooden head, in which were arranged two small copper tubes with a fine iron wire connecting them, and surrounded with gunpowder. When, therefore, a galvanic current of sufficient power was made to traverse the thin wire, it became heated to redness and fired the loose powder and the composition in the quill-tube, which being inserted in the "vent" or touch-hole of the gun, ensured the ignition of the whole charge. Among the inconveniences arising from the application of electricity derived from the voltaic battery, were, firstly, the necessity for employing two lengths of wire to complete the circuit, the tedious and difficult operation (in soldiers' hands) of charging the batteries with acid, and the inconvenience and risk of accident attending the transport of the necessary agents.

An extensive series of experiments had been instituted at Woolwich and Chatham, since 1855, by desire of the Secretary of State for War, by Professor Wheatstone and Mr. Abel, for the purpose of ascertaining the relative merits of different forms of electricity applied to the explosion of gunpowder.

The great improvements made in the construction of the induction-coil apparatus by M. Ruhmkorff rendered it desirable that experiments should be instituted, with small battery-power, to obtain the effects of electricity of high tension; and the results were considered so satisfactory, that Mr. Abel pointed to this form of apparatus, as possessing decided advantages for firing a great number of fuzes simultaneously; in all cases, indeed, when the special nature of the operation would warrant the use of a battery. The ignition of six charges of gunpowder in a series was demonstrated at the lecture, by the use of a powerful induction apparatus of Mr. Ladd's construction with small battery-power.

A large magneto-electric apparatus, constructed by Henley, with a lever-armature, was the first instrument tried at Woolwich for dispensing with the use of the voltaic battery. With this a great number of experiments were made in the endeavour to ignite gunpowder and other compositions of a highly inflammable nature. Little success attended these efforts until moistened gunpowder came eventually to be tried. This exploded and led to the introduction of Mr. Abel's first "Magnet-fuze." In describing its general construction, the Lecturer called attention to the invaluable service rendered in these experiments by a particular form of insulated copper wires manufactured according to his instructions by the Gutta-percha Company—two thin wires were separately covered with a non-conducting coating, then laid side by side, and these twin wires further coated and bound up into one by an outer covering of gutta-percha. Whenever a cross section is made in this the two copper wires are severed, and the terminals brought to view. Short lengths of this material were advantageously applied in the fuze-head, and placed so that the moistened powder rested on the bare terminals. Several hundred of these quill fuzes were fired with the large lever-magnet with great certainty, the only failures arising from mechanical defects in their manufacture. As a convenient means of preparing the moist gunpowder in a state fit for priming the fuze, the ordinary fine-grained powder was saturated with a dilute alcoholic solution of chloride of calcium; on exposure to

the air for a day or two the spirit evaporated completely, and at the same time gave the deliquescent salt in the powder an opportunity of absorbing the necessary amount of moisture. The large magnet-apparatus and a supply of suitable fuzes, composed portion of an equipment fitted out during the late Chinese war for the purpose of clearing away the obstructions in the Peiho river.

Since that period an extended series of experiments has led to important improvements in the magnet-fuze. The priming composition is made more sensitive by the employment of a mixture of phosphide and sulphide of copper and chlorate of potash, instead of the moistened gunpowder, and is now so readily ignited that single fuzes may be fired by the smallest magneto-electric machines, such as the American apparatus (a six-inch horse-shoe magnet and rotating armature), recently introduced for medical purposes. The mode of placing the priming material in contact with the poles by the adaptation of a little capsule of sheet tin; the putting together of parts in the general arrangement; and the simple mode of making the necessary metallic connexions were explained, and the fuze in several stages of manufacture exhibited.

Professor Wheatstone, who has been associated with Mr. Abel throughout the course of these experiments, has contrived a very ingenious and portable form of magneto-electric instrument called by him the "magnetic exploder." This will fire a number of fuzes, either simultaneously or in succession, according to the arrangement of wires employed, on merely turning the handle of the instrument, and pressing the stud or key when the requisite velocity of the revolution of the magnet-armatures has been attained, and the preconcerted signal given. [Ten magnet-fuzes, placed in a divided circuit, were then exploded by Mr. Wheatstone's apparatus. The simplicity of the whole arrangement, and its complete control by the operator, became at once apparent.] For the purpose of establishing metallic communication between the magnet-fuzes and the "exploder," one insulated wire is all-sufficient; this need be but of small size, and being led out from the machine, is inserted into one eye of the fuze head; the other requires merely to be connected with the ground by a short length of ordinary copper wire attached to a small metallic plate buried in the earth close at hand. All that remains to be done in order to complete the earth-circuit is to connect the second binding screw of the "exploder" with the ground beneath, using a similar plate or, more conveniently, a short wire passed under and dug in with a spade. In the event of a number of charges requiring to be fired simultaneously (frequently the case in engineering operations), the distant extremity of the main wire is placed in communication with a corresponding number of short branches of insulated wire, each leading to a mine; the juncture of the main wire with the branches is effected by twisting the bare ends together with pliers, binding the joint round with fine copper wire to ensure proper connexion throughout, and then simply covering it with sheet caoutchouc or waterproof canvas, to maintain perfect insulation. The ground wires of the series of mines are, for greater certainty, collected into one, and secured as a single earth connexion. With ordinary attention bestowed upon the repair of any injuries which the insulated wire may accidentally sustain in the course of rough usage, no difficulty is experienced in directing the operations at a great distance from the scene of the explosion. The use of ordinary uncovered wire supported above the ground on poles or stakes, provided with insulators, was referred to as another means of conveying the magnet-current to a distance.

The modifications rendered necessary in the general arrangements for the carrying out of submarine explosions were briefly described. The obstacles in the way of success were represented to be greater in this class of operations (especially when more than one charge has to be fired) on account, firstly, of the increased difficulty in

thoroughly insulating the conducting wire and the necessary connection; and, secondly, because of the flowing in of the water upon the bared poles immediately after one or two in the series were ignited, thus, by completing the circuit, preventing the explosion of the rest. The charge of powder was enclosed either in a tin canister or a mackintosh bag, with a fuse in the midst, and connection established with the insulated wire on the one hand, whilst the other pole was placed in communication with the metal of the canister, or brought out into the water in the case of the vulcanised india-rubber bag.

From a number of experiments made at Woolwich and Chatham, with a small, portable form of hydro-electric machine, fitted up for the purpose by Sir William Armstrong, the possibility of firing many (fifty to one hundred) fuses in series was conclusively established; but, inasmuch as the management of this machine involved some special precautions, and was subject to great loss of power in unfavourable weather, its employment for field purposes was not deemed so advantageous as that of the magneto-electric apparatus which had been introduced to the notice of the Society.

It was stated, in conclusion, that Mr. Abel had found, by very recent experiments, that the latest forms of induction-coil apparatus furnished, with his sensitive fuses, results fully equal in magnitude, and of greater certainty, than those obtained in the field with the hydro-electric or frictional electric machine.

Mr. ALFRED SMEE, after drawing a comparison between the several kinds of electrical apparatus capable of employment in mining operations, stated that there was no instrument which, in his opinion, so completely fulfilled all the conditions requisite for field application, as that which had been selected by Messrs. Abel and Wheatstone. He considered that not only should the magnet be employed for all purely military purposes, but that the system was equally capable of universal application in connection with the blasting of rocks and mining operations generally. Referring to the early instances of the employment of the voltaic battery in the raising of the *Royal George*, and in the removal of large masses of chalk cliff near Dover; he attributed the abandonment of the galvanic system to the inconveniences inherent to the use of the battery.

Dr. W. A. MILLER, referring to the Lecturer's remarks on the Austrian system of employing the plate electrical machine, wished to draw attention to the recently introduced material called "vulcanite" or "ebonite," as one well fitted to take the place of glass in any instruments designed for military purposes.

Mr. ABEL mentioned that the Austrian officers in their report on this subject, allude already to the use of hardened india-rubber (another name for vulcanite), and to the probability of its replacing glass, as being less liable to the condensation of moisture, and not subject to breakage.

Mr. WARREN DE LA RUE pronounced an eulogium upon the perseverance and skill of the experimenters who had brought the magneto-electric apparatus and all the arrangements to such perfection as to accomplish that which would formerly have necessitated the employment of cumbrous machinery, with which it would, however, have been utterly impossible to have produced anything like the perfect results just now exhibited to the Society.

Royal Institution.—The following is the arrangement of Lectures for the ensuing week:—Tuesday, May 21, at 3 o'clock, Mr. John Hullah, "On the History of Modern Music." Thursday, May 23, 3 o'clock, Mr. W. Pengelly, "On the Devonian Age of the World." Friday, May 24, 8 o'clock, Professor J. O. Westwood, "On the Metamorphosis of Insects." Saturday, May 25, 3 o'clock, Professor Max Müller, "On the Science of Language."

CORRESPONDENCE.

Food, and its Adulterations.

To the Editor of the CHEMICAL NEWS.

SIR,—Valonia, as an adulterant of tea, having of late excited some interest, I send herewith a small specimen of the latter, purchased in Westbourne Grove, which contains a notable quantity of the above-named astringent. In the course of the discussion upon my recent paper at the Society of Arts, January 30, 1861—(noticed in these pages)—I believe it was stated that it was impossible, or extremely difficult, to detect valonia in tea; I did not care to reply at the time, not having just then an illustrative example, now, however, I may say that any one who will examine the enclosed "fine Japan tea" by reflected light, under a moderate magnifying power, may easily perceive the peculiar "crystalline aciculæ," characteristic of the presence of valonia.

While on the subject of Adulteration, I may perhaps take the opportunity of slightly modifying a statement of mine in the paper before referred to, concerning mustard which I said was never to be met with pure (in London). This although true at that time, is, I am happy to say, not strictly so at the present moment, for very recently Messrs. Evans, Lewis, and Co., of Diglis, Worcester, have introduced into this metropolis two qualities of mustard which are perfectly pure and genuine, and two others which differ only in containing rather more of the white mustard seed, and husk, and a minute proportion of turmeric, no other substance whatever being present. The samples I have examined were obtained both from Worcester direct, and from various retailers in London. The latter, however,—as I have found in more than one instance—will constantly adulterate this as well as all other mustards, where they sell it loose, and none can be depended upon, except in perfectly secure tins.

I need scarcely apologize for troubling you with these few remarks (which I trust will show my readiness to correct, even a trifling error, as soon as it is pointed out to me)—the importance of pure mustard, either for producing cutaneous vesication, or as a wholesome condiment for the table, being very generally acknowledged.

I am, &c.

WENTWORTH L. SCOTT.

Bayswater, London, W., May 4, 1861.

The Chemical Examination of Waters.

To the Editor of the CHEMICAL NEWS.

SIR,—As a contribution towards the knowledge likely to assist in arranging into chemical compounds the experimental results obtained in the analysis of waters, I beg leave to communicate a few observations in reference to this subject.

Much reliance appears to have been placed on the indications afforded by vegetable colours, as a means of determining the presence of alkaline carbonates among the constituents dissolved in any sample of water under examination; but at the same time that the value of such tests, properly applied, must be admitted, there appears to be a necessity for drawing a distinction between two classes of indications which have hitherto been frequently confounded.

Thus, it is the property of carbonate and bicarbonate of potassa to exercise a direct influence upon the natural colour of turmeric paper, and to change it from yellow to brown. The same alkaline salts have no direct action upon litmus, blue being the natural colour; but if this vegetable infusion be previously reddened by an acid, and red litmus paper be the test employed, the effect of the alkaline carbonate can then only be properly referred to

its power of neutralising this extraneous acid, and in consequence restoring the natural colour of the litmus.

In carrying out some experiments, a short time since, for the purpose of observing critically the reactions afforded by the carbonates of lime, baryta, and magnesia, when dissolved in water impregnated with carbonic acid, the distinctions between the metals of the alkali group and those just now enumerated, appeared very decisive. When carefully prepared turmeric paper was the test employed, it was found that whilst very small proportions of the alkaline carbonates were readily detected, no direct action was apparent with pure solutions of the carbonates of the alkaline earths. When reddened litmus paper was the test used, it was found to be equally affected (restored to blue) by all the carbonates indiscriminately.

With this distinction clearly in view, I have frequently employed turmeric paper as a test for the presence of alkaline carbonates in natural waters. Many samples have not afforded the slightest indication. Artesian well-waters from different parts of London have all furnished direct proof of the existence of an alkaline carbonate among their soluble constituents. The sample of water from the coal strata under Bradford Moor, described by Mr. Abel,¹ possessed in a high degree this alkaline property, evidenced by turmeric, and easily accounted for by its containing no less than 43·5 grains of bicarbonate of soda in the gallon of water.—I am, &c.

JOHN SPILLER.

Woolwich, May 13.

MISCELLANEOUS.

Conversazione at the Society of Arts.—The Members of the Society of Arts held their first Conversazione this Session at the Society's House, Adelphi, on the evening of May 4th. A number of interesting objects were exhibited, including the spectrum apparatus of Bunsen and Kirchhoff, as manufactured by Mr. Ladd, Wenham's binocular microscope, by Messrs. Smith, Beck, and Beck, Messrs. Powell and Eland, and Mr. Baker; Abel's new mine or cannon fuses, some specimens of gutta-percha and varnishes, Indian "talc" (Mica), paintings, &c., &c. The "Exhibition of Recent Inventions" was also thrown open, and in the great room a miscellaneous series of transparent photographs were projected on to a large screen by means of the oxy-hydrogen light, by Mr. C. Jones. The scenes in Egypt and Italy were specially interesting, and the latter included a good portrait of Garibaldi (described as a "party" everyone would recognise) entirely spoilt by the colouring. The evening passed off in a manner which must have proved very satisfactory both to the Council, and to the large assemblage which responded to their invitation.

The Leicester Sewage Works.—With regard to the cost of deodorising the sewer water at Leicester, as the facts connected therewith have been occasionally misapprehended or misrepresented, the following information may not be considered misplaced in a report of this nature. The works erected there by a private company having failed to yield any remuneration, owing to the fact that the great dilution of sewage rendered the proportion of fertilising matter precipitated therefrom too small to be of appreciable value, they were (according to a stipulation in the original agreement) handed over to the Town Council of the borough, who have been in possession of, and have carried them on for above twelve months. It is not generally understood that these works consist of two parts. First, of the pumping engines employed for the purpose of lifting the water at the artificial outfall of the main sewer, which was made considerably lower than the

natural one, which latter was at too high a level to allow of the complete drainage of the lower portions of the borough, and recourse was had to this plan of an artificial outfall, because it was cheaper than the construction of a natural outfall sewer would have been, which (owing to the nature of the locality) it would have been necessary to carry many miles below the borough, in order to reach a level low enough to create a sufficiently rapid current in the main sewers, but this plan was not finally determined upon without a thorough investigation of its merits, and the result has justified its adoption. The second part consists of the deodorising tanks and machinery. The actual cost of the disinfecting process carried on at these works during the twelve months ending November, 1859, was as follows:—The quantity of sewer water deodorised being 1,061,757,200 gallons, or 4,740,000 tons, equal to 2,908,920 gallons per twenty-four hours, or 414 gallons per diem per head of the population. The quantity of slacked chalk lime used was 580 tons, which cost £369 15s. The cost of labour in slacking and mixing the lime amounted to £78. The coals consumed by a small steam engine for working the agitators, and raising the precipitated matter from the bottom of the tanks, amounted to seventy-three tons, and cost £25 11s.; and the cost of labour for working the engine, attending to the tanks and machinery, and for attending to and storing the manure or precipitate was £76 14s., making a total of £550, which is equal to a cost of less than twopence per annum per individual, or twopence for every 16,000 gallons of sewer water deodorised. These works have now been in efficient daily and continuous operation for the last six years, during which period all the sewer water from the borough has been thoroughly disinfected and rendered innoxious before it has been permitted to flow into the river Soar; and to those who can recollect the condition of the Soar at the North Bridge, above and below, in former years, it will perhaps appear a desideratum that was worth paying for to obtain. These works, although commercially a failure, for the reason before given, have been most successful as a sanitary measure, and although the gentlemen who expended large sums in the solution of this important question have been great pecuniary losers, nevertheless, the borough of Leicester has derived great benefit from their operations; and I believe it is an undisputed fact that the sewerage of the borough has been more completely and economically effected than in any other town in England.—*Thos. Wicksteed, C.E.*

ANSWERS TO CORRESPONDENTS.

. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Magenta Dye.—Cotton without a Mordant writes as follows:—In reading the *CHEMICAL NEWS* for April 27, 1861, I find a letter signed "Sharp, Leeds," saying that Magenta cannot be dyed on cotton without a mordant; but I beg to say that he is labouring under a mistake, as I have for the last three or four months both dyed it in the cotton hank and in the cotton warp, without a mordant, and I can get by far the better colour without, than with any mordant.

Green Extract.—1. There is no proximate principle in yeast which may be separated from it and be used to induce fermentation. Yeast globules are necessary for this purpose. 2. The crystals should be examined—are they organic or inorganic, acid, basic, or neutral?

Ammonia will be answered in a subsequent number.

C.R.—1. We think it is uniformly distributed. 2. It has only been proved in a few instances. 3. In ordinary analysis it would be overlooked. 4. It could only be proved by searching Chemical analysis. Perhaps we could assist you better if you were to communicate privately with us.

A Subscriber.—Papers on the subject are to be met with in the recent volumes of the *Philosophical Magazine*. No connected account is to be met with. Your coil cannot be modified. It may be used as it is, but it will only produce a few of the effects to a very limited extent.

¹ *Philosophical Magazine*, May, 1860.

THE CHEMICAL NEWS.

VOL. III. No. 77.—May 25, 1861.

WATER ANALYSIS.

On a recent occasion¹ we had an opportunity of pointing out some notable discrepancies which have arisen from the want of a good general system of classification, according to which the numerical results furnished by the chemical examination of waters might be arranged, and represented in their probable state of combination, or as saline constituents actually present in the water. With the object of eliciting a more decided expression of opinion in regard to this subject, we advert to some additional considerations which seem to necessitate an early and careful review, in order that this frequently-recurring problem, water analysis, may be placed without delay on a more satisfactory foundation.

So little progress appears to have been made in this direction that the remarks of Fresenius, pronounced in the early days of quantitative chemistry, may yet be held as strictly applicable to the present time. This distinguished analyst says:—"With respect to the principles which guide chemists in inferring in what manner the acids and bases are combined as salts in the water, it is assumed that they are united according to their relative degrees of affinity, i.e., the strongest acid is assumed to be combined with the strongest base, &c.—due attention being paid, however, to the greater or less degree of solubility of the salts, since it is well known that this exercises a considerable influence upon the manifestations of the force of affinity. Thus, for instance, when lime, potassa, and sulphuric acid are found in the water after boiling, the sulphuric acid is assumed to exist by preference in combination with the lime. It cannot be denied, however, that this way of proceeding leaves much to the individual views and discretion of the analyst, and consequently that different modes of associating the several ingredients found may lead to different results for one and the same analysis. A general understanding upon this point would be very desirable, because without it the comparison of two mineral waters is beset with difficulties. As long as a general understanding of this kind is wanting, a comparison between mineral waters can only be made with the direct and immediate results of the respective analyses."

The system recommended by Fresenius may be concisely stated as follows:—To boil a measured quantity of the water for a time sufficient to precipitate the whole of the carbonates of lime and magnesia originally dissolved in the water in that form; should carbonate of iron be also present, it will be separated as peroxide in the operation of boiling. The amounts of these constituents severally determined will represent the proportion of lime, &c., in combination with carbonic acid. The lime and magnesia still remaining in solution, and the alkaline bases have then to be estimated. Lastly, the sulphuric acid, chlorine, silica, and other constituents

present in smaller proportion may be determined in the original water. As already stated, the lime not precipitated by boiling should be combined with sulphuric acid; any excess of this acid being placed in union, first with potassa, then with soda; the rest of the sodium to be represented as chloride, and in a similar state of combination also that portion of the magnesium which has not been precipitated on boiling in the form of carbonate. Nitric acid may, however, sometimes aid in retaining the magnesia in a permanently soluble form.

With regard to such samples of water as contain alkali partly in the form of carbonate (readily ascertained by adopting the process referred to by Mr. Spiller, in a letter inserted last week in our columns), the lime and magnesia must in such cases be stated as carbonate; and sulphuric acid, if likewise present, all in the form of alkaline sulphate.

A mere casual reference to the numerous published tables of analyses describing the constitution of mineral waters will be sufficient to convince an unprejudiced mind of the necessity for urging the importance of this subject, and taking steps to obviate the inconvenience arising from the want of an uniform system of tabulating the analytical results.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Expansion of Metals and Alloys, by F. CRACE CALVERT, Ph.D., F.R.S., F.C.S., &c., R. JOHNSON F.C.S., and G. CLIFF LOWE.

HAVING been engaged for some time in investigating several of the properties of pure metals, it was thought desirable to take advantage of having pure metals at our disposal, together with a series of definite alloys of those metals, to determine their rate of expansion; and we were encouraged in pursuing this investigation by finding that several of the authors who had previously published Tables of the Expansion of Metals differed widely in their results. These discrepancies, having reference to some metals most extensively used, might, we thought, be due either to the method employed, or the fact that metals of different degrees of purity had been experimented upon. Therefore, being sure of the purity of the metals that we intended to employ, we had recourse to a method, the accuracy of which, we trust, will appear satisfactory.

Owing to the difficulty of obtaining the metals in a pure state, in large quantities, we found it necessary to employ square bars, having a length of 60 millimètres by 10 millimètres in diameter, and devised the following process to determine with accuracy the expansion of such short bars. This we believe we have effected, as our apparatus will easily indicate an expansion amounting to the 50,000th part of an inch, or about the 2000th part of a millimètre.

¹ CHEMICAL NEWS, vol. III. p. 285.

Description of the Apparatus Employed in Determining the Expansion of Metals and other Bodies by Heat.

A piece of stone is let into a thick, strong wall (part of the basement of a large building), and very securely fixed, with a portion projecting out of the wall about $4\frac{1}{2}$ inches in thickness, and having a level surface of about 6 inches square, on which the body to be measured is placed; $2\frac{1}{2}$ inches above this a steel stud is firmly fixed, projecting from the face of the wall and ground exactly to fit the hole in a brass bush which is fixed in a lever. The steel stud serves as the fulcrum on which the lever moves freely, but without any shake. The lever is a long piece of oak, about 1 inch thick and $3\frac{1}{2}$ inches broad at the fulcrum, and having a long and short arm, the longer one being 34 times the length of the shorter one. The long arm is tapered in thickness and breadth from the fulcrum to the end, where it is about $\frac{1}{2}$ of an inch thick and $1\frac{3}{10}$ inches broad. The stone and the fulcrum are placed at such a distance from the termination of the wall as to allow the long arm of the lever to project a few inches beyond it. The steel fulcrum is so fixed as to cause the lever to move parallel to the face of the wall, at a distance of about $\frac{3}{4}$ ths of an inch from it. The lever is maintained in a horizontal position by a string carried up from the extremity of its longer arm to a steel bar, to the end of which the string is hooked. This bar is suspended like the needle of a mariner's compass, and has in the middle a conical hollow, which allows it to move freely up and down upon a steel pillar with a fine conical point. This is fixed to the wall, about 3 feet above the height of the long arm of the lever. The end opposite to that to which the long arm of the lever is attached by the string has a wire depending from it, on which weights may be placed sufficient to counterpoise the long arm and maintain the lever in a horizontal position.

Thus, it will be evident that if the bar of metal to be experimented upon be placed with one end on the stone and the other under the short arm, any elongation of the metal will produce a depression of the opposite end of the lever, and, on the contrary, as the metal contracts in cooling, the short arm will follow it and the long arm of the lever will rise, and, as it is 34 times longer, it will move through 34 times the distance. To prevent any unsteadiness in its movements, sufficient weight is attached to the counterpoise to cause the lever to press firmly on the metal bar to be measured, and thus, when heat is applied, the bar raises the short arm as it expands, and as it contracts, in cooling, is followed by the short arm with the utmost regularity.

We will now describe the method employed in reading off the space through which the long arm of the lever is moved by the expansion or contraction of the metal experimented upon. A circular hole of $1\frac{1}{4}$ inch diameter is made in the lever, the centre of which is 34 times further from the fulcrum than the end of the short arm; into this hole the object-glass of a small telescope is fitted. It will be evident, therefore, that if any object be placed a few yards in front of the object-glass its image will be received upon any surface placed behind it at a proper distance. This distance in the present case is 12 inches, when the rays of light from the object are brought to a focus. The eye-piece of a telescope, of considerable magnifying power, is used to receive the image upon. An ivory scale is supported in a vertical position upon a stand opposite to the object-glass, and, on looking through it with the eye-piece, the divisions of the rule are distinctly seen. A very fine line is placed

in the eye-piece, in the middle of the field. The eye-piece is attached to the end of the wall, and is provided with screws, one to adjust the focus, and the other to raise or lower it in a vertical direction, so that the line in the middle of the field may be brought into a coincidence with any of the divisions upon the rule. The rule used in making the experiments is 12 inches long, each inch divided into 60. These, though not distinguishable by the naked eye, are easily read by the aid of the object-glass and eye-piece at the distance required. To find the distance at which the rule should be placed to make one of the divisions equivalent to a change of $\frac{1}{100000}$ th of an inch, the following method is adopted:—The eye-piece was adjusted so that the line drawn across the field stood at zero on the rule. Then, by means of the adjusting screw, the eye-piece was raised exactly half an inch, the object-glass remaining unmoved. As the eye-piece is raised the spider line appears to traverse a certain number of the divisions. The number of divisions it ought to pass over by raising the eye-piece half an inch is between 367 and 368, and by placing the graduated rule about 14 feet from the eye, the spider line is seen to move from 0 to the 368th division. Then, as 368 to the half inch is equal to 736 to the inch, the line in traversing the space between any two divisions moves through $\frac{1}{736}$ th part of an inch. Now, in making experiments in the expansion of bodies, the object-glass moves, while the eye-piece remains stationary. If we suppose, then, that under the short arm of the lever a bar is placed and heat is applied sufficient to elongate it $\frac{1}{100000}$ th part of an inch, the spider line will appear to ascend from the division it stood at to the one above it, because the long arm being 34 times the length of the short one, it will move through 34 times the space. We must, therefore, multiply 736, the number of divisions in an inch, by 34, which gives 25,024; so that an elevation or depression of the object-glass sufficient to cause the spider line to traverse one division of the rule, is equivalent, in round numbers, to a change in length of the bar of $\frac{1}{250000}$ th of an inch. With a little practice, the eye can readily estimate a change in the position of the spider line of one-half or even one-quarter of the space between two divisions, thus reading off the $\frac{1}{100000}$ th of an inch.

We will now describe the method of raising the temperature of a bar from its natural temperature to 90° Centigrade. A tin vessel, about 2 inches square, and of such a depth as to contain sufficient water to immerse the bar, is placed beneath the short arm of the lever. The bottom of the vessel is of brass, $\frac{1}{4}$ th of an inch in thickness. At the angle furthest from the bar, and close to the bottom, a tube is inserted to admit steam; and at the angle nearest to the bar, and at the surface of the water, is an outlet to carry off the overflow. The vessel is covered with a lid, in which are two apertures,—one to allow the end of the bar to come through, and the other for the insertion of a standard thermometer, divided into tenths. The experiments were made in the following manner:—The bar to be tried is placed in the vessel, with the short arm of the lever resting upon its upper end. One operator then pours water into the vessel and allows it to run out till the thermometer descends to a fixed point. At this instant another operator sets the spider line at zero. The tube at the lower angle of the vessel is then connected with a flask in which steam is generated, which is passed by this tube through the water in the vessel until it is raised to the temperature of 90°, at which it is maintained until the bar arrives at a fixed temperature. The degree of expansion is then read off by the observer at the eye-piece. The tube is

then disconnected and cold water is passed through the vessel repeatedly until a fixed temperature is again obtained. Another reading is then taken. These experiments are several times repeated until a number of coördinate results are obtained. To attain this we had no difficulty, as our readings varied only a few hundred-thousandths of an inch from one another, as will be seen by the series of experiments annexed:—

Experiments to Determine the Expansion of a Bar of Hardened Steel.

Heating from 16° to 99°.

	1st.	3rd.	5th.
16° =	289	279	276
99° =	358	345	343
83° =	69	66	67

Cooling from 99° to 16°.

	1st.	3rd.	5th.
99° =	354	347	345
16° =	285	279	276
83° =	69	68	69

1st.	9
3rd.	
5th.	67

3)202

Mean for 80° . 67·3

2nd.	69
4th.	68
6th.	69

3)206

Mean for 83° . 68·7

Mean for 100° 82·8

1st.	69
2nd.	66
3rd.	67
4th.	69
5th.	68
6th.	69

6)408

Mean . . . 68

Mean of all readings } 68
for 83°

Readings for 100° . 81·9

These results, however, require a deduction to be made from them on account of the expansion which takes place in the brass bottom of the vessel, and also in the stone upon which it rests. To ascertain this exactly the following experiments were made:—A rod of the same steel, nearly ten times the length of the short bar, is suspended by a steel pin inserted in the wall at such a height as to allow the short end of the lever to rest upon another steel pin, passing through the lower extremity of the long bar. It is enclosed in a glass tube, extending a few inches above the upper end of the bar, and open at the top. It is closed at the bottom, with the exception of an opening for the admission of a jet of steam. The steel bar, thus suspended by the two steel points, does not partake of the expansion of the tube or any other part of the apparatus. It is then submitted alternately to the action of steam and cold water, and the expansion, observed as before, gave us the following results:—

Heating from 16·5° to 100°.

	1st.	3rd.	5th.
16·5° =	625	624	618
100° =	853	851	845
83·5° =	470	473	473

Cooling from 100° to 16·5°.

	2nd.	4th.	6th.
100° =	160	155	151
16·5° =	625	624	618
83·5° =	465	469	467

Ascending Scale. Descending Scale.

Mean observed for 83·5° . . . 472 467
Ditto calculated for 100° . . . 565·4 559·3

Mean of the above bar 547 millimètres long for 100° 562·3
Ditto, calculated for 60 millimètres . . . 61·7
Mean of the small bar for 100° . . . 81·9
Ditto of the above long bar for 100° . . 61·7

Correction 20·2 (say 20·6)

From these figures it will be seen that it is necessary to deduct 20 from the observed expansion of the short steel bar from 0° to 100°, which excess we found to be due to the expansion of the bottom of the tin vessel, together with that of the stone upon which it rested. The correctness of deducting 20 from the results of all observations upon the short bars for the expansion of 100° Centigrade was further confirmed by employing several different modes of investigation.

Before giving the results, we think it our duty to make one or two remarks upon the apparatus:—

1st. If the plaited string which connects the lever with the counterpoise should stretch with the weight upon it, or be contracted or expanded by change of temperature, no effect is produced in the position of the lever during an experiment, for the action of the counterpoise is simply to keep the short arm upon the bar experimented upon, and the pressure still keeps it there, however the length of the string may vary. This was determined by direct experiment.

2nd. The spider line of the eye-piece always remains fixed across the centre of the field, so that the observer always looks through the centre of the lens; but it happens sometimes that the centre of the object-glass is not exactly opposite the eye-piece. The expansion of the bar may have raised it so far that the observer may be looking through the object-glass near its edge. This does not, however, sensibly affect the correctness of the reading, and repeated experiments have proved that it does not entail the error of $\frac{1}{10000}$ th of an inch. By proper precaution, however, this error may be avoided, as there are only one or two metals that expand so much as to throw the centre of the object-glass much above or below the spider line.

3rd. By taking the precaution of introducing the jet of steam at the bottom of the vessel, and at the point most distant from the bar, and allowing the overflow to pass out at the angle nearest to the bar, we secured perfect uniformity in the temperature of the water in every part, except in the immediate neighbourhood of the opening through which the steam was introduced. This was carefully ascertained in the course of our experiments, by placing our standard thermometer in various parts of the water. When an open tube was used, the steam was passed through till a fixed temperature of 100° was obtained at its open extremity.

Lastly. In making the experiments we have observed that the expansion for each 10° has been in the same ratio as that from zero to 100°.

(To be continued.)

On the Amounts of Lead contained in some Silver Coins, by Messrs. CHARLES W. ELIOT and FRANK H. STOREY.

FROM our experiments upon the impurities of commercial zinc,¹ we found that this metal almost invariably contains lead. In the preparation of silver at the United States Mint, zinc is used for the purpose of reducing chloride of silver,² and a sample of zinc similar to that used at the Mint, which we examined, yielded half of one per cent. of lead. The question naturally suggested itself whether lead might not thus find its way into American silver coin, and to determine this point we have analysed several American coins, as specified in the following table. For the sake of comparison we subsequently examined the other coins therein enumerated.

(1)	(2)	(3)	(4)	(5)	(a) ³	(b) ³	(c) ³
Kind of Coin.	Weight of Coin taken.	Weight of BaO, SO ₂ found.	Weight of Pb corresponding to SO ₂ in the BaO, SO ₂ of column (3).	Corresponding per cent. of Lead in the Coin.	Weight of PbO, SO ₂ found.	Corresponding weight of Pb.	Corresponding per cent. of Lead in the Coin.
	Grms.	Grms.			Grms.		
1 American half-dollar of 1824	13.2936	0.0465	0.0412	0.3100	0.0480	0.0327	0.2462
20 American five-cent pieces of 1853	24.2630	0.0571	0.0507	0.2090	0.0555	0.0379	0.1560
10 American five-cent pieces of 1854	12.1980	0.0314	0.0278	0.2282	0.0270	0.0185	0.1513
2 American twenty-five cent pieces of 1858	12.4097	0.0322	0.0286	0.2305	0.0310	0.0273	0.2209
"Fine silver" ⁴ from the U. S. Assay Office in New York, 1860...	30.6405	0.0557	0.0494	0.1611	0.0655	0.0447	0.1457
1 Spanish dollar of 1793, Carlos IV.	27.0130	0.0170	0.0151	0.0558	0.0129	0.0088	0.0326
1 Mexican dollar of 1829...	27.2265	0.0127	0.0118	0.0434	..	..	..
2 English shillings of 1816	10.4597	0.0537	0.0507	0.4847	0.0590	0.0422	0.3846
1 French five-franc piece of 1852, Napoleon III.	24.9725	0.1135	0.1069	0.4282	0.1206	0.0886	0.3546

On the supposition that the zinc used in the reduction of the silver is the source of the lead in the American coin, it is easy to calculate the amount of lead which would thus find its way into the coin, since the quantity

¹ Memoirs of the American Academy, 1860 [N. S.], viii. 57.

² Booth and Moritt's Smithsonian Report on Recent Improvements in the Chemical Arts, (Washington, 1851), p. 56. Compare Wilson's Report on the New York Industrial Exhibition, in *Dingler's Polytechnic Journal*, 1855, CXXXV. 119.

³ Columns (a), (b), and (c) contain the results of a supplementary series of experiments, made merely to control the results given in columns (4) and (5). (Vid. infra.)

⁴ The solution of this fine silver in nitric acid became blue when neutralised with ammonia. The filtrate from the mixed precipitate of sulphate of lead and gold contained a decided trace of copper and a fainter trace of iron. The solution of sulphate of soda, from which the sulphuric acid of col. (3) was determined, exhibited a slightly yellowish-light-brown color, nothing similar to which occurred in any of the other experiments. A slight black residue remained when this silver was dissolved in nitric acid, and a trace of gold was detected in the residue.

of zinc used in reducing a given weight of silver, and the per cent. of lead which that zinc may be expected to contain, are both known quantities. Professor Booth⁵ says that an excess of zinc is required to insure total and rapid reduction, and Wilson⁶ states that two equivalents of zinc are used, in practice, for each equivalent of silver. Our memoir, already cited, gives the per cents. of lead found in two specimens of Vieille Montagne zinc. The standard of the American silver coin is $\frac{1}{10}$ silver and $\frac{1}{10}$ copper, and the weight of fifty cents' worth of this alloy, in either half-dimes, dimes, quarters, or a half-dollar, has been 192 grains = 12.433 grammes, since the year 1853.⁷

Fine silver in the half-dollar . . . 11.190 gram.
Zinc used in reducing 11.19 gram. silver . 6.742 "
Amount of lead in 6.742 gram. zinc, if the zinc contains 0.292 per cent. of lead⁸ . 0.0197 "
Amount of lead in 6.742 gram. zinc, if the zinc contains 0.494 per cent. of lead⁸ . 0.0333 "

If zinc of the best quality (containing 0.292 per cent. of lead) had been used, the silver coin would have contained 0.158 per cent. of lead; if the second quality (containing 0.494 per cent. of lead) has been employed, the coin may contain 0.268 per cent. of lead. Between these two limits all our determinations of lead in American silver will be found to lie.

In offering this explanation of the occurrence of lead in American silver coin, we would by no means affirm that the zinc is the exclusive source of this impurity, for it is not at all improbable that a portion of the lead is derived from the leaden vats in which the reduction of the chloride of silver is effected, or from the sulphuric acid which is used to excite the reaction.

The method of analysis employed was as follows:—The coin having been dissolved in moderately strong, pure nitric acid, the cooled solution was treated with an excess of a solution of pure caustic ammonia, added by small portions,—no notice being taken of the small, dark-coloured residue (AgS, etc.) insoluble in nitric acid. The precipitated oxide of silver was re-dissolved in nitric acid, enough of the latter being added to render the solution distinctly acid. The whole was then heated in a water-bath, and a strong solution of pure chloride of ammonium added, until chloride of silver was no longer precipitated, care being taken to avoid adding an unnecessary excess of chloride of ammonium. It will be seen that the chloride of silver was thus precipitated in presence of a large excess of nitrate of ammonia; and since chloride of lead is readily soluble in a solution of nitrate of ammonia,⁹ any lead which the coin may have contained would remain in solution. It may be mentioned in passing, that the small amount of chloride of silver which, as has already been stated by Mulder,¹⁰ is retained in solution by nitrate of ammonia—especially by hot solutions—did not interfere in the least with the subsequent steps of the analysis, or in any way to influence the amount of lead obtained. The precipitated chloride of silver was collected upon a filter, and thoroughly washed with hot water, the lumps of chloride of silver being broken down as much as possible with a glass rod. The filtrate and wash waters were evaporated in a porcelain dish to the consistence of a thin syrup, and

⁵ Loc. cit.

⁶ Loc. cit.

⁷ Brightly's Dig. Laws U.S., for Standard, Title Coinage, s. 3; for Weights, Title Coinage, s. 13.

⁸ See our Memoir in Mem. Amer. Acad. [N. S.], viii. 62, Table I.

⁹ Bolley, *Ann. Ch. u. Pharm.*, 1854, xci. 115.

¹⁰ "Die Silber-Probirmethode," (Leipzig, 1859), p. 28.

then transferred to a flask of hard German glass, in which the evaporation was continued until the nitrate of ammonia had been entirely destroyed. This operation requires care in the application of the heat, lest violent decomposition of the nitrate of ammonia ensue, and portions of the substance be projected from the flask. When properly conducted, the process goes on tranquilly, and no loss occurs. The residue, consisting of nitrate of copper, together with nitrate of lead, and more or less gold, should be washed with hot water into a porcelain dish, where it is mixed with a slight excess of pure sulphuric acid. It may be necessary to use a little nitric acid to dissolve the last portions of the substance in the flask. The mixture is then evaporated over the water-bath until it has become almost entirely dry, or until it no longer exhibits any tendency to effervesce. It is then somewhat more strongly heated on a sand-bath, until all the free sulphuric acid has been driven off—that is, until fumes of sulphuric acid are no longer perceptible. The necessity of thoroughly evaporating this mixture cannot be too strongly insisted upon, for a solution of gold in sulphuric acid¹¹ is formed when the mixed nitrates, &c., are treated with sulphuric acid, and unless this compound be entirely destroyed by heating, it will be subsequently decomposed when water is added, and give rise to a precipitate of metallic gold, in a condition so finely divided that it cannot be separated by filtering. The occurrence of this precipitate would ruin the analysis. We at first sought to prevent the formation of this solution of gold in sulphuric acid by heating very strongly the residue left in the flask after all the nitrate of ammonia had been decomposed, until the escape of nitrous fumes had entirely ceased, and the nitrate of copper was decomposed. We then treated the mass with pure dilute nitric acid, and filtered off the solution from the gold which had been deposited and from any chloride of silver which had been dissolved by the nitrate of ammonia, with the intention of determining the lead directly, by precipitating it as sulphate from this solution after evaporation with sulphuric acid. It was found, however, that a large portion of the gold-salt escaped decomposition in the flask, the yellow solution of sulphate of gold being formed almost as abundantly after this treatment as when the flask was not heated so strongly; and since in this method there is great risk of fracturing the vessel from drops of acid condensing in its neck and flowing down upon the hot portion, it can in no wise be recommended. A perfectly dry mixture of sulphate of copper, sulphate of lead, and metallic gold having been obtained, it is treated with distilled water, a considerable quantity of the latter being added at once, and the mixture quickly stirred to prevent the sulphate of copper from forming a hard cake as it becomes hydrated. As soon as the sulphate of copper is completely dissolved, a small quantity of pure sulphuric acid is to be added to the solution, and the latter set aside for at least forty-eight hours. The precipitate, consisting of sulphate of lead and metallic gold, was then collected upon a small Swedish filter and washed, first with dilute sulphuric acid to remove the sulphate of copper, and subsequently with alcohol, until no trace of free sulphuric acid remained. After drying, the precipitate of sulphate of lead and gold, with the filter cut into small pieces, was transferred to a beaker glass, and the whole was then digested with pure bicarbonate of soda¹² (prepared from

the oxalate), during at least forty-eight hours. The sulphate of soda formed by this operation was then filtered off from the carbonate of lead and metallic gold, and the amount of sulphuric acid which it contained determined by precipitation with chloride of barium in the usual way. From the amounts of sulphate of baryta thus obtained, recorded in column 3 of the table, the amounts of lead (col. 4) in the mixed precipitates of sulphate of lead and gold were calculated.

The carbonate of lead, mixed with bits of paper and gold, above mentioned, was now dissolved in dilute nitric acid, the solution evaporated with sulphuric acid until all the nitric acid had been expelled, subsequently treated with water acidified with sulphuric acid, and, after standing for forty-eight hours, collected upon a tarred filter, washed with dilute sulphuric acid and alcohol, dried at 100°, and weighed. The results may be found in column (a) of the table.

The re-agents used in the foregoing operations were all chemically pure. A special experiment was moreover made, in which portions of the nitric acid, ammonia, chloride of ammonium, and sulphuric acid used, twice as large as any which were actually employed in the analysis of either of our samples of coin, were evaporated together, and subjected to a course of treatment identical with that which the solutions of coin were forced to undergo; but not a trace of sulphate of lead, or of any precipitate, other than a few light and utterly insignificant floating flocks, probably alumina, could be detected as the result of this trial.

That our process is sufficiently accurate to exhibit *all* the lead which the samples of coin examined really contained, we do not think probable. All of the sources of error to which the process, when properly conducted, is subject, tend to cause a slight loss of lead. Thus, in the first place, it is scarcely possible that the last traces of chloride of lead can be removed by washing from the caseous precipitate of chloride of silver,¹³ which precipitate is in enormous mass as compared with the amount of chloride of lead; and again, the first precipitate of sulphate of lead, mixed with gold, which is obtained after evaporation with sulphuric acid, occurs in so finely divided a state that it is exceedingly difficult to prevent loss by the passing of a portion of it through the pores of the filter upon which the precipitate is collected. To one unused to the process, it might seem as if the last traces of sulphuric acid could hardly be removed from this precipitate by washing with alcohol. We have satisfied ourselves, however, that this is easily effected, and that this seeming source of error is, in reality, entirely insignificant; at any rate, the results given in columns (a), (b), and (c) are in no way influenced by this imaginary error. It may not be amiss to observe, that we have been particularly scrupulous with regard to the treatment of the above-mentioned precipitate, having in every instance continued to wash it with alcohol long after any reaction of sulphuric acid could be detected. It should also here be stated, that the results in which we ourselves place the most confidence are those obtained by determining the amount of sulphuric acid in the mixed precipitate of sulphate of lead and gold (see cols. (3), (4), and (5) of the table), for columns (a), (b), and (c) contain the results of a secondary process liable to all the accumulated errors of a long series of operations. Moreover, the final precipitate of sulphate of lead is very finely divided, and, like the first precipitate, liable to pass through the pores of the filter, as has been

¹¹ Compare Pelletier, "Ann. Ch. et Phys." [2], xv. 12.

¹² See Fresenius, "Anleitung zur Quantitativen Analyse," (Braunschweig, 4te Aufl. 1858,) p. 286, s. 152, II. b. β.

¹³ Compare Mulder, *op. cit.*, p. 217.

previously explained. This second series of experiments has been carried out, not from any expectation that the results would closely agree with those of the first series, but merely to control the latter to a certain extent, and to remove any objection which might be made to the indirect method by which they were obtained. Since the possible inaccuracies of the process tend to a loss of lead, we are confident that the amounts of lead we have obtained are in no instance greater than those really existing in the coin.

The precipitates of pure sulphate of lead (column (a) of the table) were, in every instance, reduced on charcoal with carbonate of soda, and the button of lead obtained carefully tested for silver, of which none could be detected in any case; nor did the metal exhibit any reactions, other than those of pure lead.

(To be continued.)

TECHNICAL CHEMISTRY.

On the Composition of Cast Iron and Steel, by M. E. FREMY.

THE phenomena determining the production of steel have always occupied the attention of chemists and manufacturers, but at the present moment the study of this question has assumed an exceptional importance. In fact, the construction of machines and the manufacture of fire-arms are extending the applications of steel, and necessitate its production as economically as possible consistently with preserving its valuable properties. To resolve this problem, of so much interest to metallurgical industry, chemistry ought to throw fresh light on the theoretical questions relating to the production of steel, and to free manufacture from the uncertainty and empirical methods which retard its due development.

The theories hitherto proposed to explain the phenomena of steeling are evidently inadequate to guide the metallurgist wishing to produce steel either by the cementation of iron by charcoal or by decarburising cast iron by special puddling.

Thus, the influence of manganese and that of tungsten in the process of steeling is not easily explained. The utility of nitrogenised organic substances and certain saline matters in aiding solution is denied by experienced metallurgists. Some are of opinion that the best quality of cemented steel is obtained by the action of carbon on pure iron; others, that cementation can take place only under the influence of the nitrogen of the air. In either case, theory does not tell us why certain irons yield the best steel, while others, to all appearance as pure, produce a very inferior quality. It is well known that the difficulties in the fabrication of steel obtained by puddling are such as often to discourage the most skilful manufacturers.

This uncertainty in the methods of steeling is found likewise in the theories proposed for explaining the production of steel. Some chemists are of opinion that solid carbon acts directly on iron, penetrates the metal, and, circulating in its mass, converts it into steel. Others, among whom Leplay and Laurent are conspicuous, believe that cementation is invariably owing to the action of a gaseous carburetted compound on iron. Laurent goes so far even as to say that in the cementation-boxes the vapour of the volatilised carbon effects the acieration.

The action exercised on iron by cyanides widens the field of the theory of cementation. A familiar experiment in chemistry has been utilised in practice: it consists in converting iron into steel by heating it with an alkaline cyanide or ferrocyanide. And, again, M. Caron, in a recent and interesting communication to the Academy, states that cyanide of ammonium, which is formed in the cementation-boxes, acts on iron like alkaline cyanides and rapidly converts it into steel.

The papers published on the subject of acieration have, doubtless, enriched science with many new and important facts; above all, they have particularised the circumstances which appear to determine this process most readily, but they have thrown no new light on the theoretical questions relating to the chemical constitution of steel. The writers still aver that steel is a carburet of iron, which they place chemically between iron and cast iron.

My opinion as to the composition of steel differs entirely from those hitherto accepted. I hope to be able to prove that steel is not a carburet of iron, and that there exists a series of steels resulting from the combination of iron with metalloids, metals, and even with cyanuretted bodies.

I am not acquainted with a single unequivocal experiment tending to show that steel is a combination of pure carbon and iron. Minute proportions of foreign bodies, not always detectable by analysis, can modify the properties of iron. When it is proposed to study the action of pure carbon on iron, there are necessarily other bodies present besides those whose mutual action we desire to determine. Without referring to the impurities taken up from the crucible, there are the influence of the gases from the furnace, which penetrate the apparatus; the action of the atmospheric elements which the charcoal does not absorb; or the presence of the various substances contained in the charcoal itself,—all of which circumstances are wholly disregarded.

In an experiment with diamond-dust, which I shall presently state, the influence of foreign bodies have been equally unheeded.

I shall now recal the fact I have already submitted to the Academy, which is, that steel, when dissolved in acids, leaves a residue in no point resembling pure carbon, and which, by its properties and composition, is nearly allied to certain cyanuretted products; thus, both synthetical and analytical experiments are far from proving that steel contains nothing but carbon and iron.

To determine the true constitution of steel, and to ascertain whether there does not exist a series of bodies differing in composition, as tungsten steel differs from that of charcoal, yet allied by certain common properties, I propose to submit iron to the action of all the bodies capable of influencing the phenomenon of acieration.

I think that nitrogen ought to take the first place in this examination. Such was the object of my last communication to the Academy.¹

I have applied myself to free nitride of iron from the excess of metal it is capable of retaining, and to produce, as far as possible, a definite compound.

But in the nitration of iron, as in the carburisation of this metal, there are different degrees. Before forming under the influence of nitrogen, the scales which come off, and which, according to my experiments, contain 9.5 per cent. of nitrogen, the general properties of the metal undergo profound modifications; while preserving a

¹ CHEMICAL NEWS, Vol. III. p. 276.

certain malleability, it becomes white and fibrous. The iron in this state is still metallic, but, nevertheless, is strongly nitrogenised. It is this nitrogenised iron which I submit to the Academy, and which has undergone the processes connected with steeling which I am about to describe.

Desiring to study the successive or simultaneous action of nitrogen and carbon on iron, I was first obliged to find out a simple and easily-graduated method of carburisation as certain as the process for the nitration of iron by ammonia.

The action of illuminating gas on iron possesses all these advantages. In fact, I have proved that if dry coal-gas is passed at red heat during two hours on iron, I obtain a very regular carburisation, and the metal is converted into a gray cast iron, graphitic, very malleable, and in every way comparable to the best cast iron produced by wood charcoal. I present to the Academy a specimen of cast iron formed under these circumstances.

By employing ammonia and coal-gas, I possess two easily regulated processes, which enable me to study, either isolatedly or simultaneously, the action of ammonia and carbon on iron.

The result of my experiments is, that when iron is submitted to the action of coal-gas I obtain cast iron only; but when the carburetted gas is made to react on previously nitrogenised iron, the characteristics of steel are evident in the metallic compound. A very remarkable fact here appears, which is, that the properties of the steel in some measure depend on the quantity of nitrogen first imparted to the iron. If the nitration has not been carried on a sufficient time, the coal-gas, in acting upon iron, produces a body intermediate between steel and cast iron; if, on the contrary, the metal has undergone a sufficient degree of nitration, the gas produces a steel of magnificent grain. The specimens which I presented to the Academy were formed in this manner. I have thus been enabled to realise M. Despretz's anticipations, and to determine what influence nitride of iron exercises on the phenomena of steeling.

When, instead of making nitrogen and carbon react successively on the metal, I have passed a mixture of ammonia and lighting-gas on red-hot iron, the metal has immediately changed to steel, which varies with the relative proportions of the two gases.

In the experiments here described I believe I have for the first time produced steel by means of the successive action of two gases on iron,—one, ammoniacal gas, furnishing nitrogen; the other, lighting gas, furnishing carbon; and what appears to me to make the steel thus obtained still more interesting is, that with it cementation is no longer effected with wood charcoal, but with coal-gas. I ask metallurgists whether these experiments, which, in a theoretical point of view, throw light on the phenomenon of cementation, will not one day be practically utilised. Would it not be a curious circumstance in the cementation of iron were wood charcoal superseded by the products of the distillation of coal?

These facts establish positively the important part nitrogen plays in the phenomena of steeling. It remained for me to ascertain whether nitrogen, evidently a cementing agent, remains in the metallic compound, or whether its only use is to present carbon to the iron in a state favourable to chemical combination.

To resolve this interesting question, I submitted steel, obtained with ammonia and lighting gas, to the influence of the agent which could prove the presence of nitrogen

in steel with the greatest nicety,—viz., pure and dry hydrogen.

By heating in hydrogen some steel prepared in my laboratory, I immediately detected the presence of nitrogen in this metallic compound; for during the whole of the experiment it disengaged considerable quantities of ammonia.

After thus re-finding nitrogen in steel obtained by the action of ammonia and lighting-gas on iron, it became interesting to submit ordinary steel to the same proofs, and to ascertain whether these metallic compounds are equally nitrogenised.

To this end I have operated on steels of various sorts, all in high commercial estimation. My experiments were made successively on Jackson's French, Huntsmann's English, and Krupp's German steel.

(To be continued.)

On Artificial Guano, by Dr. ABRAHAM GESNER, F.G.S.

GUANO, so valuable a fertiliser, is chiefly composed of the excrements of sea fowls. Frequently it contains feathers, bones of fishes, humus, &c. It is very variable in composition, a circumstance that has been ascribed to the different kinds of food upon which the birds subsisted. Some guanos contain upwards of 25 per cent. of uric acid, in others that acid is almost entirely absent, and it is the same in regard to other acids, salts and alkalies. Ammonia usually enters largely into the best qualities of this fertiliser, and the presence of its carbonate is known by its odour. The oxalate, urate, and phosphate of ammonia and magnesia are almost always present with the phosphates of soda and lime, the phosphates having been derived from the bones of the fish upon which the birds fed. In the supply of ammonia and of earthy and alkaline salts, guano is of the greatest value for plants cultivated for food. The food of the birds, from which the guano had been deposited has been certain fish that fed upon other fish, the food of which was marine plants, or animalculæ. The origin of this fertiliser is therefore found in marine plants and animals.

The writer has obtained a product analogous to the true guano, and one nearly, if not quite, equal in its value for fertilising purposes. Chemical and mechanical means have been applied to the marine *fuci* and fishes and fish offal until an artificial guano has been obtained. The sources of the alkaline carbonate, chloride of sodium and organic matter have been found in marine plants, the phosphates and carbonates of lime and ammonia in the bones of flesh and fishes, and after many experiments, carefully performed, they have been combined so as to form a cheap and portable manure. At Long Island, in the State of New York, *menhaden* are manufactured into manure: the oil, which is very offensive, being extracted from the fish and employed for common purposes.

Having visited a great number of the fishing establishments of the Provinces of New Brunswick, Nova Scotia, Newfoundland, and the islands and coasts of the Gulf of St. Lawrence and Labrador, the writer obtained a knowledge of the vast quantity of fish and flesh offal annually thrown into the sea, or otherwise lost to every useful purpose. The garbage thrown overboard yearly from vessels fishing on the banks of Newfoundland, if properly preserved and manufactured, with the annual growth of sea-weeds upon the shore, would fertilise the

entire cultivated surface of the Eastern States and British Provinces; still the amount of animal matter thus referred to is far less than that produced by the inshore fisheries.

To the foregoing may be added the enormous quantities of *mytili* and other shellfish growing upon the shore, and which are not less applicable for the manufacture of artificial guano than the offal of the finny tribes. At many places on the shores fish are met with in such abundance that they are employed by the fishermen to manure the small patches of ground some of them cultivate. At the principal fishing stations, the refuse garbage and bones alone would supply a manufactory, and with good management, and the use of kelp, the offal may be transported from place to place without inconvenience. Like the bones of terrestrial animals, the inorganic matter, or ash of the bones of fishes consists in the greater part of the phosphates of lime, or bone phosphate, with carbonate of lime, the fertilising properties of which are well understood. Few soils preserve their fertility for any length of time. Every crop removes from the earth certain elements, which it is the business of the farmer to restore, and for that purpose no manure is better adapted than guano, either natural or artificial.—*Scientific American*.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting.

WILLIAM ROBERT GROVE, Esq., M.A., Q.C., F.R.S.,
Vice-President, in the Chair.

On the Application of the Law of the Conservation of Force to Organic Nature, by Professor HELMHOLTZ, F.R.S.

THE most important progress in natural philosophy by which the present century is distinguished, has been the discovery of a general law which embraces and rules all the various branches of physics and chemistry. This law is of as much importance for the highest speculations on the nature of forces as for immediate and practical questions in the construction of machines. This law at present is commonly known by the name of "the principle of conservation of force." It might be better perhaps to call it, with Mr. Rankine, "the conservation of energy," because it does not relate to that which we call commonly *intensity* of force. It does not mean that the intensity of the natural force is constant; but it relates more to the whole amount of power which can be gained by any natural process, and by which a certain amount of work can be done. For example: if we apply this law to gravity, it does not mean, what is strictly and undoubtedly true, that the intensity of the gravity of any given body is the same as often as the body is brought back to the same distance from the centre of the earth. Or with regard to the other elementary forces of nature—for example, chemical force: when two chemical elements come together, so that they influence each other, either from a distance or by immediate contact, they will always exert the same force upon each other—the same force both in intensity and in its direction and in its quantity. This other law, indeed, is true; but it is not the same as the principle of conservation of force. We may express the meaning of the law of conservation of force by saying that every force of nature, when it effects any alteration, loses and exhausts its faculty to effect the same alteration a second time. But while, by every alteration in nature, that force which has been the cause of this alteration is exhausted, there is always

another force which gains as much power of producing new alterations in nature as the first has lost. Although, therefore, it is the nature of all inorganic forces to become exhausted by their own working, the power of the whole system in which these alterations take place is neither exhausted nor increased in quantity, but only changed in form. Some special examples will enable you better to understand this law than any general theories. We will begin with gravity; that most general force, which not only exerts its influence over the whole universe, but which at the same time gives the means of moving to a great number of our machines. Clocks and smaller machines, you know, are often set in motion by a weight. The same is really the case with water-mills. Water-mills are driven by falling water; and it is the gravity, the weight of the falling water, which moves the mill. Now you know that by water-mills, or by a falling weight, every machine can be put in motion; and that by these motive powers every sort of work can be done which can be done at all by any machine. You see, therefore, that the weight of a heavy body, either solid or fluid, which descends from a higher place to a lower place is a motive power, and can do every sort of mechanical work. Now if the weight has fallen down to the earth, then it has the same amount of gravity, the same intensity of gravity; but its power to move, its power to work, is exhausted. It must become again raised before it can work anew. In this sense, therefore, I say that the faculty of producing new work is exhausted—is lost; and this is true of every power of nature when this power has produced alteration. Hence, therefore, the faculty of producing work, of doing work, does not depend upon the intensity of gravity. The intensity of gravity may be the same, the weight may be in a higher position or in a lower position, but the power to work may be quite different. The power of a weight to work, or the amount of work which can be produced by a weight, is measured by the product of the height to which it is raised and the weight itself. Therefore our common measure is foot-pound; that is, the product of the number of feet and the number of pounds. Now, we can by the force of a falling weight raise another weight; as, for example, the falling water in a water-mill may raise the weight of a hammer. Therefore it can be shown that the work of the raised hammer, expressed in foot-pounds, that is, the weight of the hammer multiplied by the height expressed in feet to which it is raised,—that this amount of work cannot be greater than the product of the weight of water which is falling down, and the height from which it fell down. Now we have another form of motive power, of mechanical motive power; that is, velocity. The velocity of any body in this sense, if it is producing work, is called *vis viva*, or living force, of that body. You will find many examples of it. Take the ball of a gun. If it is shot off, and has a great velocity, it has an immense power of destroying; and if it has lost its velocity, it is quite a harmless thing. The great power it has depends only on its velocity. In the same sense, the velocity of the air, the velocity of the wind, is motive power; for it can drive windmills, and by the machinery of the windmills it can do every kind of mechanical work. Therefore, you see that also velocity in itself is a motive force.

Take a pendulum which swings to and fro. If the pendulum is raised to the side, the weight is raised up; it is a little higher than when it hangs straightly down, perpendicular. Now if you let it fall, and it comes to its position of equilibrium, it has gained a certain velocity. Therefore, at first, you had motive power in the form of a raised weight. If the pendulum comes again to the position of equilibrium, you have motive power in the form of *vis viva*, in the form of velocity, and then the pendulum goes again to the other side, and it ascends again till it loses its velocity; then again, *vis viva* or velocity is changed into elevation of the weight: so you see in every pendulum that the power of a raised weight can be changed into

velocity, and the velocity into the power of a raised weight. These two are equivalent.

Then take the elasticity of a bent spring. It can do work, it can move machines or watches. The cross-bow contains such springs. These springs of the watch and cross-bow are bent by the force of the human arm, and they become in that way reservoirs of mechanical power. The mechanical power which is communicated to them by the force of the human arm, afterwards is given out by a watch during the next day. It is spent by degrees to overpower the friction of the wheels. By the cross-bow, the power is spent suddenly. If the instrument is shot off, the whole amount of force which is communicated to the spring is then again communicated to the shaft, and gives it a great *vis viva*.

Now the elasticity of air can be a motive power in the same way as the elasticity of solid bodies; if air is compressed, it can move other bodies; let us take the air-gun; there the case is quite the same as with the cross-bow. The air is compressed by the force of the human arm; it becomes a reservoir of mechanical power; and if it is shot off, the power is communicated to the ball in the form of *vis viva*, and the ball has afterwards the same mechanical power as is communicated to the ball of a gun loaded with powder.

The elasticity of compressed gases is also the motive power of the mightiest of our engines, the steam-engine; but there the case is different. The machinery is moved by the force of the compressed vapours, but the vapours are not compressed by the force of the human arm, as in the case of the compressed air-gun. The compressed vapours are produced immediately in the interior of the boiler by the heat which is communicated to the boiler from the fuel.

You see, therefore, that in this case the heat comes in the place of the force of the human arm, so that we learn by this example, that heat is also a motive power. This part of the subject, the equivalence of heat as a motive power, with mechanical power, has been that branch of this subject which has excited the greatest interest, and has been the subject of deep research.

It may be considered as proved at present, that if heat produces mechanical power, that is, mechanical work, a certain amount of heat is always lost. On the other hand, heat can be also produced by mechanical power, namely, by friction and the concussion of unelastic bodies. You can bring a piece of iron into a high temperature, so that it becomes glowing and luminous, by only beating it continuously with a hammer. Now, if mechanical power is produced by heat, we always find that a certain amount of heat is lost; and this is proportional to the quantity of mechanical work produced by that heat. We measure mechanical work by foot-pounds, and the amount of heat we measure by the quantity of heat which is necessary to raise the temperature of one pound of water by one degree, taking the centigrade scale. The equivalent of heat has been determined by Mr. Joule, of Manchester. He found that one unit of heat, or that quantity of heat which is necessary for raising the temperature of a pound of water one degree centigrade, is equivalent to the mechanical work by which the same mass of water is raised to 423½ metres, or 1389 English feet. This is the mechanical equivalent of heat.

Hence, if we produce so much heat as is necessary for raising the temperature of one pound of water by one degree, then we must apply an amount of mechanical work equal to raising one pound of water 1389 English feet, and lose it for gaining again that heat.

By these considerations, it is proved, that heat cannot be a ponderable matter, but that it must be a motive power, because it is converted into motion or into mechanical power, and can be either produced by motion or mechanical power. Now, in the steam-engine we find

that heat is the origin of the motive power, but the heat is produced by burning fuel, and therefore the origin of the motive power is to be found in the fuel, that is, in the chemical forces of the fuel, and in the oxygen with which the fuel combines.

You see from this, that the chemical forces can produce mechanical work, and can be measured by the same units and by the same measures as any other mechanical force. We may consider the chemical forces as attractions, in this instance, as attraction of the carbon of the fuel for the oxygen of the air; and if this attraction unite the two bodies, it produces mechanical work just in the same way as the earth produces work, if it attract a heavy body. Now the conservation of force, of chemical force, is of great importance for our subject to-day, and it may be expressed in this way. If you have any quantity of chemical materials, and if you cause them to pass from one state into a second state, in any way, so that the amount of the materials at the beginning, and the amount of the materials at the end at this process be the same, then you will have always the same amount of work, of mechanical work or its equivalent, done during this process. Neither more nor less work can be done by the process. Commonly, no mechanical work in the common sense is done by chemical force, but usually it produces only heat; hence the amount of heat produced by any chemical process must be independent of the way in which that chemical process goes on. The way may be determined by the will of the experimenter as he likes.

We see, therefore, that the energy of every force in nature, can be measured by the same measure, by foot-pounds, and that the energy of the whole system of bodies which are not under the influence of any exterior body must be constant; that it cannot be lessened or increased by any change. Now the whole universe represents such a system of bodies endowed with different sorts of forces and of energy, and therefore we conclude from the facts I have brought before you, that the amount of working power, or the amount of energy in the whole system of the universe must remain the same, quite steady and unalterable whatever changes may go on in the universe. If we accept the hypothesis of Laplace, that in the first state the universe was formed by a chaos of nebulous matter, spread out through infinite space, then we must conclude, that at this time the only form of energy existing in this system was the attraction of gravitation, and it was therefore the same sort of energy as is possessed by a raised weight. Afterwards, astronomers suppose, this nebulous matter was conglomerated and aggregated to solid masses. Great quantities of this nebulous matter, possibly from a great distance, fell together, and thus their attraction, or the energy of their attraction, was destroyed, and hence heat must have been produced; and the facts we know at present are sufficient to enable us to calculate the amount of this heat, that is, of the whole heat which must have been produced during the whole process of conglomeration. This amount of heat is immensely great, so that it surpasses all our ideas and all the limits of our imagination. If we calculate this quantity of heat, and suppose that the sun contained at the same time the whole heat, and that the sun had the same specific heat as water, the sun would be heated to twenty-eight millions of degrees, that is, to a temperature surpassing all temperatures we know on earth; however, this temperature could not exist at any time in the sun, because the heat which was produced by the aggregation of the masses, must also be spent partially by radiation into space. I give only the result of these calculations in order that you may see from it what a great amount of heat could be produced in this way. The same process goes on also at present in the falling stars and meteors which come down to the earth from planetary spaces. Their velocity is destroyed by the friction of the air and by the concussion with the surface of the earth,

and we see how they become luminous, and if they are found on the earth, we find them hot.

The sun also at present is hotter than any heated body here on the earth. That is shown by the latest experiments made by Professors Kirchhoff and Bunsen, of Heidelberg, on the spectrum of the sun, by which it is proved, that in the atmosphere of the sun iron and other metals are contained as vapours which cannot be changed into vapours by any amount of heat on the earth.

Our earth contains a great amount of energy in the form of its interior heat. This part of its energy produces the volcanic phenomena; but it is without great influence upon the phenomena of the surface, because only a very small amount of this heat comes through. It can be calculated that the amount of heat which goes from the interior to the surface cannot raise the temperature of the surface any higher than the thirteenth part of a degree.

We have another power which produces motion on the surface of the earth. I mean the attraction of the sun and of the moon producing the tides.

All the other phenomena on the surface of the earth are produced by the radiations of the sun, by the sunbeams; and the greater part of those changes which occur on the surface of our earth, are caused by the heat of the sun. As the heat of the sun is distributed unequally over the surface, some parts of the atmosphere become heated more than other parts; the heated parts of the atmosphere rise up, and so winds and vapours are produced. They come down at first as clouds in the higher parts of the atmosphere, and then as rain upon the surface of the earth; they are collected as rivers, and go again down into the sea. So you see that all the meteorological phenomena of our earth are produced by the effect of the solar beams, by the heat of the sun.

The light of the sun is the cause of another series of phenomena, and the principal products of the light of the sun are plants, because plants can only grow with the help of the sun-light. It is only by the help of the sun-light, that they can produce the inflammable matter which is deposited in the bodies of plants, and which is extracted from the carbonic acid and the water contained in the atmosphere, and in the earth itself.

This may give you an idea of the sense and bearing of the general principle on which I purpose to speak. As many English philosophers have been occupied with working out the consequences of this most general and important principle for the theory of heat, for the energy of the solar system, for the construction of machines, you will hear these results better explained by your own countrymen; I shall abstain from entering further into this part of the subject. At the same time that Mr. Grove showed that every force of nature is capable of bringing into action every other force of nature, Mr. Joule, of Manchester, began to search for the value of the mechanical equivalent of heat, and to prove its constancy, principally guided by the more practical interests of engineering. The first exposition of the general principle was published in Germany by Mr. Mayer, of Heilbronn, in the year 1842. Mr. Mayer was a medical man, and much interested in the solution of physiological questions, and he found out the principle of the conservation of force guided by these physiological questions. At the same time also, I myself began to work on this subject. I published my researches a little later than Mr. Mayer, in 1845. Now, at first sight, it seems very remarkable and curious, that even physiologists should come to such a law. It appears more natural, that it should be detected by natural philosophers or engineers, as it was in England; but there is, indeed, a close connexion between both the fundamental questions of engineering and the fundamental questions of physiology with the conservation of force. For getting machines into motion, it is always necessary to have motive-power, either in water, fuel, or living animal matter. The constructors of machines, instruments, watches, within the

last century, who did not know the conservation of force, were induced to try if they could not keep a machine in motion without any expenditure for getting the motive power. Many of them worked for a long time very industriously to find out such a machine which would give perpetual motion, and produce any mechanical work which they liked. They called such a machine a perpetual mover. They thought they had an example of such a machine in the body of every animal. There, indeed, motive-power seemed to be produced every day without the help of any external mechanical force. They were not aware that eating could be connected with the production of mechanical power. Food, they believed, was wanting only to restore the little damages in the machine, or to keep off friction, like the fat which made the axles of wheels to run smoothly. Now, at first, by the mathematicians of the last century, the so-called principle of conservation of *vis viva* was detected, and it was shown that by the action of the purely mechanical powers, it was not possible to construct a perpetual mover; but it remained still doubtful if it would not be possible to do so by the interposition of heat, or electricity, or chemical force. At last, the general law of conservation of force was discovered, and stated, and established; and this law shows that also by the connection of mechanical powers with heat, with electricity, or with chemical force, no such machine can be constructed to give a perpetual motion, and to produce work from nothing.

We must consider the living bodies under the same point of view, and see how it stands with them. Now, if you compare the living body with a steam-engine, then you have the completest analogy. The living animals take in food that consists of inflammable substances, fat and the so-called hydrocarbons, as starch and sugar, and nitrogenous substances, as albumen, flesh, cheese, and so on. Living animals take in these inflammable substances and oxygen, the oxygen of the air, by respiration. Therefore, if you take, in the place of fat, starch, and sugar, coals or wood, and the oxygen of the air, you have the substances in the steam-engine. The living bodies give out carbonic acid and water; and then if we neglect very small quantities of more complicated matters which are too small to be reckoned here, they give up their nitrogen in the form of urea. Now, let us suppose that we take an animal on one day, and on any day afterwards; and let us suppose that this animal is of the same weight the first day and the second day, and that its body is composed quite in the same way on both days. During the time—the interval of time—between these two days the animal has taken in food and oxygen, and has given out carbonic acid, water and urea. Therefore, a certain quantity of inflammable substance, of nutriment, has combined with oxygen, and has produced nearly the same substances, the same combinations, which would be produced by burning the food in an open fire, at least, fat, sugar, starch, and so on; and those substances which contained no nitrogen would give us, quite in the same way, carbonic acid and water, if they are burnt in the open fire, as if they are burnt in the living body; only the oxidation in the living body goes on more slowly. The albuminous substances would give us the same substances, and also nitrogen, as if they were burnt in the fire. You may suppose, for making both cases equal, that the amount of urea which is produced in the body of the animal, may be changed without any very great development of heat, into carbonate of ammonia, and carbonate of ammonia may be burnt, and gives nitrogen, water, and carbonic acid. The amount of heat which would be produced by burning urea into carbonic acid and nitrogen, would be of no great value when compared with the great quantity of heat which is produced by burning the fat, the sugar, and the starch. Therefore we can change a certain amount of food into carbonic acid, water and nitrogen, either by burning the whole in the open fire, or by giving it to living animals as

food, and burning afterwards only the urea. In both cases we come to the same result.

Now, I have said that the conservation of force for chemical processes requires a fixed amount of mechanical work, or its equivalent, to be given out during this process; and the amount is exactly the same in whatever way the process may go on. And therefore we must conclude that by the animal as much work must be done, must be given out—the same equivalent of mechanical work—as by the chemical process of burning. Now let us remark that the mechanical work which is spent by an animal, and which is given to the external world, consists, firstly, in heat; and secondly, in real mechanical work. We have no other forms of work, or of equivalent of work, given out by living animals. If the animal is reposing, then the whole work must be given out in the form of heat; and therefore we must conclude that a reposing animal must produce as much heat as would be produced by burning its food. A small difference would remain for the urea; we must suppose that the urea produced by the animal is also burnt, and taken together with the heat immediately produced by the animal itself. Now we have experiments made upon this subject by the French philosophers Dulong and Desprez. They found that these two quantities of heat—the one emitted by burning, the other by the living animal—are nearly identical; at least, so far as could be established at that time, and with those previous researches which existed at that time. The heat which is produced by burning the materials of the food is not quite known even now. We want to have researches on the heat produced by the more complicated combinations which are used as food. Dulong and Desprez have calculated the heat according to the theoretical supposition of Lavoisier—which supposition is nearly right but not quite right—therefore there is a little doubt as to the amount of the heat, but experiments show that at least to the tenth part of that heat the quantities are really equal; and we may hope if we have better researches on the heat produced by burning the food, that these quantities will also be more equal than they were found to be by Dulong and Desprez.

Now, if the body be not reposing, but if muscular exertion take place, then also mechanical work is done. The mechanical work is very different, according to the different kinds of muscular exertion. If we walk only on a plane surface, we must overpower the resistance of friction and the resistance of the air; but these resistances are not so great that the work which we do by walking on a plane is of great amount. Our muscles can do work in very different ways. By the researches of Mr. Redtenbacher, the Director of the Polytechnic School of Carlsruhe, it is proved that the best method of getting the greatest amount of work from a human body is by the treadmill, that is, by going up a declivity. If we go up the declivity of a hill we raise the weight of our own body. In the treadmill the same work is done, only the mill goes always down, and the man on the mill remains in his place.

Now, we have researches on the amount of air which is taken in and of carbonic acid given out during such work in the treadmill, made by Dr. Edward Smith. He found that a most astonishing increase of respiration takes place during such work. Now you all know that if you go up a hill you are hindered in going too fast by the great frequency and the great difficulty of respiration. This, then, becomes far greater than by the greatest exertion of walking on a plane, and really the difficulty is produced by the great mechanical work which is done in the same time. Now, partly from the experiments of Dulong and Desprez, and partly from the experiments of Dr. Edward Smith, we can calculate that the human body, if it be in a reposing state, but not sleeping, consumes so much oxygen, and burns so much carbon and hydrogen, that during one hour so much heat is produced that the whole body, or a weight of water equal to the weight of the body, would be raised

in temperature one degree and two-tenths Centigrade (two degrees and two-tenths Fah.). Now Dr. Edward Smith found that, by going in the treadmill at such a rate, that, if he went up a hill at the same rate, he would have risen during one hour 1712 feet; that, during such a motion, he exhaled five times as much carbonic acid as in the quiet state, and ten times as much as in sleeping. Therefore, the amount of respiration was increased in a most remarkable way. If we now calculate these numbers we find that the quantity of heat which is produced during one hour of repose is one degree and two-tenths Centigrade, and that these are nearly equivalent to rising 1712 feet; so that, therefore, the amount of mechanical work done in a treadmill, or done in ascending a hill at a good rate, is equivalent to the whole amount of heat which is produced in a quiescent state. The whole amount of the decomposition in the living body is five times as great as in a reposing and wakeful state. Of these five quantities, one quantity is spent for mechanical work, and four-fifths remain in the form of heat. Always in ascending a hill, or in doing great mechanical work, you become hot, and the production of heat is extremely great, as you well know, without making particular experiments. Hence you see how much the decomposition in the body is increased by doing really mechanical work.

Now these measurements give us another analogy. We see that in ascending a mountain we produce heat and mechanical work, and that the fifth part of the equivalent of the work which is produced by the chemical process is really gained as mechanical work. Now, if we take our steam-engine, or a hot-air engine, or any other engine which is driven by heat in such a way that one body is heated and expands, and by the expansion other bodies are moved,—I say, if we take any thermo-dynamic engine, we find that the greatest amount of mechanical work which can be gained by chemical decomposition or chemical combination is only an eighth part of the equivalent of the chemical force, and seven-eighths of the whole are lost in the form of heat; and this amount of mechanical work can only be gained if we have the greatest difference of temperature which can be produced in such a machine. In the living body we have no great difference of temperature; and in the living body the amount of mechanical work which could be gained if the living body were a thermo-dynamic engine, like the steam-engine or the hot-air engine, would be much smaller than one-eighth. Really, we find from the great amount of work done, that the human body is in this way a better machine than the steam-engine, only its fuel is more expensive than the fuel of steam-engines.

There is another machine which changes chemical force into mechanical power; that is, the magneto-electric machine. By these magneto-electric machines a greater amount of electrical power can be changed into mechanical work than in our artificial thermo-dynamic machines. We produce an electric current by dissolving zinc in sulphuric acid, and liberating another oxidizable matter. Generally it is only the difference of the attraction of zinc for oxygen compared with the attraction of copper or nitrous acid for oxygen. In the human body we burn substances which contain carbon and hydrogen, and therefore the whole amount of attraction of carbon and hydrogen for oxygen is put into action to move the machine; and in this way the power of the living body is greater and more advantageous than the power of the magneto-electric machine.

Let us now consider what consequences must be drawn when we find that the laws of animal life agree with the law of the conservation of force, at least as far as we can judge at present regarding this subject. As yet we cannot prove that the work produced by living bodies is an exact equivalent of the chemical forces which have been set into action. It is not yet possible to determine the exact value of either of these quantities so accurately as will be done ultimately; but we may hope that at no distant time it

may be possible to determine this with greater accuracy. There is no difficulty opposed to this task. Even at present I think we may consider it as extremely probable that the law of the conservation of force holds good for living bodies.

Now we may ask, what follows from this fact as regards the nature of the forces which act in the living body?

The majority of the physiologists in the last century, and in the beginning of this century, were of opinion that the processes in living bodies were determined by one principal agent which they chose to call the "vital principle." The physical forces in the living body they supposed could be suspended or again set free at any moment, by the influence of the vital principle; and that by this means this agent could produce changes in the interior of the body, so that the health of the body would be thereby preserved or restored.

Now the conservation of force can exist only in those systems in which the forces in action (like all forces of inorganic nature) have always the same intensity and direction if the circumstances under which they act are the same. If it were possible to deprive any body of its gravity, and afterwards to restore its gravity, then, indeed, we should have the perpetual motion. Let the weight come down as long as it is heavy; let it rise if its gravity is lost; then you have produced mechanical work from nothing. Therefore this opinion that the chemical or mechanical power of the elements can be suspended or changed, or removed in the interior of the living body, must be given up if there is complete conservation of force.

There may be other agents acting in the living body, than those agents which act in the inorganic world; but those forces, as far as they cause chemical and mechanical influences in the body, must be quite of the same character as inorganic forces, in this at least, that their effects must be ruled by necessity, and must be always the same, when acting in the same conditions, and that there cannot exist any arbitrary choice in the direction of their actions.

This is that fundamental principle of physiology which I mentioned in the beginning of this discourse.

Still at the beginning of this century physiologists believed that it was the vital principle which caused the processes of life, and that it detracted from the dignity and nature of life, if any body expressed his belief that the blood was driven through the vessels by the mechanical action of the heart, or that respiration took place according to the common laws of the diffusion of gases.

The present generation, on the contrary, is hard at work to find out the real causes of the processes which go on in the living body. They do not suppose that there is any other difference between the chemical and mechanical actions in the living body, and out of it, than can be explained by the more complicated circumstances and conditions under which these actions take place; and we have seen that the law of the conservation of force legitimates this supposition. This law, moreover, shows the way in which this fundamental question, which has excited so many theoretical speculations, can be really and completely solved by experiment.

CORRESPONDENCE.

Tasmanian Wheat.

To the Editor of the CHEMICAL NEWS.

SIR,—Mr. Wentworth L. Scott is presuming too much upon the patience of your readers. Upon the strength of a certain facility in microscopic observation, and an unfortunate *cacòthes scribendi*, he considers it his vocation to favour the public with his opinion upon all and every subject connected with food and its adulteration. . . .

This being the case, it is not surprising that a long letter from this authority upon the products and resources of Tasmania, should appear in the *Society of Arts, Journal* appended to the interesting paper by Dr. Milligan. I am, however, astonished that you, Sir, should have occupied your valuable space with such nonsense. What is the meaning of the analysis of wheat which he there gives? The tabular form in which it is printed, together with the "100'000" placed at the foot of the column of figures, evidently show that they are per-centages, the united amounts making up 100 parts. If Mr. Scott had given us the figures as he found them, and left it to others to give their proper value to them, it would not have much mattered; but when an analyst prints in one column, water, 13'4, ash, 2'2, and then gives the constituents of that ash; next nitrogenous matter, 15'7, and nitrogen 2'5; then carbonous matter, 68'5, fat, 1'4, and starch, &c. 67'1. When all these and many more are placed in order, one under the other, and then nicely finished with a rule, and "100'000" beneath it as the sum of the whole, it is evident that Mr. Scott's arithmetic is at fault, and more than probable that his chemistry is not less so.—I am &c. F.C.S.

Burlington House, May 21, 1861.

[Our correspondent will see that we have omitted several paragraphs from his letter, which, being simply abusive, would be of interest to but few of our readers. Personalities do not strengthen a bad cause whilst they materially injure a good one.—ED. C. N.]

Composition of Oxen, Sheep, and Pigs.

To the Editor of the CHEMICAL NEWS.

SIR,—In your notice of the paper by Mr. Lawes and myself "On the Composition of Oxen, Sheep, and Pigs, and of their Increase whilst Fattening," which appears in the *CHEMICAL NEWS* of May 11, you remark on the fact, that, in the paper in question, "no particulars are afforded explanatory of the analytical methods employed, and of the general system of operating."

The fact is, that the paper you have noticed, is an abstract of numerous other papers which are referred to, and all the particulars in question, will be found in the *Philosophical Transactions*, Part xi., 1859.—I am, &c.

J. H. GILBERT.

Harpenden, St. Albans, May 15.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Reaction of Hyposulphite of Soda on Sulphate of Lime.—Diehl found (*Jahresbericht d. physikal. Verein zu Frankfurt*, 1859, s. 83) that sulphate of lime was dissolved when treated with a super-saturated solution of hyposulphite of soda. This solution depends, says the author, on the great disposition of the hyposulphites to form easily soluble double salts. When alcohol is added to the solution, the double salt formed separates out in the form of a heavy, oily fluid congealing into white crystalline needles, and mixed with the undissolved hyposulphite of soda. This behaviour of the hyposulphite of soda to sulphate of lime, offers a simple means of separating lime from the other insoluble alkaline earths in the form of sulphate. To separate sulphate of lime from sulphate of baryta, the mixture of these salts must be treated with a concentrated solution of the hyposulphite and gently warmed. To be quite certain that all the lime is extracted, the treatment with the hyposulphite should be repeated once or twice until the filtered solution gives no sign of lime when tested with oxalate of ammonia.

The undissolved sulphate of baryta may then be collected on a filter, washed with hot water, ignited and weighed; and the lime in the filtrate precipitated by oxalate of ammonia, and estimated in the usual way.

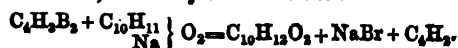
Action of Hyposulphite of Soda on Ferro- and Ferricyanides.—Diehl (*Ibid.*, s. 85) has further studied the action of hyposulphite of soda on ferrocyanogen compounds. The ferro- and ferricyanides of copper, lead, silver, and mercury, he finds to be soluble in a solution of the hyposulphite; while the corresponding compounds of zinc, manganese, cobalt, nickel, cadmium, and tin, are insoluble. This reaction, the author says, may be found useful in the qualitative separation of the above compounds.

Action of Phosphuretted Hydrogen on Salts of the Alkalies.—Winkler has made this the subject of a long investigation (*Chemisches Centralblatt.*, No. xviii. April 24, p. 285). In his experiments the author passed the non-spontaneously inflammable gas through dilute solutions of different salts, the object apparently being to see in what cases phosphoric acid was produced. With the chlorides of potassium, sodium, and ammonium, phosphoric acid was formed; but in no other cases was any obtained. The solutions were tested with molybdate of ammonia and hydrochloric acid; but in doubtful cases the author resorted to the other tests for phosphoric acid.

A solution of carbonate of soda absorbed the phosphuretted hydrogen, and evolved carbonic acid. At first the solution became cloudy, but the cloudiness soon disappeared. On the addition of the molybdate and hydrochloric acid, a bulky white precipitate was produced, which however redissolved when the mixture was shaken, and the fluid became of a bluish colour, which changed to a deep blue on being gently heated. The blue solution evaporated to dryness in a porcelain dish left a blue stain. Further investigation showed that the blue compound resulted from the partial deoxidation of molybdic acid. This led the author to try other means of detecting phosphoric acid, but he did not succeed in detecting any trace of that body. The experiment was repeated several times with the same results.

II. ORGANIC CHEMISTRY.

Acetylene.—New methods of preparing this gas (first discovered by Ed. Davy, and described by Berthelot, *CHEMICAL NEWS*, Vol. i., p. 287), by the decomposition of bromides of ethylene, have been discovered by Sawitsch at Paris and Miasnikoff at Kasan. (*Bulletin de la Société Chimique de Paris*, No. i., pp. 7—12.) The former passes the vapour of dibromated ethylene into an ammoniacal solution of cuprous oxide. A deep red-coloured powder falls, which, when dry, is very explosive. When heated with acids (sulphuric or hydrochloric) this fulminating powder gives off a gas which Sawitsch proved to be acetylene, C_2H_2 . Miasnikoff passes the vapour of bromated ethylene, C_2H_3Br (the bromide of acetyl of Gmelin, *Handbook*, Vol. ix., p. 187) once or twice through a hot and strong alcoholic solution of caustic potash and then into an ammoniacal solution of nitrate of silver. In this way a grey-coloured explosive powder is produced, which, when treated with dilute hydrochloric acid, also evolves acetylene. In both cases the formation of the acetylene is probably due to the splitting up of the bromide of acetyl into hydrobromic acid and acetylene. $C_2H_3Br = C_2H_2 + HBr$. Sawitsch also found that when bromide of acetyl is acted on in a closed vessel with amylate of sodium, bromide of sodium, amyl alcohol, and acetylene are formed.



Both authors are engaged in following up their researches with others on the bromide of propylene in the hope of obtaining the homologue of acetylene, C_3H_4 . M. Morkow-

nikoff at Kasan has, indeed, succeeded in forming from that body a detonating silver compound, which, when treated with dilute hydrochloric acid yielded a gas which appeared to be the body sought for, and which he calls *allylene*. It will be a happy day for chemists when the nomenclature of their science becomes fixed.

Ethyl-lactic Acid.—Boutlerow found that when iodoform was acted on by ethylate of soda, besides acrylic acid, another acid, $C_{10}H_{10}O_6$, was formed, to which he gave the name valero-lactic acid. This acid he has since made out (*Bulletin de Société Chimique de Paris*, No. i. p. 9) to be identical with Wurtz's ethyl-lactic acid, obtained by treating the diethyl ether of lactic acid with an alkali. Having shown that this acid is easily attacked by hydriodic acid, with the formation of iodide of ethyl, the author remarks it is very probable that the action of hydriodic acid and its congeners offers a means of eliminating the different groups which replace typical but not basic hydrogen. The splitting up of various organic compounds (e.g. glucosides) under the influence of dilute acids, belongs to the same class of phenomena. The new acids obtained by Heints, the author thinks, may be split up by hydriodic acid into glycolic acid, and the iodides of their respective alcoholic radicals. M. Konat, Zaitzetz has already proved that the action of hydriodic on anisic acid (which Kolbe and the author regard as methyl-oxybenzoic or methyl-salicylic acid) iodide of methyl is formed; but his experiments are not yet completed.

Coloured Derivatives of Naphthalene.—M. Roussin has succeeded in preparing some colours from naphthalene, which it would appear may be used as dyes. Unfortunately the colours are the same as the aniline derivatives, but they may perhaps possess the advantage of greater stability. The first step, of course, is the preparation of nitro-naphthalene. This M. Roussin makes by heating on a water-bath a mixture of 2lbs. of naphthalene and 10lbs. of commercial nitric. The naphthalene melts, and remains swimming on the surface, so the flask requires to be shaken occasionally. Some vapours escape, and in time an oily layer of nitro-naphthalene forms at the bottom. The whole is then decanted, and the nitro-naphthalene, which immediately congeals, is well washed to remove the excess of acid. It is further purified by melting it once or twice, and pressing it between folds of blotting paper after it has cooled. Melted nitro-naphthalene runs through filtering-paper like water. The next step towards the colour is the preparation of naphthylamine. Six parts of hydrochloric acid and one of nitro-naphthalene are mixed in a flask, and enough granulated tin is added to reach the surface of the mixture. It is then heated on a water-bath, the flask being shaken occasionally. In a few minutes a strong re-action takes place, the nitro-naphthalene disappears, and the mixture becomes limpid. It is then decanted into an earthen pan containing hydrochloric acid, whereupon the whole becomes nearly solid by the crystallization of the hydrochlorate of naphthylamine. The crystalline mass is drained on a cloth, and then strongly pressed. It is further purified by solution in boiling water, adding sulphide ammonium to precipitate any tin, and filtering while hot. The hydrochlorate crystallizes on cooling.

On mixing a colourless solution of this hydrochlorate and another of nitrate of potash, a pomegranate-red precipitate falls, which is completely insoluble in water. The application of this colour to dyeing is very simple. The skeins of silk are first dipped in a solution of the hydrochlorate of naphthalene, then wrung, afterwards immersed in a bath of nitrate of potash, and then rinsed as usual. Shades of colour, from a faint blush to a deep maroon, are procured according to the strength of the solutions. The colour is unchangeable in the light, and is not attacked by any bleaching agent. Its permanency, indeed, according to the author, is marvellous.

MISCELLANEOUS.

Royal Institution.—The following is the arrangement of Lectures for the ensuing week:—Tuesday, May 28, at 3 o'clock, Mr. John Hullah, "On the History of Modern Music." Thursday, May 30, 3 o'clock, Mr. W. Pengelly, "On the Devonian Age of the World." Friday, May 31, 8 o'clock, Dr. W. V. Waller, F.R.S., "On the Nutrition and Reparation of Nerves." Saturday, June 1, 3 o'clock, Professor Max Müller, "On the Science of Language."

Rock Oil as Fuel for Steam Engines.—An application of the Rock-oil of Pennsylvania for generating steam for motive power under steam engine boilers is exciting attention in the oil region. The following is a description of the apparatus used:—A series of iron pipes are laid in the fire arch of the boiler, which pipes are perforated in their upper surface with minute holes; the oil is supplied to those pipes by means of a force pump, aided by an air receiver, to preserve a constant pressure. A spray, so to speak, of oil is thus made to fill the space usually filled by the flame of wood or coal used to raise steam; this, once ignited, fills the fire arch and flues of the boiler, and maintains the desirable amount of heat in the boiler. If this fuel is not found to be too expensive, it will prove a good thing for the use of steamers on sea voyages. Its practical use has been proved, and it remains for chemists and others to test it on ships, &c., in a large way. There can be but little doubt that this oil will be found cheaper than coal for gas-making for lighting dwellings, streets, &c.; its price, under the influence of the vast supply raised, will soon come down to a matter of 15 or 20 cents per gallon.

A Fresh Water Spring at Sea.—Mr. W. A. Booth, the coast pilot of the revenue cutter *Harriet Lane*, reports the discovery of a boiling fresh water spring at sea, off the coast of Florida. He says the spring is situated twelve miles, north by east from St. Augustine, Fla., and eight miles off shore. It boils up with great force, and can be descried at a distance of two miles. When first seen it has the appearance of a breaker, and is generally avoided; but there is no danger in the vicinity, as there are five fathoms of water between it and the shore. Ten fathoms of water are found to the seaward, but no bottom can be reached with the deep sea lead and thirty fathoms of line at the spring itself. The water in the spring is fresh, and is by no means unpalatable. One peculiarity about this phenomenon is, that when the St. John's river is high, it boils up from six to eight feet above the level of the sea, and presents rather a forbidding appearance. This spring has doubtless deceived hundreds, who have hastily put about from, as they thought, imminent danger, and reported seeing a "rock with water breaking over it." The *Harriet Lane* has passed through it several times, and water has been drawn from it by a bucket thrown over the side, and when drank, no unpleasant taste or smell has been found.

Water as a Fuel in Making Iron.—The use of water as a fuel is now attracting a great deal of attention throughout the world. We translate the following from the *Revue Universelle*:—

"The vapour of water has already been utilised in metallurgy as an agent of oxidation in the roasting of certain minerals, particularly to facilitate the separation of the compounds of antimony and arsenic in metallic sulphurets. For several years, attempts have been made to employ the calorific power of the hydrogen contained in water; and it is in the same line of invention that Messrs. Maire and Vallee have sought to utilize water as a combustible in industrial furnaces, and particularly in metallurgical operations. Water, fed in a regulated and intermittent manner into a hot fire, is decomposed into oxygen and

hydrogen. The combustion of the latter, in presence of the atmospheric air (the oxygen of the water being employed in burning the carbon), produces a considerable heat in addition to that of the principal combustible. There results then a considerable augmentation of calorific without any addition of combustible, and, consequently, a more rapid fusion of metals and minerals, and an economy of fuel which the authors of the process state varies from 40 to 50 per cent. Experiments and calculations have demonstrated that the heat absorbed by the decomposition of water is less than that furnished by the combustion of the gaseous products of the water decomposed.

"The following is the manner in which Messrs. Maire and Vallee propose to apply their method to metallurgy. For a blast furnace, the water is led from a reservoir under a pressure of 1½ atmospheres in a pipe from A, the to A' the of an inch in diameter, which enters the tuyere at right angles at a distance from its end varying from 10 to 40 inches, and thus the water is blown, in the form of spray, into the centre of the fire."

We have strong doubts of the truth of the above statement which we have italicised, notwithstanding the high authority on which it is made.

ANSWERS TO CORRESPONDENTS.

"* In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

"* All Editorial Communications are to be addressed to Mr. CROOKES, and Advertisements and Business Communications to the PUBLISHERS, GARRYS, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

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X. C. will receive all the information which he desires by applying to the Secretary.

S.—No silver compound will do. Try a solution of chloride of gold.

W. P.—The best way to remove starch from Printed Liama shawls when being re-dyed, is to steep the goods for two or three days in water at 70° F. to rot it away. Re-agents will injure the wool.

Blue Ink—S. Andrews.—Three drachms of Chinese Blue (ferrocyanide of iron) are to be ground up with one drachm of binoxalate of potash, and seven ounces of water. Usually a little gum (one drachm) is added to these quantities.

J. O.—The process is by no means exact, but it was given as a rough method of obtaining the approximate value without difficulty. Benzol boils at 176° F., whilst the accompanying hydrocarbons boil at temperatures above 212°. A mixture will, therefore, boil at a lower temperature the more benzol it contains. It would probably begin to boil at about 180°, and the boiling point would gradually rise to 200 or 300°; the benzol coming over at the commencement. For this reason, commercial benzoils are generally tested by trying how much comes over below 212°.

J. Allen.—The matter shall meet with immediate attention.

A. H. B.—Dissolving and fusing a given quantity will give the insoluble portion and moisture. For the sulphates and chlorides, estimate the acids in the usual way, and then, having ascertained what other bases are present, besides potash, unite the acids and bases together by calculation.

Kyfos writes:—"Mr. Challis, of Cambridge, having established, by the aid of the hydrodynamical equations, the fact that the phenomena of colours arise from modifications of ray pressures, how does the presence of a metal in the gas flames of Kirchhoff and Runnen's spectrum apparatus stifle the colours of the solar spectra, except the characteristic stripe or stripes of those metals present in the flames?"

THE CHEMICAL NEWS.

VOL. III. No. 78.—June 1, 1861.

MINERAL ADULTERATIONS IN PAPER.

PAPER made from the best descriptions of linen or cotton rags, and well sized with gelatine and a little alum, has always been distinguished by its superior strength and power of resisting the wear and tear of rough usage. The qualities usually employed for the manufacture of bank-notes, of cartridge envelopes, and the so-called "loan-paper," are of this class; they all bear some resemblance to parchment, and although somewhat transparent, are held in high estimation by reason of their considerable power of endurance. Of the unsized varieties, the linen paper made by Munktell, and commonly known as "Swedish filtering," is remarkable for purity; and, on account of its consisting wholly of pure vegetable fibre, and leaving consequently no more than an inappreciable amount of ash on burning, this make of paper has met with universal adoption by chemists, and is generally employed for filtering purposes in quantitative analysis.

The practice of introducing small quantities of mineral matter for the purpose of imparting a superior degree of hardness and opacity to writing paper, particularly cream-laid, is generally recognised, and may not be considered objectionable so long as the proportion employed is limited to the amount actually necessary to effect this end. Plaster of Paris and China clay are the materials usually selected, and, when used sparingly in paper-making, may be accounted necessary ingredients equally with the alum employed in the size. The advance in the price of rags during late years has, however, offered temptation, particularly to foreign manufacturers, to increase the proportion of the cheaper mineral constituents in their paper, and the amount has become so excessive that they must be regarded as adulterations.

Letters have been addressed to the *Times* during the past week, pointing out instances of this growing practice. Professor Penny, of Glasgow, states that he has found in several foreign samples quantities of fine white clay, amounting to upwards of 30 per cent., or nearly one-third of the weight of the paper. He further remarks, "that there is nothing in the general appearance of this clayed-paper to indicate the presence of so large a proportion of earthy matter; but when a sheet of it is slowly burned it leaves a grey, friable residue, having the form and apparently the texture of the original paper, and consisting almost entirely of clay." Several samples of British paper were found to contain notable quantities of sulphate of lime (gypsum, plaster of Paris). Cream-laid paper of foreign manufacture contained, according to the same author, from 29 to 33½ per cent. of clay, and printers' demy from 26 to nearly 31 per cent.

Mr. Francis Sutton, of Norwich, fully endorses the statements of Professor Penny, and alludes further to a serious evil likely to arise from the presence of an excessive amount of China clay in printing paper, that, inasmuch as this mineral kaolin consists of fine white

clay, associated with a considerable proportion of gritty silicious particles, these latter would lead to a more rapid destruction of the type employed for printing on such paper,—a result further to be anticipated on account of the inferior character of the foreign sizing.

The use of starch, so frequently employed in the paper mills of France and Germany, may very possibly offer facilities for the introduction of larger quantities of mineral matter in the pulp. Whilst these practices are allowed to continue so long may we expect to hear complaints of brittleness of structure and bad wearing qualities of the paper.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Artificial Production of Alizarine,
by M. Z. ROUSSIN.

I HAVE already pointed out binitronaphthaline as a fruitful source of coloured products; the action of alkaline reducers, such as sulphides and protosalts of tin, dissolved in caustic potash, cyanide of potassium, &c., yield with this substance very rich red, violet, and blue derivatives. When the reducing agents are acid, as, for instance, when a mixture of zinc and weak sulphuric acid is employed, or iron filings and acetic acid, minute grains of tin and hydrochloric acid, &c., the binitronaphthaline undergoes no alteration. Inquiring into the cause of this unexpected resistance has led me to study more carefully than heretofore the properties of binitronaphthaline. Among these most deserving attention, the following is beyond all others remarkable.

By making concentrated sulphuric acid react on crystallised binitronaphthaline no reaction is produced. When the temperature of the liquid is raised to 250° C, binitronaphthaline dissolves completely, as soon as the liquid becomes amber colour. Only after long boiling will concentrated sulphuric acid begin to react on this substance. Binitronaphthaline is precipitated with its primitive whiteness when these acid solutions are weakened with water. This remarkable stability of an organic molecule in presence of so energetic a reagent as hot concentrated sulphuric acid, calls to mind an analogous reaction. If powdered madder-root is treated by concentrated sulphuric acid at 100° C., all its organic materials are carbonised. Only one among them can resist this violent treatment, and that is the colouring matter of the root itself,—namely alizarine. Now, all chemists know that the formula of the latter substance, as well as its principal properties, denote that probably it belongs to the naphthalic series.

The formula of alizarine is generally represented by $C_{20}H_8O_4$; that of binitronaphthaline by $C_{20}H_8(NO_4)_2$. An opportune reducing agent, which by carrying off two molecules of oxygen and making nitrogen pass to the state of ammonia would probably convert binitronaph-

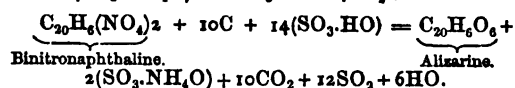
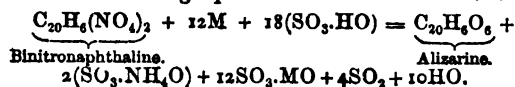
thaline into alizarine. Experience has confirmed this idea. By the following process artificial alizarine may be prepared:—

Introduce a mixture of binitronaphthaline and concentrated sulphuric acid into a large porcelain capsule, heated in a sand-bath. When the temperature is raised, binitronaphthaline dissolves completely in sulphuric acid. When the mixture reaches 200° C. throw in minute grains of zinc, and in a few instants sulphurous acid is disengaged. The operation takes about half-an-hour. Then if a drop of the acid mixture is made to fall into cold water it develops a beautiful red-violet colour, owing to the formation of alizarine. Sometimes the reaction becomes energetic if a large mass is operated upon, if there is too much zinc, and if the temperature is not carefully attended to. In such a case the sulphuric acid boils rapidly; abundant white vapours are disengaged with extraordinary noise and violence. It must be added that the latter inconvenience is easily avoided by adding only small quantities of granulated zinc, and by watching the temperature. When this accident does happen, the proportion of alizarine is greatly diminished, but still a considerable quantity remains in the residue.

The reaction over, dilute the liquid with eight or ten times its volume of water, and then boil it. In a few minutes throw the liquid on a filter. Cooling causes it to deposit alizarine, in the form of a red jelly, sometimes adhering to the vessels, sometimes suspended in the liquid. In either case this jelly appears, under the microscope, to be composed of a mass of very distinct pointed filiform crystals. The mother-waters are coloured dark red, and hold in solution large quantities of alizarine. They can be immediately used for dyeing after dilution with water and proper saturation. They contain large quantities of sulphate of ammonia. Some undissolved alizarine remains on the filter, which is easily carried off by caustic alkalis or carbonates, and precipitated anew by acids.

In the preceding reaction, zinc can be replaced by various substances; for example, tin, iron, mercury, sulphur, charcoal, &c., &c.,—in a word, by all bodies, simple or compound, organic or inorganic, which react upon and reduce sulphuric acid at a high temperature.

The two following equations describe the reaction:—



In the first equation, it is a metal which reacts on sulphuric acid. In the second, it is the carbon itself.

The alizarine obtained by the preceding process possesses all the characteristics and reactions of ordinary alizarine. Almost insoluble in water, it dissolves in alcohol and ether. It volatilises between 215° and 240°, with a yellow vapour, yielding dark red, crystalline needles; but the colour of these crystals is rather variable. Hydrochloric and concentrated sulphuric acid will not attack it. It dissolves in caustic and carbonated alkalis, with a beautiful deep blue-purple colour; acids precipitate this solution in orange-red flakes. Like madder alizarine, it furnishes most beautifully coloured lakes. Artificial alizarine dyes like natural alizarine, and imparts the same pure tints.

The elementary analysis of madder alizarine has hitherto given variable results. Doubtless, the reason is

the difficulty of freeing the natural product from impurities. The elementary analysis (which I shall make in a few days) of artificial alizarine will definitively establish the formula of this important colouring matter.

M. Dumas, while presenting the preceding paper in the name of its author, remarked, that the identity of alizarine with M. Roussin's artificial product was not thoroughly established. The elementary analysis of the latter has not yet been made. It has not as yet been possible to test certain applications to dyeing and printing processes characteristic of alizarine. It is to be hoped that the Commission nominated will decide the question promptly, the interests at stake being considerable.—*Comptes-Rendus*.

On the Detection of Bisulphide of Carbon in Coal Gas, by Dr. E. HERZOG.

A SOLUTION is prepared by saturating absolute alcohol with ammonia gas. Then a concentrated aqueous solution of acetate of lead is made, and, to insure saturation, a small portion of the solid salt is left in contact. Both these fluids must be preserved in well-stoppered bottles.

The gas to be tested may be conveniently delivered from a length of vulcanised india-rubber tubing, to the end of which is adapted a narrow glass tube, about five or six inches long. Five drops of the sugar of lead solution are then mixed in a test-tube, with about sixty or seventy drops of the alcoholic ammonia. No precipitate will be formed providing the latter solution has not been allowed to absorb any carbonic acid.

The glass tube delivering the supply of coal gas is now immersed in the mixed solution to a depth just sufficient to allow the gas to be forced out by the existing pressure, and to escape in small bubbles. In the event of bisulphide of carbon being present, the liquid becomes gradually coloured, and soon afterwards a yellowish-red precipitate is formed, which, by longer action, assumes a brownish colour. If carbonic acid existed originally in the gas, then a white precipitate is thrown down, which imparts to the yellow-red a somewhat lighter colour.

As a confirmatory experiment, the gas may be first passed through the alcohol ammonia fluid alone and the lead solution subsequently added, when an orange coloured precipitate, appearing either immediately or very shortly afterwards, will be formed if bisulphide of carbon is present. In order to meet the objection that sulphuretted hydrogen may perhaps have occasioned this reaction, let some of the gas be first passed through a small quantity of the simple lead solution. The smallest trace of sulphuretted hydrogen causes a blackening of the liquid, whereas bisulphide of carbon does not alter it in the slightest degree.

It should be mentioned that if the yellow-red precipitate be allowed to remain under the fluid, it gradually changes colour, and becomes white after the lapse of about twenty-four hours. If, however, the precipitate be filtered immediately, slightly washed, and dried, it becomes a dark brown.

With regard to the explanation of the chemical reactions which occur in this process, the interesting observations made by MM. Zeise and Debus may be quoted as proving that, by the action of sulphide of carbon on ammonia, according to the concentration and temperature of the fluids and the proportion borne by the ammonia to the sulphide, so will the relative amounts of the products of decomposition vary. In concentrated solutions, and when ammonia is in excess, sulphocar-

bonate of ammonium and sulphocyanide of ammonium are formed; in dilute fluids and when sulphide of carbon is in excess, xanthionate of ammonia. Therefore, by this experiment one or other product will preponderate according to circumstances, dependent upon the larger or smaller quantity of sulphide of carbon contained in the gas. In any case, compounds of lead are formed corresponding to the ammonia compounds, which possess at first an orange-red and afterwards a golden-yellow colour.

Notwithstanding the complicated nature of the chemical reactions involved in the testing of gas by this process, the author recommends its adoption on account of the practical simplicity which, in his hands, attended the working of a great number of comparative experiments.—*Chemischen Centralblatt*.

TECHNICAL CHEMISTRY.

On the Composition of Cast Iron and Steel, by M. E. FREMY.

(Continued from page 321.)

These bodies were reduced to very fine filings, and this metallic powder freed from all foreign matter was submitted at a red heat to the action of dry hydrogen. In these three trials the filings during the whole of the experiment disengaged considerable quantities of ammonia. This experiment leaves no doubt on the subject, and proves that nitrogen, contrary to the hitherto received opinions, is a constituent of steel. Steel is not then a simple carburet, but a nitro-carburet of iron.

If I be not deceived as to the bearing of my inquiries, it appears to me that they ought to have a certain influence on the metallurgical operations relating to the manufacture of steel. Thus in the cementation of iron, all the conditions appear to be realised which give to the metal not only carbon but also nitrogen. It is probable that the different qualities of steel depend on the duration of the cementation, and also on the relative proportions of the two elements which combine with iron. In the preparation of steel by puddling, it is equally important to determine what are the varieties of iron most likely to yield the proportion of nitrogen required for the constitution of steel; or those which, not containing sufficient, require to have it imparted while being converted into steel.

I have spoken of a steel which has for bases carbon and nitrogen; but this compound is not the only iron alloy, the composition and properties of which are of interest to metallurgical industry. It is probable that bodies bearing some analogy to carbon or nitrogen would serve also in the production of steels. Is it not already known that granulated iron, which is harder than ordinary iron, and which in some respects is allied to steel, is principally obtained by the reduction of minerals containing phosphorus? If the combination of iron with carbon and nitrogen ought to be taken as the type of steel, it would be very curious to determine the modifications undergone by iron when carbon or nitrogen are replaced by other simple bodies. I shall devote another communication to this interesting subject, showing that there are many steels, and that they form a whole group of compounds, each of which ought to be successively examined.

It appears to me that the following conclusions may be drawn from the new facts which I have communicated to the Academy:—

1. In studying the successive or simultaneous action of nitrogen and carbon on iron, ammonia may be used advantageously to furnish nitrogen, and lighting-gas carbon; the chemical reactions thus produced by gases, yield pure compounds; they can be easily observed and regulated.

2. When iron has been submitted, not for too long a time, to the action of ammoniacal gas, it produces no crust of nitride of iron, it is simply nitrogenised, becomes then of a zinc white, partly preserving its malleability, and resembling a real alloy.

3. Iron heated in a current of lighting-gas immediately becomes carburetted and transformed into a very soft, grey graphite like, fusible cast-iron, which is suitable for the finest castings. Steel is never formed in this reaction of lighting-gas on cast-iron.

4. Steel is formed by the reaction of carbon and nitrogen on iron.

5. Pure iron, which, under the influence of lighting-gas, is transformed into very fusible cast-iron, is previously nitrogenised, loses its fusibility, and is converted into steel by the action of the gas. Fragments of the same metal have been nitrogenised for different lengths of time, and then submitted to the action of lighting-gas; those retaining a small proportion of nitrogen were very incompletely converted into steel; those, on the contrary, which were strongly nitrogenised formed a beautiful steel. It is, then, in some measure, the quantity of nitrogen contained by iron during carburisation which determines the degree of acieration.

6. It does not seem to me possible to admit that cementation may be produced exclusively by a carburetted volatile body, since lighting-gas acting at red heat on iron forms only cast-iron, while by the previous presence of nitrogen in the metal it is immediately converted into steel.

7. When iron is transformed into steel the nitrogen is not eliminated by carbon, for I have ascertained that all the steels of commerce are nitrogenised, and disengage an abundance of ammonia when submitted to the action of dry hydrogen.

8. All these facts tend to the following conclusion, which is a condensation of my paper:—That steel is not, as it has hitherto been considered, a carburet of iron, but rather a nitro-carburetted iron. I have adopted the title of nitro-carburetted iron to explain the composition of steel because it well expresses my opinion of the composition of this body, in which such small proportions of metalloids so completely modify the properties of iron.

The paper having been read,

M. DUMAS rose and congratulated M. Fremy and the Academy on the important results which must flow from the labours of the author of the paper. The theory of the production of steel seemed henceforth determined, and it might reasonably be hoped that great practical results would ensue. Who, for instance, did not foresee—and it was for M. Fremy to follow out the demonstration—that great advantage would result from these new, methodical, regular, and certain processes, when there was occasion either to case-harden the surface or edge of certain iron implements or instruments? After having forged, filed, and finished them off in the state of iron, a current of ammoniacal and carburetted gases would convert them more or less completely into steel. The depth of the stratum of steel being regulated by the duration of this gaseous cementation with a certainty never obtainable by cementation with powders, or by the use of horn or animal matters in the empirical pro-

cesses. The Academy could but congratulate M. Fremy on his present success, and on his disinterestedness in publishing his important labours.

M. MORIN remarked that M. Fremy's researches explained numerous empirical receipts and processes for the cementation of steel. In most of these processes mixtures were employed containing various proportions of carbon, and of more or less nitrogenised substances, such as ammoniacal salts, horn shavings, leather cuttings, soot, &c., &c., the result being a cementation more or less deep according to the use to which the instruments are to be applied. He thought it necessary also to observe that the character of steels produced by different methods varied greatly, not only where these methods differed, but also with almost identical processes. Moreover, certain kinds of steel, and it seems particularly those obtained by puddling, after undergoing many energetic fagottings, appear to be susceptible of losing their characteristic properties of hardness and elasticity acquired by tempering, and to acquire a considerable resemblance to the most ductile irons. Lastly, the cast steels produced by the new processes of fabrication, when properly forged, possessed an elastic resistance, capable of undergoing a much greater strain than those manufactured by the ordinary methods.

Besides the foregoing speakers, several members of the Academy joined in the discussion when M. Fremy's paper had been read, and among other remarks it was stated that the composition of cast iron might be different to that of steel. M. Chevreul then made two observations, one respecting black cast iron, and the other on the composition of steels.

1. **On Black Cast Iron.**—At the end of the last century (1799) Proust observed that when treated with weak sulphuric acid, black cast iron yielded an oily matter, a portion of which was carried off by the hydrogen gas and made the tubes of the apparatus greasy, while the other portion remained mixed with the black residue, from which alcohol could extract it. I never neglect an opportunity of quoting this observation of my illustrious fellow citizen, as an example of the possibility of producing, by chemical forces, compounds analogous to those of organic nature. Experience has long since proved that aqueous vapour by reacting on charcoal, yields, besides carbonic acid, or oxide of carbon, nothing but hydrogen, and not carburetted hydrogen, as hitherto believed:¹ the combination of the cast steel with the nascent hydrogen seeming to me difficult to admit; this has led me to conjecture that in Proust's experiment the water might assist in the production of the oily matter simultaneously with the carbon and hydrogen. Now, M. Fremy's observations seem to throw a light on the subject, by indicating that it is not carbon, as was represented, which yields the oily matter.

2. **Composition of Steels.**—Independently altogether of science, two bodies possessing different properties have never been confounded; so that when an iron was observed which hardened on being suddenly cooled, it was distinguished from one preserving its original ductility after undergoing the same cooling influence. Thenceforward the name of steel was bestowed upon the first substance to distinguish it from what is properly called iron, or, in other words, between steel, which tempering hardens, and iron, which tempering does not harden. Since the time of the revival of chemistry the difference between steel and iron was attributed to the presence of about a thousandth part of carbon in the

former. Later, the influence of various bodies on steel was recognised. Berthier mentions chromium; Faraday and Stodart aluminium, platinum, and its accompanying metals; but the fact which to me seems of the greatest importance, is the method by which MM. Faraday and Stodart obtained from cast iron some centièmes of iridium and osmium, which when analysed yielded no trace of carbon. Setting aside the question whether steel is an indefinite compound of iron and one or several simple bodies distributed through the whole mass of the steel, or whether it is a definite compound of iron with one or several simple bodies distributed in indefinite proportions in the iron in excess of the elements of the definite compound, I conclude, from the whole of the facts I have stated, that in a chemical treatise steel in general must be regarded not as a definite compound by the nature of its constituent parts, but as a particular state of iron produced by the union of this metal with bodies the nature of which is variable, and it is from this point of view that, after defining steel, independently of all scientific considerations, as an iron which is hardened by tempering, I discriminate in my fourteenth lesson on "Chemistry applied to Dyeing," published in 1829, p. 78:—

1. Steels formed by iron and carbon;
2. Steels formed by iron, carbon, and a third body;
3. Steels formed by iron and some other body which is not carbon; or uncarbonised steels.

The results of M. Fremy's interesting experiments are, it seems to me, easily connected with what is already known of steel if looked upon from the point of view I have explained, instead of regarding them in the general way. It is now important to find out,—1, Whether it is true, as Guyton says, that diamond dust will turn iron into steel; 2, Whether acieration can be effected without the intervention of nitrogen.

M. FREMY said, in reply to the preceding observations, he was happy to observe the interest the Academy took in his enquiries on steel, and he thanked its members for the friendly way they had spoken of his labours. It was not his intention to treat, in the present communication, of the influence exercised by nitrogen and carbon on the properties of iron, but all questions relating to the fabrication of steel and cast iron had for a long time been carefully studied in his laboratory, and the results would by-and-by yield materials for papers bearing on the following points:—

1. The relative proportions of nitrogen and carbon required to be introduced into iron to form good steel;
2. Circumstances opposing acieration or altering the qualities of a steel when formed;
3. The mode in which carbon penetrates the metallic mass;
4. The reason why such small quantities of carbon and nitrogen can convert iron into steel;
5. The study of steels containing metals such as manganese, chromium, tungsten, aluminium, &c.;
6. Classification of different kinds of cast iron; examination of the effect which silicium, phosphorus, arsenic, and sulphur exert upon these compounds; study of the cast iron best suited to the fabrication of puddled steel.

—*Comptes-Rendus.*

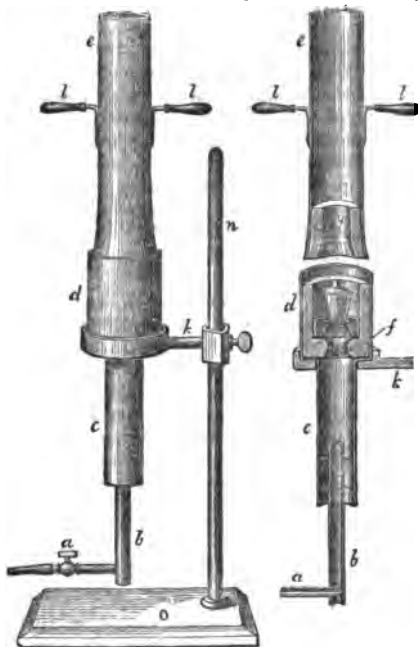
Griffin's Reverberatory Gas Furnace, for Analytical Operations with Platinum Crucibles.

THIS apparatus produces a high temperature without the use of a blowing-machine. It is especially suitable for the decomposition of siliceous minerals by fusion with

¹ "Eighth Lesson on Chemistry Applied to Dyeing," pp. 23, 24.

alkaline carbonates in platinum crucibles, being capable of fusing 1000 grains of anhydrous carbonate of soda in ten minutes.

The furnace is represented by the annexed engraving.



It consists essentially in a combination of tubes, within which a mixture of coal-gas and air is burnt, and in which a small fire-clay or plumbago furnace holds the crucible in the point of greatest heat.

Letter *a* represents a brass tube and stop-cock of $\frac{1}{2}$ inch bore, for the supply of coal-gas at ordinary pressure. The end of it, fixed within the tube *b*, is pierced with several small holes for the emission of the gas. The brass tube *b* is 9 inches long and $\frac{1}{2}$ inch bore, open at both ends. The iron tube *c* is 8 inches long and $2\frac{1}{2}$ inches diameter, open at both ends, but joined at the top to the iron body of the furnace *d*. This body is 4 inches high, and 4 inches in external diameter. The iron tube *e*, made conical at the lower end, is 15 inches long, $2\frac{1}{2}$ inches wide at the top, and $3\frac{1}{2}$ inches wide at the bottom. The body of the furnace, *d*, is fitted with a lining of fire-clay or plumbago, the upper part of which is cylindrical, with a bore of $2\frac{1}{2}$ inches, and the lower part forms a massive diaphragm, with a central opening of $1\frac{1}{2}$ inches, as shown by *f* in the section. Upon this diaphragm is loosely placed a cast-iron ring with three knife-edges, represented in the section and separately by the figures *h* and *i*, the use of which is to support crucibles. By reversing this ring it can be made to suit crucibles of different sizes. In the lower part of the chimney, *e*, a reverberatory dome of fire-clay or of plumbago is fed. This serves to deflect the flame downwards upon the top of the crucibles. The space provided in the body of the furnace for the reception of crucibles is $2\frac{1}{2}$ inches wide and $2\frac{1}{2}$ inches high, and it will take in a crucible of nearly those dimensions.

The furnace is supported by a flanged iron ring, *h*, attached to a triangular socket, which can be screwed on the $\frac{3}{4}$ inch rod of an ordinary iron retort-stand, as represented by *n*, *o*.

The gas, entering the furnace by the supply-pipe, *a*, mixes with the air, which rises through the lower end of the tube *b*, and burns with a smokeless flame when lighted at the upper end of that tube. This flame is then supplied with additional air by the lower end of the tube *c*, a great draught being produced by the action of the tall and wide chimney, *e*. When the pressure of the gas is strong, the flame reaches from the tube *b* to the top of the chimney *e*, being in that case two feet long. The point of greatest heat is at 8 or 9 inches above the upper end of the tube *b*, and the arrangement of the furnace is such as to fix the crucible in the focus of the heat. The condition of the crucible can at any time be seen on lifting the dome and chimney *e*, by means of the handles *l*, *l*.

The amount of gas supplied to the furnace is regulated by the stop-cock *a*, from which a flexible pipe should lead to a second stop-cock, attached to a fixed gas-pipe. The latter should be opened wider than is necessary to supply the furnace with gas. The pressure should then be regulated by the stop-cock *a*. This arrangement prevents, to some extent, the blowing down of the flame when the pressure of the gas in the pipe beyond the fixed stop-cock happens to be suddenly lessened.—*Pharmaceutical Journal*.

PHARMACY, TOXICOLOGY, &c.

Improvements in the Processes for the Preparation of Hydriodate of Ammonia and Veratria,¹ by MURRAY THOMSON, M.D., F.C.S., Lecturer on Chemistry, Edinburgh.

Hydriodate of Ammonia.—There are at present two methods in use for the preparation of this salt. One of these is the same as that given in the Edinburgh Pharmacopœia for the preparation of iodide of potassium, only that the iodide of iron, which is first made, is decomposed by ammonia or carbonate of ammonia, instead of by carbonate of potass. By separating with a filter the insoluble oxide or carbonate of iron, the hydriodate of ammonia is obtained in solution, which it is now only necessary to evaporate, at not too high a temperature, so as to obtain the salt. The other method is to make first a solution of hydriodic acid, by passing sulphuretted hydrogen gas into iodine suspended in water. Hydriodic acid is thus formed, and sulphur precipitated; the latter is separated by filtration, and the acid filtrate is neutralised with ammonia or carbonate of ammonia, and then evaporated to dryness as before.

In either of these ways the salt may be made quite pure, at least so far as obtaining a solution of it is concerned. It is in the evaporation of this, so as to get the dry salt, that the difficulty lies in obtaining it quite colourless. Almost invariably, as the drying of the salt is just being finished, a slight decomposition takes place, and some of the iodine is set free, and communicates its brown colour to the whole mass. To obviate this tendency to decomposition is the purpose of my improvement, and it consists, in the first place, in taking care that the salt, during the time of its being dried, shall always contain a slight excess of ammonia; and, secondly, in reconverting the liberated iodine into hydriodic acid, by from time to time allowing a small amount of sulphuretted hydrogen gas to flow over the salt while it is being dried, an apparatus to evolve the gas being kept

¹ Read before the Pharmaceutical Society of Edinburgh.

at hand during the operation. The effect of adding the gas is, that the brown salt almost immediately becomes white. As soon as this bleaching, so to speak, is effected, the stream of gas is withdrawn, and the drying continued, and only resumed if iodine should be liberated a second time. If the salt is well prepared at first, it seldom needs the second application. As soon as the salt is dry, it should, while warm, be put into a dry stoppered bottle, as it is deliquescent. As long as the salt is dry, it will keep a long time colourless, but if it get damp, it is partially decomposed and again gets brown, but its colourless condition may again be restored by a repetition of the above treatment.

It must be admitted that an infinitesimal amount of sulphur finds its way in among the salt, by the use of the sulphuretted hydrogen gas as a decoloriser; in fact, whatever quantity of that gas is actually consumed, has its sulphur precipitated into the hydriodate. To show, however, that this is really no drawback to the use of the gas, I may mention that an exceedingly small amount of free iodine is enough to communicate a considerable depth of colour to a comparatively large amount of the hydriodate. Now, it is to yield hydrogen to this iodine that the sulphuretted hydrogen gas is employed. The greater part of the gas is, however, dissipated as such, and the quantity of sulphur, therefore, is necessarily small in quantity: in fact, it is so minute that I have not been able to detect its presence in the samples I have prepared by this process, although I employed our most delicate tests for sulphur. I beg, therefore, to recommend the above process for adoption in the preparation of hydriodate of ammonia, a salt which most will agree is far from being easily obtained in a colourless condition.

Preparation of Veratria.—In all the processes as yet published for the extraction of veratria from the cevadilla seeds, which contain it in largest amount, as those given by the London and Edinburgh Pharmacopœias, as also the one given by Wittstein (*Repertoire für die Pharmacie*), alcohol is the agent employed in the very first treatment of the seeds, so as to dissolve out the alkaloid.

I propose to substitute for the use of spirit at this stage that of boiling water acidulated with hydrochloric acid, proceeding as follows:—The cevadilla seeds are denuded of their capsules by the method recommended in the Edinburgh Pharmacopœia. Any convenient quantity of them is covered with the boiling acidulated water, which is allowed to act on the seeds for twenty-four hours. The liquor is then strained and poured out, and the residual seeds again treated with hot acidulated water as before, such treatment being repeated six or eight times. The accumulated liquors from these operations are now evaporated to about one-twentieth of their total bulk. Ammonia is now added until no more precipitate is thrown down. This precipitate of impure veratria is now separated by a filter, washed, and then dried on the water-bath. It is now reduced to powder and treated several times with hot alcohol, which will dissolve out the veratria and very little else. The use of alcohol here is not, however, essential, as the alkaloid might again be extracted by acidulated water; but on the whole it seems better to use spirit at this stage, as the veratria dissolves out comparatively free from impurities. It should also be remembered that the quantity of spirit needed to dissolve out all the veratria contained in this precipitate is far less than what would be needed to extract the same from the mass of seeds it

corresponds to. The amount of alcohol used in the latter case is nearly six times that of the former.

After the spirit has dissolved out the whole of the veratria, the solution is distilled, by which much of the spirit is recovered. The half-aqueous half-spirituous residue is now evaporated, so as to expel all the spirit. Acidulated water and a moderate quantity of animal charcoal are now added, and the whole is heated and filtered. More acidulated water is added to the residue, the heating and filtering being again repeated. This should be done five or six times,—as long, in fact, as the filtrate will yield a precipitate by ammonia. Ammonia is now added to the collected filtrates, when the veratria is thrown down. It is now separated and dried. As so obtained, it has a pale straw colour. It will be obtained whiter by repeating the treatment with the acidulated water and charcoal.

By this process I have obtained at the rate of twenty grains of the alkaloid from one (avoirdupois) pound of seeds. In one or two more trials of this process, I have no doubt this amount might be somewhat increased.

I may make the following addendum on the subject of veratria, having reference to a test for it. In most chemical manuals which treat of this alkaloid, it is said that it gives with nitric acid a red or crimson colour, and with oil of vitriol also a red, which afterwards becomes yellow. In any of the samples I have either made or seen, I have not been able to get any colour at all with nitric acid, and with sulphuric acid the following is what I have observed:—When the veratria and the acid are mixed, no colour at all, or a very feeble one, is at first developed; but in the course of three or four minutes a full blood-red colour is given, and this may last for two or three hours quite unchanged, after which it gradually fades away. The only other substance which is likely to be mistaken for it is, therefore, salicine, but the points of distinction are these: with salicine, the colour given by sulphuric acid is immediate, and is not blood-red, but decidedly purple-red; and then the colour is so permanent as to last over six hours, by which time the colour of the veratria is quite gone.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, May 3, 1861.

On the Solar Eclipse of July 18, 1860, by MICHAEL FARADAY, D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.

I do not wonder at your desire to be here this evening. We had the hopes of an account of the eclipse of last July from one who was the directing spirit of the investigations then made, and with every intent and desire to give us that account here, the very matter respecting the eclipse has so accumulated upon him as to prevent that taking place; and we cannot possibly be so unjust as to urge a compliance with an assent made at a distant time, when he thought he could accomplish what we desired, since he is now preparing results, and doing far higher service to science than he would be doing even here, where the purpose is to convey to your minds matter which I may, perhaps, be able, certainly not so well, but perhaps with sufficient clearness, to lay before you. Mr. De la Rue was the person who guided the expedition, of which I am to be the mere mechanical agent in giving you an account.

There happened an eclipse in the month of July last

year. You know very well that the three great bodies—the sun, our earth, and the moon, are rolling amongst each other; that, whereas the moon must ever cast a shadow as well as the earth, the sun being the source of light, those shadows travelling through space must be always cast somewhere, except when they coincide. When the shadows of the sun and moon coincide, we being outward from the sun, we have an eclipse of the sun; when otherwise, we have an eclipse of the moon. And it so happens that the eclipse of the 18th of July last year was a most remarkably fortunate one for the observers of Europe. It was visible on a part of the earth accessible to us, namely, Spain and part of Algeria, and at a time when the moon was nearest to us, and the sun furthest from us; and it so happened that the projected discs of those two bodies in the heavens were in that position in which the image of the moon overlapped and surpassed that of the sun; and, therefore, to those who took the centre of the line described by their shadows there was, in fact, a total eclipse of the sun. And not merely was there a total eclipse of the sun, but that total eclipse would last for the longest time that could very well happen in the juxtaposition of the three bodies—namely, for three minutes and a-half. The eclipse began in North America, came across the Atlantic, passed over Spain, crossed the Mediterranean into Algeria, through the northern parts of Africa, and terminated, as far as the records reach, just about the Red Sea. We have here maps showing the path of the eclipse in Spain, and in the centre of that path, for nearly sixty miles' width, so much of the sun was covered by the moon as to make the passage of the moon in that part occupy about three and a-half minutes—the partial eclipse, of course, lasting much longer.

Now, you will understand that since the time of Herschell the elder, men have thought much more philosophically about the condition of the sun and moon than they did in former times. He first gave us views respecting the constitution of the sun, gathered partly from the spots and holes occurring upon its surface, which at first almost alarmed and startled the philosophers, but which have been, as it were, only the first sowing of seeds of knowledge to produce fruit hereafter. This knowledge has been growing from year to year, from eclipse to eclipse, in a most extraordinary manner. Every effort is being made to take advantage of these rare opportunities for observing the sun physically and philosophically; for you may get a peep at the sun at these times which you can never get at any other. To observe the moon blotting out the sun from observation, and yet letting you close by the sun, so as to see the stars and planets which would be always visible in the day time, but for that wonderful flood of light which puts all others into the shade,—to get such an opportunity is most important to the philosopher, especially since the occasions are so rare that for a man who has seen one to have witnessed another, and so been able to add the experience of the first to his judgment in regard to the second, is a remarkable occurrence. That man, Airey, whom I like best to designate by his name which is his highest honour, has seen no less than three solar eclipses, and has stored up so much information and judgment respecting them as to make him the chief of the party who went forth to observe the recent phenomena. I believe philosophers in all countries send their reports to him, and they go forth to the world hereafter in a proper manner, about which as I have said, he is now engaged.

Now, what is the sun? Is it a great fire? No. Is it an electric spark? No. There are plenty of reasons for saying that the sun is not that. It is possible to raise a conjecture in the mind of the philosopher which shall give him a reasonable hope of ascertaining the nature of the sun. The more, however, we can see of the sun, the better we shall be able to judge of it. Not the more brightly we see the sun, but the more closely and distinctly; and the great use of the late expedition to Spain

has been in its giving us a great deal more of that knowledge. What is the sun, then? I can show you a little sun here in the voltaic light. It is a brilliant spark, and what would it be to look at the edge of that light, to examine the condition of that part between it and darkness? You tell me, and you say truly, that if you look at the sun you cannot tell what is its form and condition; it is too brilliant and beautiful—"dark with excess of light."

An eclipse is the only opportunity we have of seeing anything like what belongs to the sun,—essentially belongs to him, and keeps him in that condition which makes him essential to the whole world, to the worlds of worlds, to all the stars and planets that pass through space,—that condition which makes an essential part of his constitution, which belongs to his physical history even more than the light that shines upon us. The light has shone upon us so long, it shines upon us day by day, hour by hour, minute by minute; we could not live without it; neither plants nor animals could grow without it; we have it so constantly with us that it is regarded as one of the commonest things in the world. So we think of the sun; but to get at the truth of the matter with regard to its physical condition, we must also know that which belongs to its outside, I was going to say; we do not know how it is finished off in space; is it a hot body sustained by any action going on from day to day? What evidence have we? We must look at that when the eclipse comes on, when the sun is hidden—not by your hands, as in the case of our bright light here, but by the moon, which blots it out entirely;—then is the time for our examination, for if the least part of the light is present you have no chance of seeing anything beyond the light of the sun. The smallest particle of sunlight is far brighter than our voltaic light, and prevents your seeing anything else. It does happen now and then—it won't happen for many years to come again—that you have a complete occultation. Now, think of the philosophic spirit exhibited on the last occasion, in July. There was to be a hiding of the sun for three minutes; it was to take place in a distant country; it would be an expensive undertaking to get there, up in the mountains, or the plains; and there were the chances of the weather against any observations being made—let but a cloud come over the place for three minutes and a-half, and all the exertion would be lost;—yet men were found ready to go. Fifty philosophers started from Great Britain, and Government placed a fine ship at their disposal. Many of them left the Association at Oxford, which was then sitting, to go out to the mountains and plains, and make their observations. Fortunately, they had fair weather, and they went and succeeded. I am ashamed to say that I do not remember the names of many of those who so richly deserve our thanks for their efforts. Amongst them, however, a certain party was organised to observe the sun photographically. Perhaps you wonder that we should make any point of that. You know the wonders of photography—how it not only enables the sun to give us his own picture, but enables him to give us the pictures of other persons who are lighted up by his rays. Now, of what service is this in the case of an eclipse of the sun? Why, it can do wonders. We have tried, and we therefore know, what can be done. You look at the eclipse; you see the disc of the moon gradually advancing till the sun is perfectly obscured, and you find it is hidden for three minutes. It is only during those three minutes that you can see what you want—that interval between light and darkness which belongs to the outside of the photosphere of the sun,—and where is the mind that can remain steady enough, during that brief interval, to devote all its energies to the use of the means provided for recording the phenomena, and not be led away by those momentary splendours which, perhaps, will never be witnessed again? Who, among all those who went to Spain last July, will ever see another eclipse? I am told by astronomers—I know

nothing of the subject myself—that the occurrence will not happen again during an ordinary life. What we want, then, is an eye that, in looking at the sun, shall not be blinded, as we are here in looking at our voltaic light. Providence has given to the eye such a wonderful power of adaptation, that if I look at this light for a time, the eye is so changed as to be incapable, for a moment or two, of ordinary vision. Now we want an accurate eye—which photography gives us—that can receive and record an impression of the condition of the sun when it is just hidden, or of those parts which are visible,—an eye that can see and record these things, not misled by the previous brilliancy. For this is the beauty of photography: it can record what has happened, so that it can be examined afterwards. What are mind and memory in man when disturbed by these extraordinary appearances of a solar eclipse? Some of those zealous observers who went to Spain last year told me that there was not one of them who kept possession of his mind so thoroughly as not to be a little misled this way or that way by the wonders of the phenomena they witnessed; and he to whom I now principally allude, because his investigations most concern me, at one moment almost regretted that he had taken charge of the expedition under his care (which cost him personally 300*l.* or 400*l.*, and required a large staff and a great deal of apparatus) because it deprived him of the pleasure of looking about and seeing the glorious sight comparatively undisturbed. I dare say that many of those who merely looked on were more happy in gazing at the beauties of the eclipse and the wonders of the landscape, than those who were engaged in special investigations which they had to record. You have, then, in photography an eye which can look at the sun, and which, looking there for a few seconds, can take upon its memory—that is its collodion—a perfect record of the sun's appearance, and keep it for ever; then, by means of successive plates, you have an eye which can take another look, and then another, and perhaps another, if the weather be propitious, and bring away all these impressions faithfully recorded for future observation. Now, I must try to impress you, if I can, with the real duty performed by this body of men to whom I have alluded. Mr. De la Rue went out with five assistants—Mr. Beckley, Mr. Downes, Mr. Beck, Mr. Reynolds, and Mr. Clark, all of whom were perfectly well educated in regard to the operations with which they were entrusted. They were determined not to fail when the eventful three minutes came which would never return; and so well did they carry on their operations, under the generalship of their head, that they brought away not fewer than thirty-three photographs of the sun during the eclipse, four of them taken just about the period of totality. They narrowly escaped being burned up, for the corn around them once took fire; and they had also the chance of an accident from the wind, which blew very roughly; but they succeeded in those three minutes of utmost excitement, and procured those photographs of the sun which I am now about to show you. I have no time to tell you what the Government did in lending that fine ship, the *Himalaya*, for the occasion; what the Spanish authorities did; what Mr. Vignolles, the engineer of the railway, did; and, indeed, what they all did;—I will not take up your time in returning thanks to those who have worked so well for science. You may say, it was, no doubt, an easy thing for the sun to represent his own face in a photograph, since he can give us the faces of others; but there is this peculiarity about the sun, that over action destroys everything. You see here a representation of the sun with certain dark spots, which can be very well obtained by using the proper means; but leave the picture a little too long, and out go the spots; the matter is over-acted upon, all peculiarity disappears, and nothing but a dull appearance remains behind. It required a special instrument, a remarkably beautiful instrument, which has been constructed for some

years, and placed at Kew, called the photoheliograph. This was placed in Mr. De la Rue's hands, with a sum of money by no means sufficient for the purpose, to enable him to carry out his operations. The first view I shall show you will be a picture of these men themselves in a group. You will imagine that they have left England and arrived in Spain, landed at Bilboa, and gone over the mountains. Mr. Vignolles has conveyed two tons of apparatus to the spot, and they are all now upon the ground. You see the photographic house, and the instrument, which is like a mis-shapen telescope pointing up to the sky, being balanced upon its axis by a weight with a proper clock-work arrangement. As soon as the sun's rays enter the instrument, they pass through the glasses which the optician puts there, and are afterwards expanded by means of other arrangements until they fall upon a collodion plate below, of $4\frac{1}{2}$ inches diameter. Upon that the sun has to make its own face; but let the sun look there but a moment, whilst you count one, and the face is spoiled. In order to get a proper impression, you require a time so short as scarcely to be measured except by micro-metrical means. The only way was to take a certain opaque screen, and cut a small slit in it, and then, by a proper arrangement, to draw the slit as fast as possible across the bundle of rays passing at that spot; and that—I do not say second, but that instant, was sufficient to secure a full and perfect impression. This is the way in which pictures are taken by the photoheliograph. You have now a view of the *Himalaya* on its voyage outward, for you are fairly entitled to see how they proceeded from the commencement. Another view gives you a representation of one of the waggons in which luggage had to be carried over the country. Mr. De la Rue has set himself down upon a threshing floor,—a hard place where the corn is spread out, and the oxen wander to and fro treading it under their feet. You see also the chamber which had to be taken out and built. Here also is the telescope and the apparatus. Mr. De la Rue will now give us a view of the sun itself, with one of its spots. A second view shows us the same spot but in a different position, owing to the revolution that has taken place. In this way we are able to compare the history of these spots from time to time.

A few views will now show us the progress of the eclipse. The first shows the sun to be partially obscured, but the spots are visible. The next shows the moon's disc advancing. And the third exhibits an almost total obscuration, leaving only a crescent of light. The next view is taken at the period of totality, when there is an entire extinction of the sun by the moon. And now you see all these beautiful phenomena, these luminous appearances which have been so accurately photographed. You must not suppose that all these pictures precisely correspond in luminosity to the brilliancy of the objects represented. Though the negative may give an exact representation, the different impressions from the same negative will vary, and present some irregularities owing to the peculiar state of the atmosphere at the time. Clouds, dew, rain, all these different phenomena operate as a disturbing influence, and hence the irregularities in some of the impressions. However, we are obliged to compound for many things, and accept them as we can get them. But vain are all regrets. The services rendered by photography are still remarkable. I do not ask Mr. De la Rue what he remembers, what he observed, of the eclipse; I ask him what he has recorded and brought away. Two of us may differ in opinion as to what took place, but there is the fact, the record made at the time, to appeal to—the enduring representation of the exact state of the sun at that time. While Mr. De la Rue was directing the photographic operations, attending to the time, giving as much as a minute to the first photograph and about twenty seconds for the second one, he was looking through his telescope at the sun, either with or without shade, as the eclipse advanced, and making

drawings by hand. I have some of them here, and they tally perfectly with the images and representations given by the photographs. In order to give you an idea of what was seen around the sun during the eclipse, I must refer you to our diagrams. There you perceive what are called red prominences—one never does get good names for these things—certain red appearances not of an ordinary kind, which are seen upon the sun when it is blotted out by the moon. You see them in different positions according as the moon advances. You observe one prominence, or rather independent existence, floating above the sun, touching neither the sun nor moon. You would wonder if I could tell you how these things have been recorded—these great prominences about the body of the sun. Here is one 28,000 miles in length. Now we know that the body of the sun is exceedingly light, far lighter than the matter of the earth. What, then, can this be which is floating above it, a cloud in the air, with the most luminous part towards the sun, whilst the other bodies had their luminous parts away from the sun? That one prominence is, I think, 28,000 miles in length—for these things can be measured and have been measured—and 74,000 miles above the sun. Now what is 74,000 miles? It is three times the circumference of the earth. Such is the height of that cloud, as far as we know it. It may be much farther in physical existence for what we can tell. Perhaps we may know hereafter. At present we are only taking the first steps. We are infants learning to walk with regard to this part of the sun. No one can imagine that what we are doing here is anything else than what will appear as absurd speculations and crude conjectures when we make further discoveries in the mode of observing these things. There has been no eclipse in which the mode of observation has not advanced on that of its predecessor in the most wonderful manner, and we shall still go on advancing. We now give you views of the eclipse in its various stages; and here we have a view of that beautiful appearance the corona, which I knew would call forth your admiration. This differs in character at different times, sometimes appearing like superposed discs, and at other times having this glorious stellar character streaming out behind the moon, the moon appearing like a brazen globe before it. This is not a photographic corona which you have before you, but it is an accurate drawing brought away from the spot.

The most important question to be solved by these observations was, what are these prominences? And the first part of that question is, what do they belong to? Are they solar? Or are they lunar? Or do they depend upon the sun, moon, and earth? Where do they exist, and what are they? One of the chief results, perhaps, of Mr. Downes's observations is, that they belong to the sun. Consider what is taking place. There is the disc of the moon moving over the disc of the sun, and it is looked at by persons who choose a position where they may get into the centre line of the projection. Amongst other prominences there was one, here represented in the diagram, which, I believe, appeared before the sun was entirely out of sight. Mr. Airey and several others saw it before the sun was completely obscured. Now, that is not a prominence which is first of all brought into sight by the shading away of the sun, and then cut off from the sight by the moon sliding over it; but it is a prominence which, when it comes into sight, goes sailing along upon the edge of the moon, and may remain in sight some time. There is another of the same kind, projecting above the moon at every part, and not disappearing at all from us by any occultation—only disappearing when the brilliant light of the sun, arising from the uncovering of his face, comes into view and extinguishes all other things. But then there is this other question—what do these prominences tell with regard to their place upon the sun or the moon? One or the other or both? What is their story? Their story, as I understand it, is that they are

upon the sun, and for this simple reason. Very great care was taken by the astronomers to measure off the places of these appearances; they are all marked off in a beautiful manner, so that they can identify the different prominences when they see each others drawings, whether they stand in the line of the eclipse accurately, or a little to the right or the left—they all correspond in the position of the prominences. The sun, then, being put into the telescope, and motion given to it, so that it remains fixed in the telescope, the moon appears to pass over the sun in a given direction, and while it covers up some prominences it uncovers others, and slides between others; so that they would appear to belong to the sun and not to the moon. That is a question which seems to be entirely solved by these observations. Here is a prominence in the diagram; if it belonged to the moon, it would have moved with the moon; but belonging to the sun, it is left behind. A careful comparison of the centre of the moon and the centre of the sun, with the places where these prominences appeared, declares that they belong to the sun. That is a most essential and important point to have ascertained with certainty. No sound philosopher can run away and say that these things belong to one body when they belong to another. The great thing is to proceed cautiously, and draw our conclusions with care, lest we slip and fall. We must go on step by step, and be certain as we proceed. I have said that some of these objects are 74,000 miles distant from the sun. One or two of them appear like detached clouds, the time is too short to observe any change in their shape, but they are of different shapes and appearances in different eclipses. Of course you do not suppose that the profile of the sun directed towards us at the time of the eclipse is the only one that has these protuberances upon it. Surely the sun revolves, as the spots prove to us, and if another profile was presented to us we should have other prominences. We cannot doubt that these things which we see exist all over the face of the sun. We know that the moon is a round body because every profile she gives us is round. So it is with the sun; whatever profile we have, these prominences come into view, and we may therefore conclude that they exist all over the surface. It is a remarkable circumstance that a few days after the protuberances were seen there came into view a set of spots upon the face of the sun which corresponded in position according to the revolution of the sun, with the places where the prominences were observed during the eclipse. That is a suggestion that one catches hold of, with a view of verifying at a future time, and putting upon record with the other data which are given.

Another question has been whether the light that came from these prominences or from the corona was polarised. Now it is not my intention to take you into the question of the polarisation of light, but it is an important indication of the probable nature of these substances if the light from the corona or the protuberances is found to be polarised. The light which comes direct from the sun is not polarized, but that which is reflected is polarized; therefore there is an indication of something that sends that light to us in a modified form. Many questions of that sort will come before us in due form as the investigations proceed. Then with regard to its actinic character. We have before us the voltaic light, and we have so arranged it that it shall come through the lens, then through the prism, and then go away to the screen, and there you will see it, decomposed, according to Newton's discovery, into what he called the spectrum—that is, the rays separated. These rays have different powers, and here you begin to see some of the wonderful circumstances connected with the eclipse. You see that the different rays have different characters by their various colours on the screen. These rays write photographically upon proper media (we are now dealing photographically with the sun), and they write not in proportion to their brightness as we see them, not as they write upon the nerve of the human eye. And here comes

a most important part of the philosophy of this subject. You see on the screen how far the rays are visible to you; but then we know that, chemically speaking and photographically speaking, beyond what you can see there are rays which are far more important to us, in many respects, than those which are apparent to the eye. To show this Mr. Downes will take a photographic view of the ray of light which you see going on to the screen, by intercepting it with a prepared plate. As soon as the plate is put in you see how far the rays are visible—the red, green, violet, &c., as you saw them on the screen, and we will mark the limits to which they visibly extend. If those were all the rays, you would of course find that the picture produced on the plate, when developed, would be limited to that space. Now the picture has been developed, and you observe that it extends beyond the limits of what was previously visible, and the outer rays have told chiefly in the production of the picture. They write their image far more beautifully by photography than they do upon the eye itself. These outer rays which have formed the darker part of the picture, where on the screen we could see nothing, are those magnificent and wonderful rays which make plants grow green, and make animals thrive and become cheerful. We have here a picture marked at the places where the red, and the green, and the blue rays fell, and you see these are not the places that exhibit the actinic power. In fact, the red ray has made no impression; the green has made but little; it is at the outer part, beyond the blue, where the most abundant indication of these beautiful actinic rays is apparent. We should make no advance unless we had men to make these observations, to think about them, and consent to have no answer to give to many questions which you may put to them.

My remarks may have suggested a score or a hundred questions to your minds—what is this? and what is that? Well, we must be content to confess our ignorance; we must be satisfied with getting only one point in advance, which will be a point of attachment for further progress. I had intended to say something about the spots in the sun, but our time is nearly expired. We have several drawings here showing a certain condition of the sun, which is only visible now and then, and which appears differently to different eyes. According to Mr. Nasmyth the whole photosphere of the sun is covered with images such as you have here represented, and he calls them willow leaves, a term as little expressive as possible of any meaning, so that you may get nothing but the simple observation. You perceive from our drawing that a spot in the sun is not a mere uniformly black thing. It is very dark in the centre, but gets lighter as you approach the circumference, giving an idea of different coats of atmosphere till you come to the outer one, which is the most luminous. These are the appearances which present themselves under circumstances the most favourable for observations. It happens, however, that certain conditions of the atmosphere give rise to the appearance of what are called sun-boils. This is a most serious evil in regard to taking the sun photographically, the quivering of the atmosphere destroying the character of the rays, and thus interfering with accuracy of representation. It is, perhaps, not more than two or three times in the year that true observations can be taken. The spot represented in the drawing was observed and taken during seven or eight days in which time it had been going on revolving on its own axis, and not going with the sun itself. What a wonderful thing that a spot of that kind, many thousand miles in diameter, can be going on as a great continent! There are many other things about the sun equally wonderful, which men have learned to think of and imagine. Imagine! No, it is something better than imagination. The poet never could have conjured up in his mind what we now soberly speak of as making part of the sun's constitution and existence. It is a reason to give us thoughts of this kind.

But our time is gone. Before you leave I want to show you a photograph of the moon; for although I have been speaking of an eclipse of the sun you are still to understand that photography has advanced so far as to be able to give us representations of our own satellite. We cannot show you the whole of the moon at once; but we can show you a picture of the moon as taken by Mr. De la Rue, not by his hand, but by his mind, by means of photography; and if you apply the stereoscope you will there see that glorious body—for glorious she is, though by no means to be compared to the sun—as a perfect globe floating beautifully in space. You see here the most beautiful contrasts of light and shade, such as no ordinary picture could give and such as no imagination could supply. Here, I think, we must end our discourse. I hope you will accept what I have done to the best of my ability; and I am sure that we shall unanimously join in thanking Mr. De la Rue for all that he has accomplished.

SOCIETY OF ARTS.

Wednesday, May 1, 1861.

PETER GRAHAM, Esq., Member of Council, in the Chair.

Mr. JULIUS DAHLKE read a short, but on the whole, effective paper, at this Meeting of the Society, on "*Filtration and Filtering Media*," of which we propose to sketch the more important points.

As we all know, filters, in some form or other, have been employed from a very early period, but the historical part of Mr. Dahlke's paper is very imperfect, and does not present a good view of the progress of the art of filtration, as it should have done, to carry out its title properly. Thus, while a casual mention was made by the Lecturer of the porous stone and earthenware filters of the ancient Egyptians and Chinese, the early Roman arrangements for this purpose were not even named, nor were the more recent processes of Peacock, Paul, Rommerhausen, Thom, Leloge, Sterling, Beart, Murray, &c., referred to in any way. As regards the scientific aspects of filtration also, Mr. Dahlke is equally deficient, but as other parts of his paper are not without merit, we proceed to quote the same.

"During the past seventy years, gravel, sand, and charcoal, used as a mixture, have been the agents most in vogue amongst filter makers, and it is only lately that due attention has been paid to charcoal as the most efficient filtering medium. Its use is much more frequent now, because not only has it a powerful detergent effect, but it possesses also the peculiar advantage of not becoming foul, while it protects from decomposition other bodies in contact with it.

"It has been often asked why animal charcoal is so effective as a filtering medium? Some attribute this to the presence of so much carbon; but that this is an insufficient reason, is shown by the fact that, although coke contains more carbon than sand, yet it is not superior as a filtering agent.

"Animal charcoal filters about three and a-half times more rapidly than either coke or sand, while it is also greatly superior in this, that it removes many inorganic impurities held in solution, over which the former substances exert no power.

"It appears that the more porosity a filtering medium possesses in itself, the more rapidly does it filter, and the greater is the effect it produces on the water. The latter will be still more decided when, with a greater porosity, peculiar substances are combined.

"This leads me to believe that we may attribute the extraordinary filtering quality of animal charcoal to the fact that its principal component parts are lime and carbon, so combined as to secure a wonderfully fine porosity. Vegetable charcoal, although very porous, and containing far more carbon, has less effect on water.

"I have observed that another substance, of which I shall presently speak, and which (although of an entirely different origin) possesses great similarity in this respect, may in many cases be successfully substituted for animal charcoal. Indeed, there are doubtless numerous substances and compounds which may be used with as great effect. Do we not see that Nature supplies the most beautiful waters from limestone beds? It is hardly necessary to say, could we but imitate her action, that we should be able to do more in this as well as in other things, but we must content ourselves with as much success as our defective knowledge of her laws will permit us.

"Although we know of powerful agents for the removal of different impurities from water, circumstances may and do interpose which render it extremely difficult to obtain the medium in the requisite form for our purpose, and there is nothing yet discovered which will perfectly meet all the requirements of the case. Those who assert that it is possible to construct an apparatus to act as a universal filter for purifying any kind of water effectively whatever may be the impurities, remind me of the vendors of certain patent medicines, who vaunt their nostrums as capable of curing every disease. Their claims are about equally trustworthy.

"I should classify the art of filtration into three systems, viz.:—1st, where the action takes place simply on the surface of the filtering medium; and, where the whole bulk of the filtering medium is calculated to operate on the water, and the detergent effect in its most delicate form may be produced; and 3rd, where both of these systems are conjointly employed.

"The first system requires a filtering medium of such a fine porosity that its pores must be smaller than the minute particles composing the impurities suspended in the water. Such an agent of course must sooner become clogged, than a filtering medium of coarser porosity, and which is meant to act with its whole bulk on the water. But both systems employed together may prove to be useful in several instances, as in the case of domestic filters. The greatest failing of these is that they must become clogged, and the more they are liable to this, the more effectively they act. We often hear of self-cleansing domestic filters, but the fact is that no invention of the kind has been made yet, without involving complications too great for the purposes of ordinary domestic use.

"However, it is not difficult to make a filter for general domestic purposes—although the effective self-cleansing of such an apparatus is still a problem to be solved.

"If the filtering medium employed in this case be solid, and of a fine porosity in its upper part, the clogging impurities will not only be retained on the surface, but may be easily removed by scraping; and then, if the lower part of the filtering medium be prepared of a material capable of producing a detergent effect, it will act the more readily through not being interfered with by the rougher and clogging impurities.

"It should be remembered, too, that in most cases we have here only to deal with some rougher impurities which have found their way into the water on its passage from water works, or other source, to the tap of the consumer.

"Being deeply interested in the subject of filtration, I have never omitted an opportunity of carefully inspecting those house-cisterns which came under my observation; I have, however, seen but few to which the attention necessary to secure the due cleansing had been paid. Most of them were loaded with mud, and in some of them I actually noticed the growth of vegetation (fungi). I conclude, from my observations, that hardly one-fourth of the house-cisterns in London are in such a condition as to afford the consumer a supply of wholesome water like that which flows from the main.

"The difficulty, or I may say the impossibility, of keep-

ing water which is stored in cisterns entirely free from accidental contamination, should lead us to provide a domestic filter capable of removing chemical impurities, as, for example, any lead which may be held in solution; in fact, the practice of filtering water preserved in cisterns and intended for domestic use cannot be too warmly recommended.

"To remove lead from water, Professor Faraday recommends the practice of stirring up animal charcoal with the water so contaminated, the same being then allowed to settle. I have found, however, that, by using this material in a manner to be described hereafter, I never failed in producing the same effect by means of filtration.

"It is easy enough to purify small quantities of water, but the greater the quantity the greater are the difficulties of purification, especially when a certain chemical effect has to be produced.

"It will not be necessary for me to dwell upon the filtering processes required for large waterworks, as the supply is generally taken from such sources that the common sand filter-bed answers the purpose; and where the water is too hard for domestic uses, the beautiful process of Dr. Clark will meet and remedy the evil.

"Experience shows that it is not prudent to adopt the same means of purification for every kind of water, and I should make a difference in the treatment of the water used for domestic and that employed for manufacturing purposes. In the latter it will be often of the greatest importance to have the water as pure as possible, whereas certain so-called impurities in water may not be at all injurious to health. When we consider that no one would call human blood impure which contained 420 grains of saline matter per gallon, I do not know that we are justified (of course, speaking in relation to health) in calling water impure which contains small quantities of certain saline matters, particularly when we have no medical evidence that the small portions of them drunk in such water ever did any harm. Besides which, it should be remarked that the quantity of lime and magnesian salts drunk in water must be greatly exceeded in amount by that which enters the system in the food."

Mr. Dahlke had better have omitted this last paragraph, his knowledge of our language not being sufficient to show him the true signification of the words "pure" and "impure." There are a good many substances essential to the legitimate composition of our bodies that cannot be considered other than "impurities," if present in the water we drink; there is, moreover, abundance of medical and social evidence that large quantities of lime and saline matters in drinking water are productive of decidedly injurious effects, especially upon the skin, kidneys, and digestive organs.

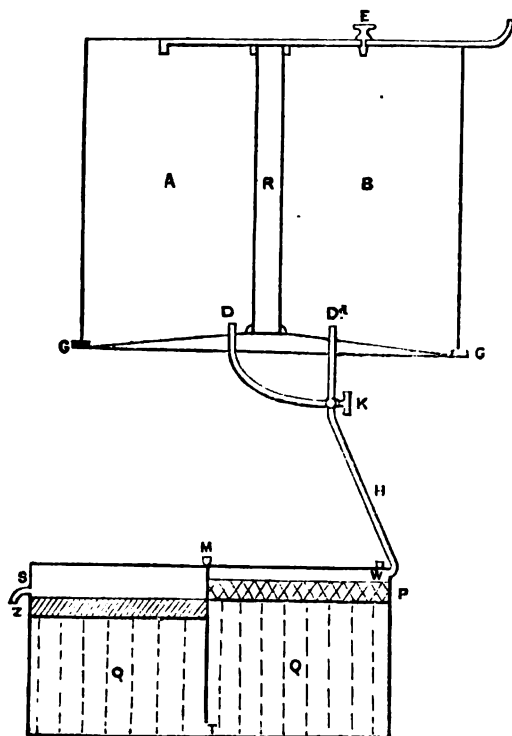
Mr. Dahlke next said that "too pure water is distasteful, and unfit for drinking purposes," citing an illustrative case of some flat-tasting, remarkably pure water, which he had rendered more palatable by adding to it "some finely-dissolved organic impurities," and then filtering through animal charcoal!

Our author prefers animal charcoal to all other filtering media, and regrets that no practical method is yet known of moulding it into blocks without diminishing its powers. Charcoal, he said, as regards its filtering qualities, stands to coke as 15 to 4, and all attempts at solidifying it by calcination with pitch, tar, &c., have failed in practice, owing to the glazing effects of the bitumen, which greatly impairs its action.

Mr. Dahlke's filter we shall shortly describe, as it appears to possess certain advantages over others of the same class that render it well worthy of notice; after various trials he found that the residue, after distillation, of the well-known Torbane-hill mineral, with a small addition of fine clay, will, if saturated with fatty or oily

matter,² and calcined, furnish a very powerful filtering medium, capable of reducing the hardness of water, and removing its colour and odour. He also adds bone-dust both to improve the quality of the "filter-blocks," and in order to regulate their degree of porosity with greater precision. We must now, however, permit Mr. Dahlke to describe his own filter more particularly:—

"Supposing 5000 gallons of water are daily required to be purified, that the water supplied contains nine grains of organic matter per gallon, has a bad smell, and is of seventeen degrees of hardness, I should employ an apparatus of which a woodcut is here given.



"The upper part is a tank made from slate, completely closed, and is divided into two compartments by means of a solid filter block with one inlet, and one outlet for each of them. The supply-pipe is provided with a three-way cock, *x*, which allows the necessary arrangement to be made for admitting the water to one compartment of the tank, and for causing it to pass into the other through the filter-block. If, for instance, it enters at *A*, it has to pass through *x* into *B*, flow off by *DD'*, and through *K* and *H* into the second apparatus.

"This tank, being only intended to free the water from the rougher and clogging impurities, its action is to be reversed as often as may appear necessary in order to remove the collected sediment. This is easily done by shutting off the supply-pipe for *A*, and opening that for *B*; but before the water is allowed to enter the connecting pipe of the second apparatus, the accumulated impurities must be removed through the opening of the corresponding plug *G*, and, that it may flow off easily, the bottoms of the two compartments are inclined towards the plugs.

"The two walls of the tank which face the filtering medium, are so fixed, that they can be opened from time to time, in order that the surface of the filter may be scoured, it being of such a consistency that its surface can be easily abraded by rubbing with hard stone.

² We would suggest blood, or milk, as more likely to answer the purpose.—Ed.

"The water is thus freed from its coarser organic impurities, so far that they cannot possibly clog the second apparatus, which is thus retained almost entirely for chemical action on the water. It consists of a vessel which is also completely closed, with one inlet (*w*), and one outlet (*e*), divided from *m* to *r*, and fitted in the following manner:—*r* is a solid filter-block, cemented into the apparatus and covering the whole surface of the body *a*, thus forming the top part of this filter; it is of much finer porosity than the filtering-block in the tank, in order to separate the finer particles of organic matter from the water. The space *a* is filled up with coarse granular charcoal and a preparation of the Torbane-hill mineral spoken of, intended to operate upon the water so as to remove those matters held in solution, which impart colour, smell, and hardness. *z* is a solid filter-plate, of a more neutral character, also cemented into the vessel to hold together with *r*, the packing of the apparatus.

"The reason why the two materials which fill *a* are employed in a rather coarse grain, is, because the oxidation of several of the impurities taking place during the filtering process, will produce considerable quantities of gases which would soon accumulate in a finer medium and interfere with the rate of filtration, wherefore I rather prefer employing the mixture in a thicker layer.

"The dimensions of the apparatus, and the different filtering media employed in this case, will be 3 feet square by 4 inches in thickness for the filter-block in the tank; each compartment of Apparatus No. II. to be 18 inches high by 2 feet square (internal measure); its fittings are: the plate *r*, 3 inches thick, *a*, 2 feet, and *z*, 3 inches.

"The first clogging in Apparatus No. II. will take place on the surface of *r*, which can be easily avoided by removing the corresponding cover of the vessel, and rubbing the surface of *r* with a piece of hard stone, to which it will slowly yield, and thus be easily freed from its clogging matters, which cannot penetrate into it, the porosity of the filter-cake being too fine.

"The filter-block in the tank is so fixed that it can be taken out and replaced without much inconvenience, should it become worn out.

"The apparatus thus arranged is calculated for not less than 12 feet pressure, and its effect on such water as we supposed to have for illustration will be as follows:—Organic impurities reduced to about half-a-grain per gallon, no trace of smell or colour remains, and its hardness is reduced to about 7 or 8 degrees.

"With regard to the continuance of the chemical effect, I admit that this must have its limits; however, from the fact that the rougher clogging impurities are so easily removed from the apparatus, and that the quantity of the inorganic impurities which will be absorbed must necessarily be comparatively small, a good portion of them escaping in the form of gas, I do not hesitate to assert that it can be successfully employed, for a considerable time, before the filtering agents get exhausted, and repacking is required.

"I do not believe it would be possible to work such a quantity of water so effectively and lastingly in such a small apparatus, if the whole of the filtering medium were to be used in a loose state, as its porosity in the latter case could not be condensed to the state of fineness required.

"This apparatus being only meant to serve for the special purpose I spoke of, it is obvious that it will have to be altered according to circumstances, both as regards the filtering agents to be employed, and its mechanical arrangements.

"If the quantity of water to be filtered be so great that a very large filter-bed is required, I prefer employing the preparation of the Torbane-hill mineral, as described, in a granular state, rather than sand; for this reason, that it filters more than three times as quick, and is five times as light as the latter; consequently a ton of it will, by a layer of equal thickness, filter about sixteen times the

quantity of water that a ton of sand would filter, with the advantage that the filtering would produce at the same time a greater decolorising effect and a considerable softening of the water. A clogging from the precipitation of chalk is not likely to take place, as this substance is separated in a crystalline and granular state. Moreover, those particles of the material which become saturated with organic impurities may, through calcination, regain the greater part of their former efficiency."

Omitting the concluding paragraph of the Paper, which is unimportant, we have now to present to our readers the "cream" of the discussion which followed.

Mr. SPENCER rose first, to deliver a speech, of which the chief object seemed to be, to lay claim to Mr. Dahlke's invention or application entirely. He had broken open one of the filters in question, and found the filtering medium contained about ten per cent. of the magnetic oxide of iron, to which, he said, all its virtues were due. He had determined (quantitatively?) the presence of this substance by means of the magnet, and accused the Author of the paper of infringing his (Mr. Spencer's) patent.

Mr. ATKINS set forth the merits of "carbon" *per se*, as a filtering agent, of which he had manufactured some 40,000 or 50,000 blocks, and proceeded to praise the Moulded Carbon Company and their manufactures in high terms.

Dr. WALLER LEWIS bore testimony to the efficacy of the filter supplied by Mr. Dahlke to a very large public department, as to its powers of deodorising and decolorising water. It was new to him to hear that too pure water was distasteful; and he was afraid Mr. Dahlke was assuming an untenable position. He thought inhabitants of towns were no judges of water, as their palates were disorganised by the impure water supplied to them. He had tasted the water of Lake Bala in North Wales, containing only $\frac{1}{4}$ grains of mineral matter per gallon. Many persons did not like it at first, if they had been accustomed to London water.

Mr. PAINTER dwelt upon the good qualities of a mixture of silica and carbon, and of the balls of the Moulded Carbon Company. These balls were cleaned by blowing air through them—a superior plan to that of Mr. Dahlke, he thought.

Mr. DANCHELL preferred animal charcoal to anything else, and had abandoned all other filtering media.

Mr. WENTWORTH SCOTT said he had been called upon to make various experiments upon the relative merits of animal charcoal and prepared carbons of several kinds, as regards their power of absorbing organic matter, which resulted in favour of the first-named substance. The Moulded Carbon Company's balls were subject to the defect of having in many cases their central cavity greatly on one side, thereby modifying their action injuriously. He recommended the *platinising* of the balls—a simple process—for waters containing much organic matter.

Mr. MOROAN remarked that the public were the best judges of filters in the end. He regretted that scientific discussions were so frequently sullied by personalities, and commented severely, but justly, upon Mr. Spencer's speech.

After a brief reply from Mr. DAHLKE, a vote of thanks was passed to him, on the motion of the Chairman, for his Paper, and the meeting then adjourned.

NOTICES OF BOOKS.

Elements of Agricultural Chemistry. By THOMAS ANDERSON, M.D., F.R.S.E., F.C.S., Professor of Chemistry in the University of Glasgow. Edinburgh: A. and C. Black. 1861.

In many respects this is an admirable manual for the attentive study of the farmer. It contains a full and fair

account of the relation of chemistry to agriculture, telling us what advantages the latter may reasonably expect from the former, and what it may not. The reader is gradually introduced, by easy stages, to such technicalities as are necessarily involved in the subject,—for the Author writes for those who are unacquainted even with the elementary facts of chemical science.

The book opens with an account of the constituents, organic and inorganic, of plants. The proximate vegetable and animal constituents, and the processes whereby these are originated by organic action on the soil and air, are described. The chapter on the General Principles of Manuring is crowded with facts, accurately but cautiously given. The distinctions made between *special* and *general* manures are remarkably sound. The following are the titles of the remaining chapters of the volume:—The Composition and Properties of Farm-yard and Liquid Manures—of Vegetable Manures—of Animal Manures—and of Mineral Manures—The Valuation of Manures—The Rotation of Crops—and the Feeding of Farm-Stock. Almost every page gives evidence of Dr. Anderson's large experience as a practical agricultural chemist. His views on the various controversies connected with this important branch of practical science are temperately expressed, and supported by a large number of well-contrived experiments.

The book, for the most part, is clearly written. Occasionally, however, we meet with ungrammatical or awkward constructions in it, which detract from its general literary excellence.

CORRESPONDENCE.

Dead Black for Metal Work.

To the Editor of the CHEMICAL NEWS.

SIR,—You would be doing a good many Riflemen a service if you would inform us of the best method of staining the sights of our rifles black. These sights are black when new, but after a little wear become bright and it is then difficult when the sun shines upon them to take so correct an aim. Some persons rub a little Prussian blue on them, but this is a dirty plan, and rubs off on our hands and clothes. Some method of giving them a permanent dead black stain would be the thing.

I am, &c.

A RIFLEMAN.

Chemical Notices from Foreign Sources.

1. MINERAL CHEMISTRY.

Transformation of Nitrates into Ulmates.—

M. Paul Thénard put some solution of glucose and some nitrate of baryta into a tube and heated them to 200°. He obtained thereby a colourless liquid and a white powder, which, when exposed to the air, browned and blackened or took the colour of the ulmic compounds. This seemed in contradiction to the already known fact that in the soil ulmates are converted into nitrates. But a further investigation has convinced M. Thénard that both processes go on simultaneously in the earth. That on the surface of the ground ulmates are converted into nitrates, and in the subsoil nitrates are changed into ulmates. Iron, according to the author, appears to be the carrier of the oxygen, which effects the transformation.

Comparative Action of Nitrates and Ammonia on Vegetation.—

We may here quote the results of some experiments by Ville (*Comptes-Rendus*, t. li. p. 874), which go to prove that nitrate of potash has a much greater influence on the growth of plants than ammonia—that in fact the nitrogen of nitrates is much more assimilable than the nitrogen of ammonia.

Solubility of Different Forms of Silica in Potash.

—Rammelsberg made a series of experiments (*Bericht der Akad. d. Wissenschaft zu Berlin*, 1860, s. 758) to ascertain whether a strong potash ley afforded a means of distinguishing between quartz and opal. Fuchs had already made known the fact that while quartz is attacked with difficulty by a potash solution, some opals are easily dissolved by it. Rammelsberg's object was to see whether the amount of each could be estimated by the same means. In this respect the experiments do not seem to be conclusive. Some opals dissolve very slowly; and the rate at which the different forms of silica are dissolved is not so constant as to allow us to separate in this way one form from another.

Phosphate of Cobalt and Zinc.—Gentele has analysed (*Journ. für prakt. Chem.*, Bo. lxxxii., s. 58) some pigments used for colouring porcelain. One was a very pure dark blue, of extraordinary beauty, in which the author found CoO 15.09, ZnO 18.24. The flux contained PbO 26.85, SiO_2 24.46, Al_2O_3 1.24. The blue was perfectly dissolved from the flux by acetic acid, and this fact led the author to the discovery that the cobalt and zinc oxides were in combination with phosphoric acid. The colour may be made in the following way: to a solution of ordinary phosphate of soda is added first a solution of sulphate of zinc, and then of sulphate of cobalt. A green precipitate falls which turns blue on the addition of more sulphate of cobalt. A certain amount of phosphate of soda must, however, remain in excess. The blue precipitate becomes green again if more sulphate of zinc is added, and again blue on the addition of the cobalt solution. The supernatant liquid remains red. The washed precipitate is a dark blue with just a suspicion of red, but after ignition it is a pure blue. An analysis of the precipitate dried at 30° gave:—

Zinc oxide	41.95
Cobaltous oxide	12.97
Phosphoric acid	33.25
Water	12.06

which gives the compound $3(3\text{ZnO} \cdot \text{PO}_5 + 3\text{HO}) + 3\text{CoO} \cdot \text{PO}_5 + 3\text{HO}$.

II. ORGANIC CHEMISTRY.

Peculiar Product from some Coal Oils.

MM. Riche and Baidy have examined with some minuteness (*Bulletin de la Société Chimique de Paris*, No. I. p. 14) a yellow body obtained when certain coal oils are treated with nitric acid. As first obtained, it is a solid black mass smelling strongly of nitrobenzine, and staining the skin yellow like picric acid. Purified by repeated filtration through moistened bibulous paper, to remove the black viscid oil, crystals of a deep yellow colour are procured which no longer smell of nitrobenzine. Their analysis gave results which led to the formula $\text{C}_{12}\text{H}_9\text{N}_3\text{O}_{12}$. On drying the crystals in a vacuum for four days, they broke up and became reddish. The analysis of them in this condition gave the composition $\text{C}_{12}\text{H}_9\text{N}_3\text{O}_{10}$, showing that the crystals had lost two atoms HO . The exact nature of this body the authors have not been able to determine. It gives precipitates with salts of most of the metals, and the compounds formed with lead and silver easily crystallize. The silver compound blackens in the light, and explodes with violence at a very slight elevation of temperature.

Reactions of Cinchonine.—A small quantity of a solution of yellow prussiate of potash added to a solution of a salt of cinchonine gives a precipitate. If the mixture be carefully heated for some time the precipitate redissolves, and when the solution cools, numerous gold-yellow crystals deposit. These crystals Dr. Seligsohn found (*Medicin Centralzeit.*, 1861. Stück 17, s. 129) to be composed of one atom cinchonine and one atom ferrocyanogen. The solution of the cinchonine salt

must not be acid, nor must it be warm when the prussiate is added. Cinchonine may be detected in the urine by this test. The urine must be evaporated to dryness on a water bath, the residue treated with alcohol, the alcoholic filtrate evaporated to dryness, and the residue from it dissolved in water. The solution of prussiate of potash is then cautiously added: the solution remains clear at first, but after a time the golden-yellow crystals appear on the sides of the test glass.

Green Dye from the Rhamnus Catharticus.

M. Charvin has discovered in the *Rhamnus Catharticus* a green dye which seems to bear a great resemblance to the Chinese green *Lo-kao*. It is prepared in following way:—The bark of the buckthorn is put into boiling water and after boiling some minutes the contents of the vessel, bark and water together, are turned into an earthen pan, and covered up that they may cool slowly. After some days the liquid, which is of a brownish yellow colour, is poured off, and clearlime water is added to it. It is then exposed in shallow dishes to air and light under the influence of which the colour soon changes to green, and a deposit of the same tint settles in the dishes. Carbonate of potash is afterwards added, which produces a green precipitate, the fluid taking a dark brown colour. When the precipitate has deposited, the fluid is poured off, and the precipitate collected on a filter and dried. Glénard in a report addressed to the Lyons Chamber of Commerce (*Bulletin de la Société d'Encourage.* November, 1860) has recommended the discoverer of this dye for the prize of 6000 francs offered by the Chamber in 1857, for a colour like the Chinese green extracted from a home plant.

ANSWERS TO CORRESPONDENTS.

*. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

*. All Editorial Communications are to be addressed to Mr. CROOKES, and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the *CHEMICAL NEWS*, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

J. C.—The distillate contains a good quantity of benzol. The method we gave was not intended to separate all, or even the greater part, of the benzol from the liquid, but merely to enable the relative values of different samples to be ascertained. That which yielded the most distillate below 212° being the best. You will find no work so good as the published volumes of the *CHEMICAL NEWS*.

G. F. A.—We are glad to hear from an old friend. Perhaps our opinions coincide with his, but we must not exercise authority too harshly.

Phœnix.—Wood may be stained to imitate rosewood by boiling 8 oz. of logwood in 3 pints of water until reduced to half. Apply it boiling hot three times, letting it dry between each. Afterwards put in the strokes with a camel hair pencil dipped in a solution of copperas and verdigris in decoction of logwood.

A Constant Reader.—Boil 1 oz. of logwood, $\frac{1}{2}$ oz. sumach, and 1 drm. of copperas in 4 pints of water.

Tasmanian Wheat.—The letter of our correspondent, "F.C.S." (in our last Number) was founded upon a misconception, resulting from a typographical error in Mr. W. Scott's Analysis of Tasmanian Wheat, published in No. 76. The figures showing the per-centages of Water, Ash, Nitrogenous Matter, and Carbonous Matter should have been printed in larger type than the rest, as should also the total "100.000" which is derived from these four parts alone.

R. J. Rutter.—Apply to the Secretary, at Burlington-house, Piccadilly.

Erratum.—P. 327, Article "Coloured Derivatives of Naphthalene," sixth and eleventh lines from bottom, for *nitrate* read *nitrite*.

THE CHEMICAL NEWS.

VOL. III. No. 79.—June 8, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Amounts of Lead contained in some Silver Coins, by Messrs. CHARLES W. ELIOT and FRANK H. STOREY.

(Continued from page 320.)

THE residue of gold and filter paper left after dissolving the carbonate of lead in nitric acid for the final lead determination, was in each case incinerated, and then fused with carbonate of soda before the blowpipe. A distinct button of metallic gold was obtained in every instance. The amount of gold obtained varied, however, to a very considerable extent: it was abundant in the American and Spanish coin, less was observed in the English and Mexican, and but little in the French coin or the American fine silver.¹

The silver of the French coin was undoubtedly prepared by the sulphuric-acid process,² in which the granulated alloy of silver, gold, copper, &c., is dissolved in hot concentrated sulphuric acid, and the silver subsequently precipitated, by means of metallic copper, from a somewhat diluted hot solution of the sulphate of silver thus obtained. Since ordinary commercial sulphuric acid³ is used in this operation, it is not strange that a portion of the lead with which it is contaminated should

be transferred to the silver. The lead salt is, perhaps, reduced by the metallic copper, in the manner so well described by Odling, in his Memoir "On the Reciprocal Precipitations of the Metals."⁴ It is, moreover, not impossible that some sulphate of lead may fall when the acid liquor is diluted, and become mixed with the precipitated silver. It is not credible, however, that the lead in the coin can have been derived from the copper used to form the standard alloy; for this supposition would imply that copper of very inferior quality had been employed. According to Karsten,⁵ copper which contains but one per cent. of lead is utterly unfit for manufacturing purposes, since it cannot be worked at any temperature. Now, the least amount of lead which we found in fifty cents' worth of American silver is 0.0253 gram. in 10 half-dimes of 1853. These 10 half-dimes contain 1.243 gram. of copper, and if this metal had been the source of the lead, it must have contained more than two per cent. of that impurity.

We are ignorant of the process of preparing silver which was in use at the British Mint in 1816. Perhaps the silver in the coin of that date was obtained by cupellation, and it is well known that silver so obtained almost always contains lead.⁶

The silver of the Spanish and Mexican coins was, doubtless, prepared by the American system of amalgamation.⁷ In this process, speaking in general terms, it would appear that silver is reduced by means of metallic mercury from a solution of chloride of silver in chloride of sodium, the amalgam which is formed being exposed meanwhile to the action of chloride of copper and perchloride of iron.

It is to be inferred from the investigations which we have cited, that, under these circumstances, the amalgam should be almost entirely free from any contamination with the more strongly electro-positive metals.⁸ It

¹ Besides the quantitative analyses recorded above, which have been made by this method, it should be mentioned, that in several qualitative experiments upon American coins, by the method as described or slightly modified, indications of lead were obtained in every instance. The same remark is true of two qualitative analyses,—one of a dime and another of a quarter-dollar,—made as follows:—the coin having been dissolved in nitric acid, and the insoluble black residue of sulphide of silver and a little gold separated by filtering, the solution was mixed with pure acetate of soda (see Fresenius, *op. cit.*, p. 428, § 163, i. 7) to prevent precipitation of chloride of lead in subsequent operations, and the whole heated over the water-bath; the silver was then precipitated with dilute chlorhydric acid, and the chloride of silver collected upon a filter and washed. Through the filtrate sulphuretted hydrogen was passed, the mixed sulphide of copper, lead, and gold collected upon a filter, treated with nitric acid, the acid solution, evaporated to dryness, the residue taken up with water, and the solution filtered; in the filtrate lead was detected, while the residue contained gold. With regard to this residue of gold, which has also been alluded to in the text, it should be remarked that it has been entirely neglected by many analysts who have published determinations of the amounts of gold in various silver coins. Indeed, it would appear as if the fact were not generally known to chemists, that nitric acid can dissolve a little gold (as it can platinum), when an alloy containing a small quantity of gold is treated with this acid (compare Mulder, *op. cit.*, p. 159). Another common error of analysts has been, to class as "gold" the dark-coloured residue which is usually left when silver coin is dissolved in dilute nitric acid. This precipitate does, in fact, generally contain a little gold, though in our own experiments by far the larger portion of this metal passed into solution in the nitric acid, and was separated at a subsequent stage of the analysis, as has been described; Gay Lussac (*Ann. Ch. et Phys.*, 1836, [2.] liii. 334) long ago showed that it is principally composed of sulphide of silver; to which fact attention has also been called by Mulder, in his excellent little treatise, just cited, p. 178.

² See D'Arctot in *Dingler's Polyt. Journ.*, 1828, xxviii. 3; from "Annales Mensuelles," *Mal*, 1827, p. 131. Also, "Ure's Dictionary of Arts," &c., 4th edition (Boston, 1853,) li. 543. Dumas's "Traité de Chimie Appliquée aux Arts," (Paris, 1833,) iv., 464.

³ D'Arctot, *Journ. für tech. u. kon. Ch.*, 1829, iv. 420.

⁴ *Quarterly Journal of the Chemical Society of London*, 1857, ix. 289.

⁵ In his "System der Metallurgie," (Berlin, 1832,) v. 245.

⁶ Authorities differ as to the amount of lead contained in crude cupelled silver ("lightened silver," *argent éclairci*, *Blacksilver*). According to Berthier ("Essais par la Voie Seche," Paris, 1848,) li. 724, it contains, on an average, only 1 per cent. of lead. Kerl ("Handbuch der metallurgischen Hüttenkunde," (Freiburg, 1855,) iii. 152), on the other hand, says that it contains from 5 to 10 per cent. of impurity; and according to Karsten ("System der Metallurgie," v. pp. 200, 201) Blacksilver contains, at least, 12 per cent. of lead, and often more, the proportion of lead to silver being entirely dependent upon the temperature of the cupelling furnace. Since such silver is unfit for use, it is refined by small portions, either by a second cupellation at a high heat, or by melting it in crucibles with saltpetre and borax, or some other flux (Kerl, *op. cit.*, iii. pp. 181 to 198); but it appears to be impracticable, or at least not advantageous in practice to remove the last traces of lead from the silver by either of these processes. (See Kerl, *op. cit.*, i. 224; or Plattner, "Probirkunst mit dem Löthrohre," (Leipzig, 1853,) p. 403).

⁷ Described by Humboldt in his "Essai Politique sur le Royaume de la Nouvelle Espagne," (Paris, 1811,) li. 558. See also Karsten, "Abhandlungen der phys. Klasse der Akad. der Wissenschaften zu Berlin," 1828, p. 1; and "Karsten u. Dechen's Archiv für Mineralogie," etc., 1829, i. 161; and again, *ibid.* xxv. 178, and in *Dingler's Polyt. Journ.*, 1852, cxxvi. 357. Compare the subsequent statements of Boussingault, *Ann. Ch. et Phys.*, 1832, [2.] li. 350.

⁸ In the European system of amalgamation, as practised at Freiberg, in Saxony, where the silver is reduced from its chloride by metallic

would seem, indeed, that the American system of amalgamation furnishes purer silver than is obtained by any of the other processes which are employed upon the large scale.

So far as concerns the occurrence of lead in the silver coin of our own country, it could probably be mainly, if not altogether, avoided, by employing zinc free from lead, such as is manufactured in Pennsylvania, as we have already described in our Memoir.

It is interesting to observe that the occurrence of lead in some of the silver coins of the ancients⁹ has been thought to indicate "that the process of separating lead and silver was less perfectly executed in the ages of antiquity than is at present the case."¹⁰ Yet, in none of the recorded analyses of ancient silver coins to which we have had access have we been able to find that any one has detected so large an amount of lead in these coins, as we have shown to occur in American fine silver of the year 1860, if we except a single analysis by Professor Draper, who found nearly 3 per cent. of lead in a silver coin of Hadrian. The greatest per-centage of lead observed by Brüel (the author of the remark just quoted) was only 0.12. It is probable, however, that the methods of analysis—none of which are recorded—employed for separating lead from silver by the chemists to whose labours we have referred, were less delicate than the process which we have ourselves made use of.

It is interesting also in this connection to observe, that the American system of amalgamation, which at one time—before its peculiar fitness for the circumstances of the case in which it is employed had been recognised—was so frequently criticised by European metallurgists, affords silver which is less strongly contaminated with lead, and is probably purer in other respects than is produced by any other process of manufacture.—*Proceedings of the American Academy of Arts and Sciences.*

On the Preparation of Hydriodic and Hydrobromic Ethers by the Substitution of Amorphous for Normal Phosphorus, by M. J. PERSONNE.

In a paper published in 1857,¹¹ I showed that red, or amorphous phosphorus, behaved with various chemical agents in the same manner as normal phosphorus, but that its reactions were less intense. These facts have led me to substitute amorphous for normal phosphorus in the preparation of various chemical compounds, the production of which has hitherto been attended with some difficulty, on account of the energy of the reaction between the normal phosphorus and the bodies acting upon it. I intend treating of hydriodic and hydrobromic ethers, and also hydriodic acid. The results obtained are so clear and so easily practised, that by publishing

iron instead of quicksilver, a similar degree of purity in the silver is not to be expected. (For analyses of such silver, "Tellerhäuser," see Karl, *op. cit.*, i. 234.)

⁹ Walchner, Schwigger's *Journal für Ch. u. Phys.*, 1827, ii. pp. 204, 205. J. W. Draper, *Silliman's American Journal of Science*, 1856, [1.] xxix. 160. *Berzou's Journal de Pharmacie*, 1859, xxv. 503. Brüel and Hausmann, "Karsten u. Dechen's Archiv für Mineralogie," &c., 1844, xviii. 505; also in *J. pr. Chem.*, xxx. 334. J. W. Mallot, *Transactions of the Royal Irish Academy*, 1853, xxii. 319. Brüel and Hausmann refer also to earlier determinations of lead in antique coins by Klapproth (probably in his "Beiträge zur chemischen Kenntniss der Mineral-Körper," 1795-1798, B. vi.), and by Goebel (doubtless in his brochure, "Ueber den Einfluss der Chemie auf die Ermittlung der Völker der Vorzeit," &c., Erlangen, 1842); but they do not indicate precisely where these analyses are to be found, nor have we any means of ascertaining this point.

¹⁰ Brüel and Hausmann, *loc. cit.*, *Archiv.* p. 509; *J. p. Chem.*, p. 338.

¹¹ *Comptes-Rendus*, July, 1857.

them I hope to render a real service to chemists, who so frequently have occasion to employ these compounds, especially in the preparation of compound ammonias and organo-metallic radicals.

Hydriodic Ether.—If the preparation of hydriodic ether has been facilitated by the improvements recently introduced by MM. E. Kopp, F. Marchand, Soubeiran, and lastly by M. Hofmann, the process is, notwithstanding, just as tedious, since it is always necessary to add the phosphorus very gradually to the alcoholic solution of iodine. Thus, according to M. Marchand, it requires three days to prepare the quantity of hydriodic ether furnished by 680 grammes of iodine and 200 grammes of phosphorus, that is to say, about 800 grammes. If, on the contrary, amorphous phosphorus is employed, the operation becomes exceedingly easy and surprisingly rapid. The following is the mode of operating:—

In a tubulated retort, fitted with a receiver, place 30 grammes of powdered amorphous phosphorus, and 120 grammes of absolute alcohol; then add, at two separate times, and with an interval of a few minutes, 100 grammes of iodine. Place the retort on a sand-bath, and proceed with the distillation until the distilled liquor ceases to be precipitated by water. The distilled product is scarcely coloured by the iodine, and if washed with water containing some drops of solution of potash it becomes perfectly colourless. The weight of the obtained product, when washed to expel the alcohol, corresponds exactly to the theoretical quantity. Thus, from 100 grammes of iodine I have obtained at one operation 125 grammes; at another 120 grammes; and at a third, 118 grammes—the theoretical quantity being 123 grammes.

In operating thus it requires less than a quarter-of-an-hour to prepare more than 150 grammes of hydriodic ether, and I feel assured that 1 kilogramme could be prepared in an hour. The quantity of amorphous phosphorus employed is larger than is really necessary, but no inconvenience results from this, because it remains in the retort, soiled, it is true, by the phosphoric and phosphorous acids, but requiring only washing in hot water to render it perfectly fit for another operation.

Hydrobromic Ether.—The preparation of hydrobromic ether with normal phosphorus and bromine is attended with even more danger than that of hydriodic acid,—so much so, indeed, that this method of preparing it has been almost discarded, and it has been principally obtained by treating alcohol by excess of bromine, and afterwards separating the hydrobromic ether by distillation from the bromal and ethylene and perbromated products. If, by this method, there is no danger, there is at least the inconvenience of losing a great quantity of bromine. The following is the method of operating with amorphous phosphorus:—

To 40 grammes of amorphous phosphorus, and from 150 to 160 grammes of absolute alcohol placed in a tubulated retort fitted with a receiver, add by degrees 100 grammes of bromine by means of a funnel tube, with a tapering and slightly recurved extremity immersed in the alcohol; the bromine must only be added by degrees, because of the energy of the reaction, which would cause a great part of the product to distil before the whole of the bromine had been added. It is advisable to place the retort in a cold water-bath, and to cool the receiver by a current of water. When all the bromine has been added to the liquid, remove the funnel, pour into the retort the small quantity of liquid which the temperature of the reaction has caused to pass into

the receiver, then proceed at once with the distillation, operating in the same manner as with hydriodic ether.

This operation is as easy and as little dangerous as the former, but it requires double the time. The theoretical quantity is obtained as nearly as possible; thus with 100 grammes of bromine I have obtained, at one operation, 122 grammes; at another, 115 grammes; and at a third, 120 grammes of product—the theoretical quantity being 136 grammes. This slight difference is occasioned by the unavoidable loss of bromine while adding it by degrees to the alcoholic liquid.

Hydriodic Acid.—Notwithstanding M. Deville's improvements in the preparation of gaseous hydriodic acid, my opinion is that it will be found to be more convenient and expeditious to prepare it with amorphous phosphorus. In fact, it is enough to place a sufficiently large quantity of amorphous phosphorus in a tubulated retort with a glass stopper, to cover it with a layer of water, and to add the iodine in adequate quantity, to obtain by the aid of a slight degree of heat a regular current of gas, perfectly free from iodine vapour.

Now that amorphous phosphorus is used for industrial purposes, and is easily procurable, I am persuaded that valuable results may be obtained by substituting it for normal phosphorus, in most of the reactions in which this body takes a part.—*Comptes-Rendus*.

TECHNICAL CHEMISTRY.

On the Chemical Composition of Cast Iron and Steel,
by M. FREMY.

(Continued from page 332.)

THE object of my previous publications on steel was to prove that the part played by nitrogen in acieration has been hitherto misunderstood, and that nitrogen is not only a very active cementation agent, but that it remains in the metallic compound. Hitherto, my papers have been almost exclusively dedicated to synthetical operations, and to experiments in the laboratory. In this communication it is my intention to prove by means of chemical analysis that steel ceases to exist when its nitrogen is expelled, and that all practical operations tend to confirm my previously published opinions touching the constitution of steel. I will first describe an analytical experiment which I think will satisfy the most exacting critics.

My former studies on nitrides have convinced me that hydrogen can carry off in the state of ammonia the nitrogen contained in nitride of iron; after establishing the fact that steels are nitrogenised, I concluded that hydrogen also could decompose steel, and re-convert it to the state of iron. Experience has confirmed this surmise. I present to the Academy a plate of cast steel of the finest quality, a part of which only has been submitted at red heat to the action of hydrogen. The operation occupied three hours, and during the whole of this time the steel constantly disengaged ammoniacal vapours, and probably other nitrogenised alkalies, the vapour of which smells like burnt horn. The portion of the specimen submitted to the action of hydrogen, and which has parted with its nitrogen, lost entirely the properties of steel, being transformed into soft and wonderfully malleable iron, which tempering had no power to modify; while the other portion of the plate which had not been de-nitrogenised preserved all the characteristics of steel.

In this transformation steel loses about 1 per cent. of

its weight. This diminution in weight probably represents all the substances existing in steel besides those found in iron, for ammonia induces the conversion of the carburetted compounds into cyanide of ammonium. The constitution of steel may be considered, then, as established both by synthesis and analysis. Iron is converted into steel by nitrogenising it in presence of carbon; and steel is re-converted to iron by withdrawing the nitrogen by hydrogen. The results from the experiment here described may be easily foreseen. To preserve the valuable properties of steel it should be kept from contact with any substance capable of depriving it of nitrogen.

In manufacturing fire-arms from cast-steel, the action which the gases proceeding from the detonation of the powder may exert upon steel should be taken into account. It is probable that nitrogen inheres with greater tenacity in some kinds of steel than in others; for example, it is not so difficult to expel it from puddled as blistered steel. By the action of hydrogen on steel we get a soft iron which may perhaps be usefully applied to the construction of certain magnetic apparatus.

Several members of the Academy have asked me to examine a very malleable iron obtained from a modification of steel; probably this metal is identical with that produced by expelling the nitrogen from steel, as in the preceding experiment.

After decomposing steel by depriving it of nitrogen by means of hydrogen, it seems interesting to inquire to what extent the metal is susceptible of modification by the action of combustion gases. The influence of carbonic acid ought to be carefully studied in this respect. I find that steel quickly loses its carbon by the action of carbonic acid, which reduces it to the state of nitrogenised iron; an excess of carbonic acid burns the metal completely. All steel-workers know that this body is liable to destruction; as they express it, "it burns itself" when too long exposed to the action of the gaseous products of combustion. This modification is evidently due to the oxidising compounds carrying off the carbon. All the methods employed in manufactories to recover burnt steel have probably for their object to restore the carbon taken from it by the gases. I believe, however, that this restitution is always incomplete, and that a steel thoroughly burnt can never be restored; I even think that, under certain conditions, heat alone suffices to make a physical modification in steel, which tempering in a fatty body would to some extent destroy. The alterations which steel undergoes when "burnt,"—that is to say when carelessly heated,—may be due either to chemical decomposition or simple physical modification.

I turn now to the examination of some practical, established facts, tending to confirm my opinion in reference to the constitution of steel. The supporters of the old theory of acieration, who still consider steel as a carburet of iron, will deny the possibility of producing a nitride in a medium containing nothing but iron and carbon. I am about to show that in cementation cases, far from being submitted to carburising influences exclusively, the iron can derive nitrogen not only from the gases circulating in the cases, but also from the carbon employed.

At a recent meeting of the Academy, M. Boussingault stated, with an authority acquired by long observation in steel-works, that nitrogen always penetrates and circulates in the cementation cases, and consequently that it may nitrogenise the iron. Mr. Saunderson's important work, which I have already had occasion to cite several times before the Academy, also shows that cementation

is produced with difficulty except when nitrogen is present. M. Caron's observations on the rapid way in which steel is produced by cyanide of ammonium, and the formation of this body in cementation cases, prove equally that steel is, at the time of its production, in an ammoniacal and consequently nitrogenising atmosphere. The inference is that the gases and vapours circulating in the cases can nitrogenise iron.

I ought to inquire whether, apart from atmospheric nitrogen, the iron cannot also derive this element from the solid compounds found in cementation cases. I have ascertained that carbon itself may become a very active nitrogenising agent. The result of my experiments is that a nitrogenised organic matter when calcined leaves a carbonaceous residue, almost always containing nitrogen; exposed to the action of hydrogen it disengages abundance of ammonia. This production of ammonia cannot be attributed to the presence of cyanides in the residue, since the same result happens with an ash which has undergone washings in acids. Wood charcoal itself, after prolonged calcination at red heat, which gets rid of its alkalis and cyanides, still disengages ammonia under the action of hydrogen.

(To be continued.)

On the Application of Cyanide of Potassium in Soldering Metals, by Dr. AUGUST VOGEL.

In the operation of soldering metals, it is very essential to keep the metallic surfaces to be united clean and bright, so that the solder may adhere firmly when in a melted condition. For the purpose of protecting the metallic surfaces from the oxidising action of the atmosphere, certain fusible substances are usually rubbed on with the solder, and immediately form a thin layer over the surface of the metal. These substances produce, however, not only a protective, but also a reducing action. In practice it is sought to ensure these two essential conditions in the choice of the substances generally employed, viz., for soft soldering, resin, turpentine, olive oil, powdered sal-ammoniac, mixed either with oil, or with tallow and resin, or a very concentrated solution of chloride of zinc. For hard soldering borax, or a melted mixture prepared from borax, potash and common salt, and, in the special case of iron, pounded green glass is generally used.

It is well known that the substances above mentioned fulfil to a greater or less extent these two conditions of soldering, viz., deoxidation and protection of the metal from the atmosphere. A material possessing these two qualifications in the highest degree, would, of course, best effect this purpose. As the result of a great number of experiments, I have come to the conclusion that the ordinary commercial quality of cyanide of potassium possesses decided advantages in this respect over all other substances. It melts very readily, covering the surface of the metal with a very efficient protective layer, and at the same time is known to exert a strong reducing action, a property which has gained for it many important applications both in technical and analytical chemistry.

Cyanide of potassium will be found particularly useful when the surfaces to be soldered cannot be thoroughly brightened. It is difficult, and sometimes impossible, to solder metals when their surfaces are at all corroded, or when they are incapable of bearing the high temperature necessary in this operation, with the ordinary agents, on account of their inferior reducing power; but

cyanide of potassium, from its extraordinarily energetic action, is able to deoxidise all rusty particles standing in the way of the perfect union of the solder with the metal.

The mode of applying the cyanide of potassium in soldering is the same as with borax. Some powdered cyanide of potassium is kept ready at hand in a well-closed glass bottle; and sprinkled over the metallic surface after it has been slightly moistened. In some cases of soldering at very high temperatures, which, by practice, are soon ascertained, it will be found expedient to use a compound of borax and cyanide of potassium, for the purpose, on the one hand, of increasing, by this addition, the small reducing power of the borax, and, on the other hand, of diminishing the volatilising tendency of the cyanide of potassium.

Another reason for preferring the employment of this agent is, that during the operation no corrosive vapours capable of acting on the soldering tools are generated, as is the case with chloride of zinc.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, May 16, 1861.

Dr. A. W. HOFMANN, F.R.S., President, in the Chair.

Mr. PERKIN delivered the following discourse "*On the Colouring Matters, Derived from Coal-Tar.*"

The subject which I have the honour of bringing before you this evening is one which is remarkable for the very rapid progress it is making. Previous to the year 1856 the colouring matters derived from coal-tar products were practically unknown; but owing to recent discoveries they are now enumerated amongst the most important colouring matters employed. It is not my intention to enter into a discussion respecting the numerous patents that have been taken out for the production and application of coal-tar colours, nor yet to enter into the practical details of their manufacture; but to notice more particularly their various Chemical characters, and also to speak of the methods employed for their application to the arts. I shall endeavour to bring before you all the principal coloured products obtainable from the derivatives of coal-tar—both those which are practically employed by the dyer and printer, as well as those which still remain as chemical curiosities only. I may mention that the latter class of compounds is more extensive than the former, and also that many of the colouring matters in it are likely to remain so, for reasons to be mentioned presently.

The coal-tar products which have, up to the present, yielded colouring matters are numerous. The following is a list of the most important, which are obtained either directly or indirectly from coal-tar; namely, aniline and its homologues, carbolic acid, chinoline or quinoline, naphthalene, and pyrole bases.

I will commence by speaking of the first and most important of these products, and its derivatives,—namely, Aniline.

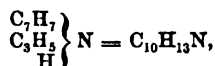
Aniline was discovered in 1826 by Unverdorben. The original method for its preparation was by digesting indigo with hydrate of potassium, and subjecting the resulting product to distillation. Aniline was also obtained from the basic oils of coal-tar; but the process which is now employed for its preparation is a remarkable instance of the manner in which abstract scientific research becomes, in the course of time, of the most important practical service. It was Faraday who first discovered benzole; he found it in oil-gas. After this it was

obtained by distilling benzoic acid with baryta, which result determined its formula, and was the cause of it being called *benzo's*. After this, Mansfield found it to exist in large quantities in common coal-tar naphtha, which is the source from which it is now obtained in very large quantities. Benzole, when studied in the laboratory, was found to yield, under the influence of nitric acid, nitro-benzole. Zinin afterwards discovered the remarkable reaction which sulphide of ammonium exerts upon nitro-benzole, converting it into aniline. And, lastly, Béchamp found that nitro-benzole was converted into aniline when submitted to the action of ferrous acetate. It is Béchamp's process which is now employed for the preparation of aniline by the ton. Had it not been for the investigations briefly cited above, the beautiful aniline colours now so extensively employed, would still remain unknown. When I discovered aniline purple, nitro-benzole and aniline were only to be met with in the laboratory; in fact, half-a-pound of aniline was then esteemed quite a treasure, and it was not until a great deal of time and money had been expended that I succeeded in obtaining this substance in large quantities, and at a price sufficiently low for commercial purposes.

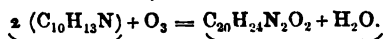
The colouring matters obtainable from aniline are numerous. They are the following:—Aniline purple, violine, roseine, fuchsine, alpha aniline purple, bleu de Paris, nitroso-phenylene, dinitraniline, and nitro-phenylene diamine.

Aniline Purple, generally known as Tyrian Purple, Mauve, Phenamine, Indisine, &c.—It has been known for many years that the hypochlorites react on aniline and its salts, producing a purple-coloured solution; in fact, hypochlorites are the distinguishing test for aniline; but nothing definite was known of this purple-coloured solution, it being simply stated that aniline produced with hypochlorites a purple-coloured liquid, but that this colour was very fugitive. As many very absurd statements have been made respecting the discovery of aniline purple, I will just briefly mention how it was that I first became acquainted with it.

In the early part of 1856 I commenced an investigation on the artificial formation of quinine. To obtain this base I proposed to act on toluidine with iodide of allyle, so as to form allyle-toluidine which has the formula



thinking it not improbable that by oxidizing this I might obtain the desired result, thus—



Allyle-toluidine.

Quinine.

For this purpose I mixed the neutral sulphate of allyle-toluidine with bichromate of potassium; but instead of quinine I obtained a dirty reddish-brown precipitate. Nevertheless, being anxious to know more about this curious reaction, I proceeded to examine a more simple base under the same circumstances. For this purpose I selected aniline, and treated its sulphate with bichromate of potassium. This mixture produced nothing but a very unpromising black precipitate; but on investigating this precipitate I found it to contain the substance which is now, I may say, a commercial necessity, namely, aniline purple.

The method adopted for the preparation of aniline purple is as follows:—Solutions of equivalent proportions of sulphate of aniline and bichromate of potassium are mixed, and allowed to stand till the reaction is complete. The resulting black precipitate is then thrown on a filter and washed with water until free from sulphate of potassium. It is then dried. This dry product is afterwards digested several times with coal-tar naphtha until all resinous matter is separated, and the naphtha ceases to

be coloured brown. After this, it is repeatedly boiled with alcohol to extract the colouring matter. This alcoholic solution when distilled leaves the colouring matter in the bottom of the retort as a beautiful bronze coloured substance.

The aniline purple prepared according to the process just described, although suitable for practical purposes, is not chemically pure. If required pure it is best to boil it in a large quantity of water, then filter the resulting coloured solution, and precipitate the colouring matter from it by means of an alkali. The precipitate thus obtained should be collected on a filter, washed with water until free from alkali, and dried. When dry it is to be dissolved in absolute alcohol, the resulting solution filtered, and then evaporated to dryness over the water-bath. Thus obtained, aniline purple appears as a brittle substance, having a beautiful bronze coloured surface, but if some of its alcoholic solution be evaporated on a glass plate, and viewed by transmitted light, it appears a beautiful bluish violet colour. If considerable quantities of an alcoholic solution of the colouring matter, containing a little water be evaporated to dryness, the surface of the colouring matter next to the evaporating dish when detached, often possesses a golden green appearance. Aniline purple is difficultly soluble in cold water, although it imparts a deep purple colour to that liquid. It is more soluble in hot water, but its hot aqueous solution when left to cool assumes the form of a purple jelly. It is very soluble in alcohols, though nearly insoluble in ether and hydrocarbons. Aniline dissolves it readily. In properties it seems to be slightly basic, as it is more soluble in acidulated than in pure water. Alkalies and saline substances precipitate it from its aqueous solution as a dark purplish-black powder. Bichloride of mercury precipitates it in a very finely divided state. A little of this precipitate, which appears to be a double compound of chloride of mercury and colouring matter, when suspended in water and viewed by transmitted light, appears of a blue or violet colour. If a small quantity of hydrate of potassium or sodium be added to an alcoholic solution of the colouring matter, it causes it to assume a violet tint, but without effecting any change in the colouring matter itself. Ebullition with alcoholic potash does not decompose it. Aniline purple dissolves in concentrated sulphuric acid forming a dirty green solution. This when slightly diluted assumes a beautiful blue colour. Excess of water restores it to its original purple colour. I have had a specimen of this colouring matter heated for an hour to 100 Centigrade with Nordhausen sulphuric acid without suffering decomposition, being restored to its original colour by means of water, and possessing precisely the same properties as it had before being subjected to this powerful agent. Hydrochloric acid acts upon it in the same manner as sulphuric acid. It is decomposed by chlorine, and also by fuming nitric acid. Bichloride of tin is without action upon it. Powerful reducing agents have a peculiar action upon this colouring matter, somewhat analogous to the action of reducing agents on indigo. An alcoholic solution of sulphide of ammonium, when mixed with an alcoholic solution of the colouring matter, causes it to assume a pale slightly brownish colour. This solution when brought in contact with the atmosphere instantly assumes its beauty and intensity of colour. An alcoholic solution of the colouring matter when mixed with a little protoxide of iron changes to a pale brown colour. This solution also becomes purple when exposed to the action of the atmosphere. Sulphurous acid does not affect the colour of this substance.

This colouring matter forms a remarkable compound with tannin. When an aqueous solution of the colouring matter is mixed with a solution of tannin, precipitation takes place. The precipitate thus formed, after having been well washed, no longer possesses the properties of the pure colouring matter. It is insoluble in water. Like

the pure colouring matter it dissolves in concentrated sulphuric acid, forming a dirty green liquid; but on adding an excess of water to this solution the new compound is precipitated unchanged. This compound is rather duller in colour than the pure colouring matter itself. Aniline purple when agitated with a little moist binoxide of lead is transformed into roseine. This colouring matter is remarkable for its intensity, a few grains of it colouring a considerable quantity of spirits of wine.

Violine.—This colouring matter, which is a product of the oxidation of aniline, was first obtained by Dr. David Price. He prepares it by heating an aqueous liquid containing two equivalents of sulphuric acid, and one equivalent of aniline to the boiling point, and then adding one equivalent of binoxide of lead, boiling the mixture for some time and filtering it whilst hot. The filtrate, which is of a dark purple hue, is boiled with potash, both to separate the excess of aniline, and also to precipitate the colouring matter. When all the free aniline is volatilised, the residue is thrown on a filter and slightly washed with water, and then dissolved in a dilute solution of tartaric acid. This solution after filtration is evaporated to a small bulk, re-filtered, and then precipitated by means of an alkali. Thus obtained, violine presents itself as a blackish purple powder, which when dissolved in alcohol, and evaporated to dryness, appears as a brittle, bronze-coloured substance, similar to aniline purple, but possessing a more coppery-coloured reflection. It is more insoluble in water than the preceding colouring matter; it is very soluble in alcohol, but insoluble in ether and hydrocarbons. These solutions possess a colour somewhat similar to that of the field violet. Concentrated sulphuric acid dissolves it, forming a green solution, but excess of water restores it to its original colour. Like aniline purple, reducing agents deprive it of its colour, but which is restored by the action of the atmosphere. Tannin produces an insoluble compound with it, when agitated with a small quantity of binoxide of lead, it is converted into aniline purple; excess of this re-agent changes it into roseine.

Roseine.—This substance nearly always accompanies aniline purple, though in very small quantities. It was first noticed publicly, by C. Greville Williams, and afterwards by Dr. David Price. Williams used manganates for its preparation, but Dr. David Price prepared it by means of binoxide of lead. His process is as follows:—To a boiling solution of one equivalent of sulphate of aniline, two equivalents of binoxide of lead are added, and the mixture boiled for a short time. The rose-coloured solution is then filtered, and the filtrate evaporated to a small bulk, which causes a certain amount of resinous matter to be separated. This evaporated solution is then filtered, and the colouring matter precipitated by means of an alkali, it is then collected on a filter, slightly washed, and then dried. The colouring matter thus prepared, readily dissolves in alcohol, forming a fine crimson-coloured liquid, which when evaporated to dryness, leaves the colouring matter as a dark, brittle substance, having a slightly metallic reflection. It is much more soluble in water than either aniline purple or violine, but like them, it is insoluble in hydrocarbons, and is more soluble in acids than in neutral liquids. Concentrated sulphuric acid dissolves it, forming a green solution; excess of water restores it to its original colour. It forms a compound with tannin, and is also deodorised, or nearly so, by powerful reducing agents.

The three colouring matters just mentioned, namely, aniline purple, violine, and roseine are evidently closely allied, for they have nearly the same properties. They are all formed under similar circumstances, namely, by the action of oxidising agents in the presence of water; they are all slightly soluble in water, though as the shade of colour becomes redder, so their solubility increases; alkalis precipitate them from their aqueous solutions;

concentrated sulphuric acid dissolves them, forming green solutions which excess of water restores to the original colour of the colouring matters; powerful reducing agents deprive them of their colour or nearly so; but it is again restored by the influence of oxygen; and, lastly, tannin forms insoluble compounds with them all.

Fuchsine or Magenta.—This beautiful product, which is often improperly called rosine, is a member of an entirely different series of compounds from the foregoing, being formed under very different circumstances, and possessing very different properties. This colouring matter was first observed by Natanson, in 1856, when studying the action of chloride of ethylene on aniline, and afterwards, shortly before it was practically introduced into the arts, by Dr. Hofmann, when preparing cyantriphenyle-diamine by the action of bichloride of carbon on aniline. It was M. Verguin who first brought it forward as a dyeing agent, and who, I believe, taught manufacturers how to prepare it on the large scale. Fuchsine is invariably formed at a temperature ranging from 170 to 190 Centigrade. It is produced from aniline by the action of reducible chlorinized, brominized, iodized, or fluorized substances, as well as by weak oxidizing agents. The substances used for its preparation on the large scale are perchlorides of tin and of mercury, and the nitrates of mercury. It has also been prepared with bichloride of carbon.

Preparation of Fuchsine by the Action of Bichloride of Tin on Aniline.—Aniline combines with bichloride of tin, evidently producing a double compound. This product is a white substance, and may be prepared by adding to aniline, bichloride of tin in the anhydrous state or dissolved in water. Anhydrous bichloride of tin combines with aniline with great energy to form this compound. To prepare fuchsine from this double compound it is necessary that it should be free from water, or nearly so; therefore anhydrous bichloride of tin is generally employed for its preparation. The process adopted is as follows:—Anhydrous bichloride of tin is slowly added to an excess of aniline, the mixture being constantly stirred, and the pasty mass thus formed gradually heated. As the temperature increases it becomes quite liquid and also brown in colour. As soon as the temperature nearly approaches the boiling point of the mixture it rapidly changes to a black-looking liquid which when viewed in thin layers presents a rich crimson colour. This is kept at its boiling point some time, and then well boiled with a large quantity of water. By this means the principal part of the colouring matter is extracted together with considerable quantities of hydrochloride of aniline. The residue is a solid of brown colour and contains considerable quantities of tin in the form of a proto-compound. The aqueous solution of the colouring matter and hydrochlorate of aniline is then boiled, so as to volatilize any free aniline it may contain, and then saturated with chloride of sodium. The chloride of sodium causes the colouring matter to separate as a semi-solid, pitchy substance of a golden green aspect, while the hydrochlorate of aniline remains in solution. The colouring matter thus obtained may be further purified by digestion with benzole which dissolves out a certain amount of resinous matter.

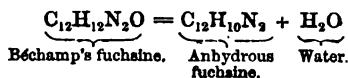
Preparation of Fuchsine by the Action of Nitrate of Mercury on Aniline.—When protonitrate of mercury is left in contact with aniline for some time it forms a white pasty mass; but when carefully heated to 170° or 180° Centigrade it reacts upon it, forming a brown liquid, which gradually changes till of a dark crimson colour. At the same time, the whole of the metal of the mercury salt collects at the bottom of the vessel the experiment is conducted in. This product, when separated from the metallic mercury and allowed to cool, becomes semi-solid, being filled with crystals of nitrate of aniline. To purify this product it is best to dissolve out the nitrate of aniline it contains in a small quantity of cold water, and then to boil the remaining product several times with fresh

quantities of water until the principal of the colouring matter is extracted, and filter the resulting aqueous solutions while hot. On cooling the solutions will deposit the colouring matter as a golden-green tarry substance, from which benzole separates a small quantity of a brown impurity, leaving the colouring matter as a brittle solid.

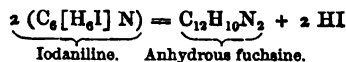
I have briefly described the above processes because they may to some extent be regarded as types of most of the methods employed for the production of this colouring matter, the first representing its formation by the action of reducible chlorides upon aniline, and the latter by the influence of weak oxidising agents.

Fuchsine is undoubtedly an organic base, and a more powerful one than is generally supposed. The product obtained from aniline by means of bichloride of tin is hydrochlorate of fuchsine, and that obtained by the oxidising action of nitrate of mercury is the nitrate of fuchsine. My reason for stating this is, that on examining the colouring matter obtained by chloride of tin I found it to contain large quantities of combined hydrochloric acid, and when nitrate of mercury was used considerable quantities of combined nitric acid; therefore I conclude that the former is the hydrochlorate and the latter the nitrate.

Fuchsine is separated from its salts by precipitation with a small quantity of ammonia. When freshly precipitated, fuchsine is a red bulky paste which, when dry, contracts, forming a purplish red powder. It is difficultly soluble in water, though tolerably soluble in alcohol; it is not soluble in ether or hydrocarbons; a small quantity of hydrochloric acid causes it to dissolve freely in hot water, but an excess of either hydrochloric acid or sulphuric acid dissolves it, forming a brownish yellow liquid from which ammonia separates it unchanged. By this reaction it may be distinguished from rosine, which dissolves in strong sulphuric acid, producing a green liquid. Caustic alkalies, or ammonia in excess, partially precipitate fuchsine from its salts; but, at the same time, dissolve a considerable quantity of it, forming nearly colourless liquids. Acetic acid added to these alkaline solutions restores the colour of the fuchsine; and if the liquids are concentrated, the base precipitates as a red, flocculent substance. An alcoholic solution of fuchsine when evaporated to dryness, leaves the colouring matter as a brittle mass, having a remarkably beautiful golden green metallic reflection. By transmitted light it has a red colour. M. Béchamp has analysed carefully-prepared specimens of fuchsine, and found it to have the formula $C_{12}H_{12}N_2O$. This hydrochlorate he found to contain an amount of hydrochloric acid corresponding with the formula $C_{12}H_{12}N_2O, HCl$. He also examined the hydro-chloro-platinate, which is a purple precipitate; it has the formula $C_{12}H_{12}N_2O, HPtCl_3$. The existence of oxygen in this base is remarkable, because in many instances it is produced from agents which do not contain a trace of oxygen, as, for example, bichloride of tin and aniline. The only way I can account for the presence of oxygen in the product analysed is, that it was a hydrate; thus:—



This is, perhaps, to some extent confirmed by an experiment I made with iodaniline. I find that iodaniline, when heated, yields fuchsine. This change can only be expressed thus:—



But supposing the fuchsine examined by M. Béchamp to have been a hydrate, it is remarkable that its hydrochlorate and, more particularly, its hydro-chloro-platinate should also be hydrates. But as our knowledge of this

body is as yet but scanty, we must wait for the accumulation of facts before we can form any fixed opinion respecting its constitution. The compounds investigated by M. Béchamp appear to be uncrystallisable. Some time back, I had upward of 100 gallons of a hot aqueous solution of this colouring matter, which had been prepared by means of nitrate of mercury. This solution, on standing until cold, deposited a considerable quantity of colouring matter in the form of small octahedra, having a most beautiful green metallic reflection. Reducing agents decolorise fuchsine, but the oxygen of the atmosphere restores it to its original colour. If an alcoholic solution of fuchsine be left in contact with sulphide of ammonium until decolorised, or nearly so, and then exposed to the atmosphere, its colour is immediately restored; but if left to stand for several days and then subjected to the atmosphere, several hours elapse before it assumes its original colour. Like aniline purple, fuchsine is a very intense colouring matter. Tannin precipitates both fuchsine and its salts, forming difficultly soluble substances. Bichloride of mercury also precipitates this base and its salts, forming double compounds. When preparing fuchsine by means of bichloride of tin, or more particularly when using nitrate of mercury, there are two other colouring matters produced, one possessing an orange colour, and the other a purple hue. It is on account of the presence of the former that some samples of commercial fuchsine possess a scarlet character of colour. Of this orange colouring matter scarcely anything is known. It is not precipitated from these salts by means of alkalies. Of the purple colouring matter little also is known. It is formed at the same time as fuchsine, but in very small quantities, and from which it is difficult to separate perfectly. Its properties are similar to those of fuchsine. It dissolves in concentrated sulphuric acid, forming a reddish-brown solution, from which part of the colouring matter is separated by dilution with water. Hydrochloric acid dissolves it, forming a dirty yellowish-red liquid.

Bleu de Paris.—This is yet another colouring matter produced under circumstances similar to those which give rise to fuchsine. MM. Persoz, V. de Luynes, and Salvétat give the following account of its preparation and properties:—"9 grammes of bichloride of tin and 16 grammes of aniline, heated for 30 hours to a temperature of about 180 Centigrade, in a sealed tube, produce neither a red nor a violet, but a very pure and lively blue. This blue, which resists acids, darkens in colour by alkalies, but passes to a groselle violet when submitted to this agent in a concentrated state. It preserves its beauty of colour by artificial light, and it dyes animal fibres of a shade whose beauty leaves nothing to be desired." I repeated the above experiment twice, and was inclined to give up in despair, for instead of a fine blue I obtained nothing more than a dirty green product. But from this unpromising product I at last succeeded in obtaining this beautiful blue colouring matter, and found it to possess all the properties mentioned above. MM. Persoz, V. de Luynes, and Salvétat, have lately given a more particular account of this colouring matter. They describe it as crystallising from the alcoholic solution in the form of fine needles, having an aspect similar to that of ammoniacal sulphate of copper. It is soluble in water, alcohol, wood-spirit, and acetic acid; but insoluble in ether and bisulphide of carbon. With concentrated sulphuric acid it forms an amber-coloured solution which water converts into a magnificent blue liquid. Strong nitric acid decomposes it, chromic acid precipitates it from its aqueous solutions without decomposition; chlorine destroys it; sulphurous acid does not decolorise it. I found that sulphide of ammonium is also without action upon it. It is precipitated from its aqueous solutions by alkalies and saline compounds.

Aniline Green, or Emeraldine.—Most chemists who have worked with aniline in the laboratory must have noticed

the peculiar green coloured-substance which forms on the outside of the various species of chemical apparatus that have been standing in the vicinity of any quantity of this body. This product is aniline green. Aniline green has been known for several years. It may be formed by various processes. One process consists in oxidising aniline with chloric acid. This is effected by mixing a hydrochloric acid solution of aniline with chlorate of potassium. It may also be obtained by oxidising a salt of aniline with perchloride of iron. Obtained by either of these processes it presents itself as a dull green precipitate which, when dried, assumes an olive-green colour. It is insoluble in water, alcohol, ether and benzole; sulphuric acid dissolves it, forming a dirty purple-coloured solution from which it is precipitated unchanged by water. With alkaline solutions it changes to a deep colour somewhat similar to indigo; but acids restore it to its original colour. The colour of aniline green is much enlivened by the presence of an excess of acid; but, unfortunately, as soon as this acid is removed it passes back to its normal colour.

The bases toluidine, xylydine, and cumidine yield colouring matters under the influence of oxidising agents; and also when submitted to the action of reducible chlorides at high temperatures, analogous to those obtained from aniline under similar circumstances; but the results generally are not so good, the colour of the products becoming tinged with brown as the bases get higher in the series.

Nitroso-Phenylene.—This remarkable body is obtained by the action of nascent hydrogen on an alcoholic solution of di-nitro-benzole. It is represented by the formula $C_6H_5N_2O$. This body is almost insoluble in water, but soluble in acids, and in alcohol, producing crimson-coloured solutions, but its colour is not nearly so brilliant as that of fuchsine. It seems gradually to decompose when kept. I have not tried any experiment with it as regards its dyeing properties.

Dinitraniline.—Dinitraniline is obtained by decomposing dinitro-phenyle citraconamide by means of carbonate of sodium. When pure, it crystallizes in yellow tables. It dissolves very sparingly in water, producing a yellow liquid. It has the formula $C_6H_5(NO_2)_2$. It does not combine with acids or alkalies, although it appears to be more soluble in acidulated than in pure water. Silk can be dyed yellow with dinitraniline.

Nitrophenylene-diamine, or Nitrazophenylamine.—Dinitraniline, when submitted to the action of sulphide of ammonium, changes into this beautiful base, which crystallizes in needles of a red colour, somewhat similar in appearance to chromic acid. It dissolves in water, forming a yellow or orange-coloured solution like that of bichromate of potassium. Alcohol and ether dissolve it freely. It is not at all similar to fuchsine in its properties, as it forms salts of yellowish brown or grey colour; whereas fuchsine forms crimson ones. I mention this, because nitrophenylene-diamine has been spoken of as having properties similar to those of fuchsine. This base possesses the power of dyeing silk a very clear golden yellow colour.

Picric or Dinitrophenic Acid.—This beautiful acid was discovered as early as 1788, by Hausmann. It may be obtained by the action of heated nitric acid on a great variety of substances; the following are the names of some of them:—indigo, aniline, carboic acid, saligenine, salicylic and salicylic acids, salicin, phlorizin, cumarin, silk, aloes, and various gum resins. It is now prepared for commercial purposes from carboic acid, and also from certain gum resins. I have prepared it from carboic acid on the large scale, in the following manner, with success:—As strong nitric acid acts very violently when brought in contact with carboic acid, I have found it best to use an acid having a gravity less than 1.3, so as partially to convert the carboic acid, and afterwards to boil it in stronger

acid to change it into picric acid. On diluting the acid solution the impure picric acid precipitates. To further purify this it should be crystallized from boiling water. When preparing this product for commercial purposes it is advantageous to let all the nitrous fumes formed in its preparation, together with a certain amount of atmospheric air, to pass over a fresh quantity of carboic acid. This will absorb them, and at the same time be converted into nitro- or dinitrophenic acid, and consequently diminish the quantity of nitric acid required for its manufacture. When preparing picric acid from carboic acid there is always a quantity of a yellow, resinous matter produced, and at times a considerable quantity of oxalic acid. The latter is always produced when the acid which is used to finally convert the carboic acid is too weak, for then it rapidly decomposes the picric acid, yielding carbonic and oxalic acids. Picric acid, when pure and dry, is of a light primrose-yellow colour, crystallizing in strongly-shining lamina. It possesses an extremely bitter taste, and dissolves in water with a beautiful yellow colour. When digested with protoxide of iron in the cold it yields a brown amorphous compound, which dissolves in water with a blood-red colour. Picrate of copper is of a beautiful yellowish-green colour when in solution. If required for dyeing purposes, it may be prepared by mixing a solution of a picrate with sulphate of copper. Picric acid was introduced as a dye about five or six years since, by MM. Guinon, Marnas, and Bonney, eminent silk dyers of Lyons. Many of the cheap products sold as picric acid are of a brown colour, and consist of impure di- and tri-nitrophenic acids, and sometimes of this crude product and ground turmeric.

Rosolic Acid.—Runge first noticed this substance in 1834, when studying creosote, but it was almost lost sight of until again observed by Dr. Hugo Müller only a short time since. He accidentally observed that when crude phenate of calcium is exposed to a moist, heated atmosphere, as that of an ordinary drying stove, it gradually changes in colour, and assumes a dark red tint. This coloration is owing to the formation of rosolate of calcium. I have lately heard that phenate of sodium undergoes a similar change. Dr. Müller prepared rosolic acid from this product in the following manner:—The crude rosolate of calcium is first boiled with a solution of carbonate of ammonia. By this means a crimson solution containing the rosolic acid is obtained. This solution is then evaporated nearly to dryness, during which process ammonia is given off, and the crimson coloured liquid gradually changes to a yellowish red, and at the same time a dark resinous matter separates. This resinous substance is crude rosolic acid. In order to purify it it is submitted to the following treatment, proposed by Runge:—The crude rosolic acid is dissolved in alcohol, and hydrate of calcium added in slight excess. The beautiful crimson solution which is thus formed is agitated for some time with the undissolved portion of the lime, filtered, and the filtrate diluted with water, and, lastly, the alcohol distilled off. The residuary rosolate of calcium is then decomposed with just a sufficient quantity of acetic acid, and the whole boiled until every trace of free acetic acid and still adhering alcohol is volatilised. The rosolic acid separates first as a red precipitate, but when heated, cakes together, forming a dark, brittle substance, having a greenish metallic lustre.

It may be still further purified by solution in alcohol, to which a little hydrochloric acid has been added, and precipitation with water. Pure rosolic acid is a dark amorphous substance, possessing the greenish metallic lustre of cantharides. Its powder is of a red, or rather, scarlet shade, which, if rubbed with a hard, smooth body, assumes a bright gold-like lustre. In thin layers, rosolic acid presents an orange colour when viewed with transmitted light, but with reflected light a golden metallic appearance. When thrown down from an

alcoholic solution with water, it forms a flocculent precipitate of a bright red colour resembling the basic chromate of lead. Concentrated acids, as acetic, hydrochloric and sulphuric, readily dissolve rosolic acid, forming brownish yellow solutions from which water precipitates this acid unchanged. To cold water it imparts a bright yellow colour, and it is more soluble in hot than cold water. Alcohol and ether dissolve rosolic acid with great facility, forming orange or brownish yellow solutions which, on evaporation, leave it in an amorphous state. With ammonia, caustic alkalis, and caustic earths, it forms dark red compounds which dissolve with a magnificent red colour. These compounds are very unstable. No precipitates are formed with aqueous solutions of the rosolates, with the basic acetates of lead, or with any other metallic salt; nor is any carried down by alumina or any other metallic oxide. Dr. Müller made two combustions of rosolic acid, which yielded results that agree with the formula $C_{23}H_{23}O_4$. I and Mr. Duppa, when investigating some of the derivatives of acetic acid, found that when phenic and bromacetic acids were heated together at 120 Centigrade, two products were formed, one possessing all the properties of rosolic acid, while the other had the character of brunolic acid. We also found that a mixture of iodine and carbolic acid when heated with formic, acetic, butyric, or valerianic acids, produce rosolic acid, or a similar substance. A mixture of carbolic acid and iodine did not give a similar result; but a black solid, containing iodine. Rosolic acid has lately been prepared on a large scale, and employed for the purpose of printing muslins. I believe it was a rosolate of magnesium that was employed for this purpose. I do not think it is now used, having been replaced by the more beautiful colouring matter fuchaine. It was fixed by means of albumen.

The Colouring Matters of Quinoline or Chinoline.—Chinoline is found associated with lepidine, cryptidine, and other of the higher nitrile bases, in the basic oils obtained from coal tar, and also in the products obtained by distilling cinchonine with caustic alkalis. It was previously termed leucoline or quinoline; but the substance described under these two names was found to contain three or more distinct bases. These have been studied by C. Greville Williams, who has applied the name chinoline to the product which forms the principal part of the impure substance originally termed quinoline. The substance used for the production of colouring matters need not be pure chinoline, as that product originally termed quinoline is of sufficient purity for this purpose. Cinchonine appears to be the best source of chinoline, yielding, when distilled with excess of caustic alkali, 65 per cent. of this body sufficiently pure for manufacturing purposes. Chinoline yields three colouring matters—a violet, a blue, and a green. The following is an account of their preparation by C. Greville Williams:—

"In order to procure the blue colour, one part by weight of chinoline is to be boiled for ten minutes with one and a-half parts of iodide of amyl. The mixture from being straw-coloured becomes deep reddish brown, and solidifies on cooling to a mass of crystals. This product of the reaction is to be boiled for ten minutes with about six parts of water, and, when dissolved, filtered through paper. The filtered liquid is to be gently boiled in an enamelled iron pan over a small fire, and excess of ammonia gradually added. The ebullition may be prolonged with advantage for one hour, the evaporation of the liquid being compensated for by the gradual addition of weak solution of ammonia. The latter may be prepared by the admixture of equal volumes of ammonia of the density of 0.880 and distilled water. The hour having elapsed, the whole is allowed to cool, when the colour will almost entirely have precipitated, leaving the supernatant liquid nearly colourless. On pouring the fluid away (preferably through a filter, in order to retain floating particles of colour) the dish will be found to contain

resinous-looking masses, which dissolve readily in alcohol, yielding a rich purplish-blue solution, which may be filtered and kept for use.

"The colour prepared as above is, as has been said, of a purplish tint, but if a purer blue be required the following modification is to be resorted to. The filtered aqueous solution of hydriodate of amyle-chinoline, is as before, to be brought to the boiling temperature, but instead of adding ammonia, a solution of caustic potash containing about one-fifth of its weight of solid potash is to be substituted. The addition is to be continued at intervals until three-fourths as much potash has been added as is equivalent to the iodine in the iodide of amyle used. The fluid may, after a quarter of an hour's ebullition, be filtered to separate the resinous colour. The product is a gorgeous blue with scarcely any shade of red. On adding the other fourth of potash to the filtrate while gently boiling, a black mass will be precipitated containing all the red, which otherwise would have been mixed with the blue. This mass dissolves readily in alcohol, yielding a rich purple solution containing, however, an excess of red. The alcoholic solution on filtration leaves on the filter a dark mass soluble in benzole, and as sometimes prepared, affording a brilliant emerald green solution of great beauty. It is not always easy to obtain this green colour."

The properties of chinoline violet, and chinoline blue are, as far as I have been able to ascertain, identical. They are resinous substances which present a coppery appearance by reflected light; but when in very thin layers, appear of a violet or blue colour by transmitted light. They are bases, and dissolved in acids, forming pale red solutions, which ammonia restores to their original colours. They are slightly soluble in hot water. Tannin precipitates them from their aqueous solution, apparently forming an insoluble compound. Reducing agents do not affect their shade of colour.

Of chinoline green I know but little. Greville Williams describes it as having a brilliant emerald green colour of great beauty. I have observed that when chlorine is passed through an alcoholic solution of chinoline blue, it changes into a green; but whether this is the green spoken of by Williams, I am unable to state.

Naphthaline Colours.—The beautiful hydrocarbon, naphthaline, which has yielded such a long category of substances to the chemist,—up to the present time has yielded nothing of practical importance to the dyer. From it the following coloured derivatives have been obtained,—namely,—chloroxynaphthalic acid, perchloroxynaphthalic acid, carminaphtha, ninaphthalamine, nitrosanaphthaline, and naphthamein.

Chloroxynaphthalic and Perchloroxynaphthalic Acids.—These acids were discovered by Laurent. They are produced by digesting their chlorides, namely, the chloride of chloroxynaphthyle and the chloride of perchloroxynaphthyle, with an alcoholic solution of hydrate of potassium. They appear to be very difficult substances to obtain in quantity. I have not obtained satisfactory results when endeavouring to prepare them. They have the formula $C_{10}(H_5Cl)_2O_3$, and $C_{10}(HCl_2)_2O_3$, respectively. They are regarded with great interest as being very closely allied with alizarine, the colouring matter of madder; in fact, they are viewed as chloralizaric acid. That hypothesis is based upon the idea of alizarine having the formula $C_{10}H_4O_3$; but it happens very unfortunately for this theory, that the formula of alizarine itself is still a disputed point. Chloroxynaphthalic acid is of a yellow colour. It is insoluble in water, and difficultly soluble in boiling alcohol and ether; but it dissolves in concentrated sulphuric acid. This acid is a very sensitive test for alkalis, being changed to an orange red by them. This may be shown by moistening paper with a weak alcoholic solution of this acid, drying it, and then exposing it to

ammoniacal vapours. This will cause it to assume a red colour.

The chloroxynaphthalates are described as possessing great beauty, and are of yellow, orange, or crimson colours. The potassium salt is of a red crimson colour, and slightly soluble in water; the barium salt crystallizes in silky needles, having a golden reflection. The strontium, calcium, aluminum, and lead salts are of an orange colour; the cadmium salt is a vermillion-coloured precipitate; the copper and cobalt salts are crimson; and the mercury salt is of a red brown colour. I once dyed some silk with a small quantity of chloroxynaphthalate of ammonium, which I prepared, and found it to produce a good golden yellow colour, of great stability under the influence of light. Perchloroxynaphthalic acid is a yellow, crystalline body, insoluble in water, but soluble in alcohol and ether. With potash or ammonium it forms insoluble salts of red or crimson colour of great beauty.

Carminaphtha.—This colouring matter was also discovered by Laurent. It is obtained by heating naphthaline with a solution of bichromate of potassium, and then adding sulphuric or hydrochloric acid. It is described as a fine red substance, soluble in alkalies, but precipitated from its alkaline solutions by means of acids. I have never obtained this product when oxidizing naphthaline.

Ninaphthalamine.—Ninaphthalamine is a name which has been given to a remarkable base which was noticed by Laurent and Zinin; but nothing was known of its nature until subjected to investigation by Mr. Wood, who has both described and analysed it and some of its salts. Its formula is $C_{10}(H_2NO)_N$, or naphthalamine in which H is replaced by NO. Mr. Wood prepares this base in the following manner:—Sulphuretted hydrogen is to be passed through a boiling solution of dinitronaphthaline in weak alcoholic ammonia until nearly all the alcohol has distilled off, which operation should occupy two or three hours. The residue is then to be boiled with dilute sulphuric acid, and filtered. The filtrate, on cooling, deposits an impure sulphate of ninaphthalamine in the form of brownish crystals which are purified by recrystallization in water two or three times. I have found when crystallizing this salt that it is best to use water acidulated with sulphuric acid. When pure, this sulphate has to be decomposed with ammonia, and the resulting precipitate of ninaphthalamine washed with water. Thus obtained, ninaphthalamine appears as a bright red coloured crystalline precipitate, which when viewed under a lens appears as beautiful needles. It is very soluble in alcohol producing a solution which, when dilute, is of an orange colour slightly tinged with brown, not nearly so pure in colour as that of nitrophenylenediamine. It is slightly soluble in water, and possesses the power of dyeing silk with a colour somewhat similar to that of ordinary annata. With acids it produces colourless salts. Its formula is the same as that of nitroso-naphthaline, though it possesses very different properties. As a dyeing agent I do not think it would be of any value even if it could be obtained cheaply.

Nitroso-naphthaline.—This peculiar body is a product of the action of nitrous acid on naphthalamine. It is prepared by mixing a solution of hydrochlorate of naphthalamine with nitrate of potassium. From this mixture it separates as a reddish-brown precipitate. This, when washed with water on a filter and then dried, is dissolved in alcohol, filtered, and evaporated to dryness on the water-bath. Thus prepared, it is a crystalline, dark-coloured substance, having a greenish metallic reflection. It is soluble in alcohol, and also in benzole, forming orange red solutions. When acids are added to an alcoholic solution of nitroso-naphthaline it immediately assumes a most beautiful violet colour, as fine as any aniline purple. Alkalies restore it to its original colour. Silk may be dyed a beautiful purple shade with this substance, provided a certain quantity of hydrochloric acid or sulphuric acid be present. But what is most unfortunate

is, that when the silk thus dyed is rinsed in water the colour immediately passes back to that of the pure nitroso-naphthaline, and also that the amount of acid required to keep up the purple shade if left in the silk rots it in a few days. Could this purple be fixed, nitroso-naphthaline would be a cheap and most useful dye. I have endeavoured to produce the sulpho-acid of nitroso-naphthaline, thinking that if such a compound could be obtained, it would possess a purple colour because it would be an acid itself. But although sulphuric acid does dissolve it, forming a blue solution, yet no combination takes place. I also endeavoured to produce this desired result by treating sulpho-naphthalamine acid with nitrous acid, but obtained only nitroso-naphthaline, the acid of the sulpho-naphthalamine acid having apparently separated.

Naphthamein.—Piria observed that naphthalamine and its salts produced blue precipitates, afterwards becoming purple, when brought in contact with perchloride of iron, terchloride of gold, nitrate of silver, and other oxidizing agents. This product of oxidation he terms naphthamein. It is prepared by adding a solution of perchloride of iron to a solution of hydrochlorate of naphthamein. This mixture gradually changes and becomes blue, and after the lapse of a short time deposits a blue precipitate. This, when separated by means of a filter, is washed with water which causes it to change in colour until of a reddish-brown purple. The filtrate from this substance contains proto-chloride of iron, and, according to Piria, chloride of ammonium. Naphthamein when heated fuses and decomposes, leaving a residue of charcoal behind. It is insoluble in water, sparingly soluble in alcohol, but more soluble in ether. It forms a blue solution with concentrated sulphuric acid, and is reprecipitated from it by water. It dissolves in concentrated acetic acid with a purple colour, and is not precipitated from this solution by means of water. Silk and cotton may be dyed with it, but the colour of this compound is so inferior as to render it useless as a dyeing agent.

Tar Red.—This colouring matter was discovered by Mr. Clift, of Manchester, in 1853. It is obtained by exposing a mixture of the more volatile parts of the basic oils of coal-tar and hypochlorite of calcium to the air for about three weeks. Of the pure colouring matter I know nothing, except that with tannin it forms an insoluble, or difficultly soluble, substance. With different mordants it yields different colours. It seems probable that this colouring matter is derived from pyrrole.

Azuline.—This substance, which is a beautiful blue dye, has been introduced within the last six months. It was discovered by MM. Guinon, Marnas, and Bonney, of Lyons, who keep the process for its preparation a secret. It is obtained from coal-tar, but from which of its numerous derivatives is not known. This colouring matter is a brittle, uncrystallizable body, possessing a coppery metallic reflection. It is very difficultly soluble in water, but soluble in alcohol, producing a magnificent blue solution, having but a slight tinge of red. With concentrated sulphuric acid it forms a blood-red liquid which, when poured into an excess of water, precipitates the colouring matter unchanged. Dilute acids have no effect upon azuline. Its alcoholic solution, when mixed with an alcoholic solution of hydrate of potassium, also changes to a dull red colour. This, when diluted with water, forms a purple liquid which is gradually restored to its original blue colour by hydrochloric acid. With excess of ammonia the solutions of azuline change to a reddish purple colour. This ammoniacal solution, when treated with sulphide of ammonium gradually assumes a dull, yellowish-brown colour. Iodine destroys the colour of azuline. In colour it is not quite so fine as chinoline blue, though far superior to Prussian blue.

Application of Coal-tar Colours to the Arts of Dyeing and Calico Printing.—I cannot enter fully into this subject, because not being a practical dyer or printer, I do not feel

sufficiently acquainted with the various operations of the dye-house or print-works to do so, and also because the technical details of dyeing and printing operations would not, I think, interest the Society. I therefore propose to speak of the different processes employed for dyeing and printing with coal-tar colours, in general terms only.

Dyeing Silk and Wool.—Silk and wool can be dyed with all the coal-tar colours with the exception of the rosolates, these fibres possessing, in most cases, a remarkable affinity, if I may so speak, for these colouring matters. Many of them, as aniline purple and violine are taken from their aqueous solutions so perfectly by these substances that the water in which they have been dissolved is left colourless; in fact, silk and wool take them up so rapidly that one of the great difficulties the dyer has to contend with is to get the fibres dyed evenly.

To Dye Silk with Aniline, Purple, Violine, and Rosoline.—One process is applicable for dyeing silk with either of these colouring matters, and it is a very simple one. An alcoholic solution of the colouring matter required is to be mixed with about eight times its bulk of hot water previously acidulated with tartaric acid, and then poured into the dye-bath, which consists of cold water slightly acidulated. After being well mixed, the silk is to be worked in it until of the required shade. If a bluer shade than that of the colouring matter is required, a little solution of sulpho-indigotic acid may be added to the dye-bath, or the silk may previously be dyed blue with Prussian blue, or any other blue, and then worked in the dye-bath.

To Dye Silk with Fuchrine, Picric Acid, Chinoline Blue, and Chinoline Violet.—This process is still more simple than the above, as it is simply necessary to work the silk in cold, aqueous solutions of these colouring matters. With fuchrine or picric acid a little acetic acid may be used, but with chinoline colours acids must be avoided. With picric acid a very clear green colour may be obtained by adding a little sulpho-indigotic acid to the dye-bath. I may mention that violine is not of such a fine colour as that produced by aniline purple and indigo blue; and also that rosine is not such a good colour as fuchrine or magenta.

To Dye Silk with Aniline.—The dyeing of silk with this colouring matter is far more difficult than with the preceding, requiring to go through two or three different processes. This difficulty, I believe, arises from the insolubility of aniline in water. The process generally employed is to work the silk in a solution of the colouring matter acidulated with sulphuric acid, and when of a sufficient depth, to raise the temperature of the dye bath to the boiling point, and work the silk in it again. After this the silk is well rinsed in water until free from acid, and worked in a bath of soap lather. It is then again rinsed and finished in a dilute acid bath.

To Dye Wool with Aniline Purple, Violine, Rosoline, Fuchrine, and Chinoline Colours.—This operation is generally conducted at a temperature of 50 or 60 Centigrade, and the dye-bath is composed of nothing but a dilute aqueous solution of the colouring matter required. Acids should be avoided, or only a very small quantity used, as the resulting colours are not so fine when they are employed.

Methods of Dyeing Cotton with Coal-Tar Colours.—When aniline purple was first introduced, considerable difficulty was experienced in dyeing cotton so as to obtain a colour that would resist the action of soap. Aniline purple is absorbed by vegetable fibres to a certain extent, and very beautiful colours may be obtained by simply working cotton in its aqueous solution; but when thus dyed the colours will not stand the action of soap. I have tried the use of tin and other mordants, but without any satisfactory result.

In 1857, Mr. Fuller, of Perth, and myself simultaneously discovered a process by which this colouring matter could be fixed upon vegetable fibres so as to resist the action of

soap. This process is based upon the formation of an insoluble compound of the colouring matter with tannin and a metallic base in the fibre. To effect this the cotton has to be soaked in a decoction of sumac, galls, or any other substance rich in tannin, for an hour or two, and then passed into a weak solution of stannate of sodium, and worked in it for about an hour. It is then wrung out, turned in a dilute acid liquor, and then rinsed in water. Cotton thus prepared is of a pale yellow colour, and has a remarkable power of combining with aniline purple.

The above process may be modified: for example, the stannate of sodium may be applied to the cotton before the tannin, and alum may also be used in the place of stannate of sodium. To dye this prepared cotton with aniline purple it is only necessary to work it in an acidulated solution of the colouring matter; and when thus prepared the cotton will absorb all the colouring matter of the dye-bath, leaving the water perfectly colourless. It has been found that cotton thus prepared can be dyed with any colouring matter that forms insoluble compounds with tannin; therefore it is used for dyeing with rosine, violine, fuchrine, and chinoline colours.

Cotton may also be dyed a very good and fast colour by mordanting it with a basic lead salt and then working it in hot solution of soap to which aniline purple has been added. Oiled cotton, such as is used for dyeing with madder, is also used in dyeing these colours. Cotton simply oiled, and before mordanted with alum and galls, also combines readily with these colouring matters; but as the colour of the prepared cotton is generally rather yellow, it interferes sometimes with the beauty of the result. Cotton is sometimes coated with albumen, which is coagulated by the action of steam, and the albumen which covers the cotton dyed in the usual manner. I may mention that violine, rosine, fuchrine, and also the chinoline colours combine with unmordanted vegetable fibres, as well as aniline purple. Picric and rosolic acids are not applicable for dyeing cotton.

Printing Calico with Coal Tar Colours.—The process generally employed for printing with these colouring matters is simply to mix the colouring matters with albumen or lacterine, print the mixture on the fibre, and then to coagulate the albumen or lacterine by the agency of steam. I and Mr. Gray, of the Dalmonach Print Works, discovered the first process of applying these substances to fabrics in a different manner to the above. It consisted in forming a basic carbonate or an oxide of lead on those parts of the cloth which were to be coloured, and then working the cloth thus prepared in a hot lather containing the colouring matter. Where the cloth was mordanted with the lead compound colouring matter was absorbed; but when unmordanted it was left white, because pure cotton is not dyed with these colouring matters in the presence of soap. This process was intended for the application of aniline purple, for at the period of this discovery the other coal tar colours were unknown. Colours dyed by this process were very pure, but it had many disadvantages, which have caused it to be disused. Lately the process previously described for dyeing colours upon cotton prepared with tannin has been applied to calico printing. It consists in printing tannin upon the fabric previously prepared with stannate of sodium, and then dyeing it in a hot dilute acid solution of the colouring matter. By this means the parts of the fabric which are covered with tannin are dyed a deep colour, but the other parts are only slightly coloured. These are cleared by means of well-known processes. This method of applying these colouring matters is also modified by printing a compound of the colouring matter required and tannin on the prepared cloth, instead of tannin only, and then steaming the goods.

Method of Applying Aniline Green to Fabrics.—This process is interesting as being the first example of the production of coal-tar colours on the fabric itself. It was

proposed by Professor Calvert. His process is very simple. The design is to be printed on the cloth with a thickened solution of chlorate of potassium, dried, passed through a solution of an aniline salt, again dried, and allowed to hang in a moist atmosphere. In the course of two or three days the colour will be fully developed. The colour thus produced may be changed into a dark blue by the agency of soap or an alkaline liquid. This process is very inexpensive, the amount of aniline required being very small.

Application of Nitrosonaphthaline.—If cloth is printed with a thickened solution of a salt of naphthalamine, dried, and then passed through a solution of nitrite of potassium, nitrosonaphthaline will rapidly make its appearance as a reddish orange colour; but, unfortunately, the colour thus obtained will not resist the action of soap well.

Of the numerous colouring matters of which I have briefly spoken, there are only four that are at present employed by the dyer and printer, namely, aniline purple, fuchsine, picric acid, and azuline; but I think it probable that others of them will soon be introduced, such as the *bleu de Paris*; and nitrophenylene diamine might be used for silk dyeing, as its colour is good and it stands the action of light well. Unfortunately, the chinoline colours, though very beautiful, are most fugitive. There has been an endeavour to introduce the chinoline blue of late, but although a considerable quantity of silk was dyed with it at first, it is now scarcely used, because when exposed to the sun for two or three hours the dyed silk becomes bleached. Aniline purple resists the light best. Fuchsine and alpha aniline purple soon fade, especially when on cotton. Azuline and *bleu de Paris* are not easily acted upon by light when on silk.

When the colouring matters of coal-tar were first introduced, there was a great fear that the workmen engaged in their manufacture, would suffer in health. All I can say is, that during the few years I have had to do with this branch of manufacture, there has not been a single case of illness among the workmen that has been produced by any operations carried on for the production of aniline purple.

The CHAIRMAN: Gentlemen, I am sure I am only expressing the opinion of the Society in conveying to Mr. Perkin the best thanks of the meeting for the important and remarkably illustrated Lecture which we have just heard. The great interest which this subject has excited is amply testified by the large audience which it has collected together, anxious to hear an account of the development of an industry in the carrying out of which Mr. Perkin has been particularly instrumental. As far as I am myself concerned, I must say that I have listened to this Lecture with the greatest interest, and with particular gratification. Having had the good fortune of guiding the early researches of Mr. Perkin, I cannot but feel the most lively interest in the success of this great industrial achievement which Mr. Perkin has accomplished. The courage and the perseverance with which he has gone on in his experiments must be appreciated by everyone familiar with the great many difficulties which beset the path of an inventor. These difficulties I know have not failed in the case of Mr. Perkin, and he deserves infinite credit, that whilst engaged in carrying out commercial undertakings of very considerable magnitude, he has found time to devote his attention to investigations of purely scientific interest—investigations which have occupied the attention of this Society repeatedly during the last few years. There is another cause from which I have derived great gratification in listening to these Lectures. This is of a mere personal character, and I will allude to it only in the most general terms. We all return with very great satisfaction and delight to our first loves. I must confess that aniline was one of my early flames—(laughter)—and I feel great pleasure in seeing it so popular in these times. I see before me quite an array

of distinguished members of the Chemical Society, and more especially the very father of this whole industry—Mr. Faraday, the discoverer of benzole. (Applause.) Perhaps he will kindly favour us with some remarks.

Professor FARADAY: What an encouragement it affords, sir, to the young chemist to pursue his studies earnestly and heartily, when he finds that such a small thing as the separation of benzole from other matters may lead to such large results!

The CHAIRMAN: The meeting is honoured by the presence of another gentleman whose remarks the Society have always heard with the greatest pleasure and delight,—Professor Playfair, of Edinburgh, is with us this evening.

Professor PLAYFAIR: Your commands, Mr. President, are imperative from the chair, but I have exceedingly little to say, except to repeat the gratification which, with you, I entertain, and also to express the feelings which one must share when we consider that the parent of these discoveries has produced from his first love such an array of children, to whom, as parent is apt, he has given such long and wonderful names—(laughter),—they producing in their turn a progeny of great beauty with most blooming colours and with the promise of numerous descendants with much more elegant names, such as fuchsine and roseine, and which are much more likely to carry down his fame to posterity. I am speaking seriously in this, for I believe and I have heard in some manufacturing districts when speaking with several calico-printers, that they look upon this discovery—to which Mr. Perkin has added so much—as very great in a national point of view. I have heard some who are well informed upon the subject, and well qualified to judge of it in this point of view, say that the progress making with regard to these colours from coal-tar was so great, and the practical discoveries for applying them to industry are becoming so simplified, that they are of opinion that in a few years instead of our being a colour-importing nation—instead of depending upon Turkey and Holland for madders,—by producing a staple colour of our own, we shall in all probability become the great colour-exporting nation of the world. I therefore think that Mr. Perkin whom Dr. Hofmann, in his sagacity and his care for these beautiful children of his, employed early and trained to be a nurse to rear them in the admirable way in which he has done, will be exceedingly gratified in finding that he has founded a national industry; and in the Exhibition of 1862, I hope Mr. Perkin will take care that the world shall see what has been done in the last few years in this branch of science, and that he will give us a full illustration of these beautiful discoveries.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting, Friday, March 15, 1861.

*A Lecture on Electrical Quantity and Intensity,
by LATIMER CLARK, Esq.*

THE modifications of the strength of the electric current in dynamic electricity, and in the amount of charge in static electricity, are at present usually defined by the terms Quantity and Intensity. The speaker pointed out that the expression intensity, as ordinarily understood, really involved two perfectly distinct qualities, and dwelt on the advantage which would accrue to electrical science by the habitual separation of the complex idea of intensity into its two component parts, viz. that of tension, as propounded by Ohm in his celebrated mathematical investigation of the galvanic circuit; and that of quantity, as developed by Faraday in those valuable researches in which he established the definite quantitative character of electrochemical decomposition and the action of electricity on the galvanometer. The term "tension," as here used, is

intended to convey the same idea as the expression electromotive force, or as the term "electric potential," employed by Green and other mathematicians, and is entirely dissociated from the idea of quantity; both terms are equally applicable to electricity at rest or in motion.

The quantity of electricity, both in its static condition and in its motion through conductors, usually varies directly as the tension, and hence their joint effects have been ordinarily confounded together and attributed to one cause under the name of intensity; but since the tension and quantity do not, under all circumstances, vary in the same ratio, there exists an absolute necessity for their clear separation before any numerical reasoning can be founded on them. Cases of the independent variation of tension and quantity were shown, and it was pointed out that all the most striking properties of electricity, such as the decomposition of water and salts, the combustion of metals the deflection of the galvanometer, the attraction of the electro-magnet, and the physiological effects of the current were really dependent as regards their magnitude and energy, solely on the quantity of electricity passing. Their greater energy when the tension was increased, was an indirect effect, due not to that tension, but to the increased quantity which passed in a given time by reason of the increased tension. A galvanometer wound with a few turns of thick wire was shown to be deflected as powerfully by one cell as by six, or even by 600 cells of the same size, because by reason of its shortness the wire conveyed freely the whole quantity which one cell could produce, which was the same as that produced by the whole 600; but any alteration in the size of the cell produced a consequent change in the quantity and in the deflection of the galvanometer. On the other hand, a galvanometer with many thousand turns of fine wire gave the same deflection with a battery formed of a small gun-cap, as with one of twenty square feet of surface, because the quantity in this case was regulated and limited not by the size of the plates, but by the power of conduction of the wire; the quantity being therefore the same in both cases. In every case the deflection was dependent solely on the quantity of electricity actually passing through the instrument without reference to its tension.

The combustion of metals was shown to be a phenomenon dependent on quantity, and not on tension; one cell of Grove's battery ignited a certain length of platina wire; and whatever its size it would ignite no greater length; but two, three, or more cells were shown to ignite two and three times the original length, the quantity passing in the greater length being under the higher tension, precisely the same as in the original length. This explained Faraday's oft misunderstood remark, that the same quantity of electricity which would ignite an inch of wire, would ignite a foot or a mile.

The pain and shock experienced on touching a powerful battery, or shocking coil, or Leyden jar, were proportionate to the quantity of electricity passing through the system, and not to the tension. A carrier ball or minute Leyden jar charged to the highest tension, would produce no sensation if the quantity were absent, and the same was the case with a Zamboni's pile. Sparks nearly eighteen inches long were received from an electrical machine; but although of this high tension, they produced no violent physiological effects, owing to their deficiency in quantity. Long sparks, nevertheless, produced greater effects than short ones because at double the striking distance the tension is doubled, and the quantity is therefore twice as great. The quantity contained in a Leyden jar or battery is comparatively great, and the effect on the system proportionately violent. Two conditions were necessary for these effects: first, that the quantity present should be considerable; and secondly, that the tension should be sufficiently great to make it pass through the system. A battery of two or three cells, which could readily fuse platina wire, was shown to produce no painful sensation

on the tongue, because, although the quantity was abundant the tension was low; while another battery of 600 cells, which produced the most intolerable shock to the system had, from its deficiency in quantity, scarcely any power to fuse wire. The Ruhmkorff coil combined very high tension with considerable quantity, and its physiological effects were therefore very violent.

A frictional machine was exhibited by Mr. Varley, constructed on a plan of Dr. Winter's; the plate was of vulcanite, or vulcanized Indian-rubber, about three feet in diameter, excited by amalgam in the usual way; its peculiarity was a large and lofty wooden ring, with a metallic rod in its interior, which, by its overshadowing inductive influence, increased the length of the sparks from six or seven inches to nearly eighteen.

The forces of electrical attraction and repulsion are sometimes stated to vary as the square of the intensity, sometimes as the square of the quantity, and sometimes as the square of the distance; but it was contended that these effects were due to the circumstance that the quantity usually varies in the same ratio as the tension, and as the distance; and that all the phenomena were more rationally explained by the assumption that electrical attraction and repulsion vary in the simple ratio of the quantity and of the tension, and of the distance inversely.

(To be continued.)

CORRESPONDENCE.

On the Adulteration of Food.

To the Editor of the CHEMICAL NEWS.

SIR,—In the discussion of this question the mustard trade has been so often alluded to, as one in which adulteration is carried to its greatest extent, that it becomes a matter of importance to know how far the statements that have from time to time been put forward are correct, and to ascertain what changes we may expect to bring about in supplies of this article by means of legal enactments; for since this trade appears to be so generally acknowledged as most fully exemplifying the evils of adulteration, we may, by investigating this particular branch of manufacture, arrive at some conclusion as to how far we may hope to protect ourselves against the adulteration of articles of food by passing more stringent prohibitory regulations, and how much must still be left to the exercise, on the part of the public, of discretion and common sense. I have, therefore, endeavoured to collect information from various sources on this subject, and have considered that a short detail of the results of my inquiries may not be uninteresting to your readers.

Mustard-seed is of two kinds, brown and white, a mixture of the flour of both kinds being generally used in all qualities of mustard. The flour from the brown seed possesses the greatest pungency, whilst that from the white seed has a finer appearance and more delicate flavour. The seed is crushed by steel rollers, then pounded in iron mortars, and the flour separated from the bran by sieves. On account of its oily nature, the seed is not readily reduced to powder, but requires careful crushing, the operation being repeated several times. The flour which passes through the finest sieves after the first grinding is sold as "D.S.F." (double superfine), and is worth, wholesale, from 1s. 3d. to 1s. 9d. per lb. The coarser flour is again ground with a little corn flour and cayenne, and sold as "S.F." (superfine). The product of a third grinding is mixed with more corn flour, cayenne, and a little turmeric, and is sold as "fine;" whilst the produce of a still further grinding of the coarse particles is again mixed with corn flour, cayenne, and turmeric, and sold as "seconds."

Admixture of mustard with flour, capsaicums, or other spice, and turmeric as a colouring material is in general use by the first manufacturers, and no pretence is made of concealing the fact.

Apart from the object of lowering price, admixture is resorted to in order to suit the taste of customers, and to render the mustard capable of being kept for a longer period. There is every reason to believe that the use of plaster of Paris is not uncommon, though this practice is not resorted to by the more respectable houses. The manufacturer is just as ready to sell genuine mustard as he is the mixed qualities, provided he can obtain a corresponding price, but it is impossible he can do this whilst "best mustard" is sold at 11d. or 1s. per lb., whereas the genuine would cost from 1s. 3d. to 1s. 9d. per lb. So long as the public will buy mustard at present prices, the manufacturer is obliged to resort to admixture. If the agent of a manufacturer offer to the retail tradesman a genuine article, he is told, that "We buy at twenty per cent. below yours; we don't know how it is made, but our customers make no complaints, and it answers our purpose. When you have an article to offer at our price, we can do business with you." Or, even supposing that the manufacturer has managed to obtain a demand for a genuine article, he knows very well, unless he will supply all qualities, his returns will not pay his traveller's expenses. The manufacturer is thus compelled to follow in the track of others, and so long as he abstains from using any pernicious ingredient, and selling a mixed article as genuine, I cannot see that any portion of the blame can rest with him. The prohibition of the sale of any but a genuine mustard would not be likely to meet with the approval of the public, inasmuch as, since the coarser qualities could scarcely be made into a marketable article, the price of mustard would be raised to an enormous extent, and the poorer classes would find themselves, in a great measure, deprived of a cheap and agreeable condiment. The question of the purity of mustard appears to me to rest principally with the consumer. So long as the public insist upon being supplied at a price below that at which the genuine can be sold, so long will adulteration be resorted to. Create a demand for the genuine article at a remunerative price, and there is no difficulty in obtaining a supply. It would, indeed, be a matter of surprise if a sample of mustard, sold retail at three farthings per oz., should be found to be genuine, and the only use of analysis of such samples is to show the nature of the adulterating material employed. I think it must be manifest to all who fairly consider this question of adulteration that all which legislation can do is to make the admixture of any substance acknowledged to be pernicious or deleterious to health with articles of food, and the selling of an adulterated article as "genuine," punishable by fines and penalties, on conviction of the offending parties before a magistrate.—I am, &c. VERAX.

On Naphthalic Alizarine.

To the Editor of the CHEMICAL NEWS.

SIR,—Observing in your last impression a notice (from the *Comptes-Rendus*) of M. Roussin's note on the artificial production of alisonine, I wish here to record that more than five years ago my researches upon naphthaline and some of its derivatives led me somewhat in the direction of the colour I endeavoured to produce; and early in 1857 I obtained, by a process differing only in detail from that of M. Roussin, a fine red colour, approaching the one sought. I showed some of the crude product, in the same year, to my friend Dr. B. W. Richardson, who expressed his opinion that the product in question very greatly resembled alizarine, if it was not identical with that principle. Circumstances unnecessary to publish, but which I explain herewith for your personal satisfaction

only, prevented my continuing the investigation until a few weeks ago.

I need scarcely say that the preceding remarks are not intended to be taken as in any way antagonistic to the perfectly independent experiments of M. Roussin or others (which indeed they tend rather to corroborate), but are penned in simple justice to myself.—I am, &c.

WENTWORTH L. SCOTT.

Bayswater, June 3, 1861.

Dead Black for Metal Work.

To the Editor of the CHEMICAL NEWS.

SIR,—In reply to your correspondent's enquiry relative to the most suitable dead black varnish for coating the sights of rifles, I would recommend a trial of the material ordinarily used 'by foundry' pattern-makers for staining the wooden model black, and rendering it less impervious to moisture before proceeding to make the sand mould. This varnish is easily prepared, by incorporating lamp black with a solution of shellac in methylated spirit. Applied to the metal surface with a camel's-hair pencil, it quickly dries to a dead black surface, firmly adherent, not sensibly affected by rain, and so simple of application, that should it happen to become rubbed off, it may easily be renewed.—I am, &c.

ANOTHER RIFLEMAN.

MISCELLANEOUS.

Royal Institution of Great Britain.—At the General Monthly Meeting, held on Monday, June 3, 1861, Rev. John Barlow, M.A., F.R.S., in the chair, the Secretary announced that his Grace the President had appointed the following Vice-Presidents for the ensuing year:—the Lord Wensleydale, the Lord Stanley, M.P., F.R.S., Sir Henry Holland, Bart., D.C.L., D.D., F.R.S., the Rev. John Barlow, M.A., F.R.S., John Peter Gassiot, Esq., F.R.S., and William Pole, Esq., F.R.S., the Treasurer. Frederick Augustus Burgett, Esq., John Dobie, Esq., Henry Wells Foote, Esq., William W. Gull, M.D., John Philip Malleon, Esq., B.A., and John Wells Wainwright, M.D., were elected Members of the Royal Institution. H. A. Pitman, Esq., was admitted a Member of the Royal Institution. The presents received since the last meeting were laid on the table, and the thanks of the Members returned for the same.

ANSWERS TO CORRESPONDENTS.

* In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

* All Editorial Communications are to be addressed to Mr. CROOKES, and Advertisements and Business Communications to the PUBLISHERS, GRIFFIN, BORN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

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A Subscriber.—1. Professor Way, Welbeck Street. 2. Messrs. Johnson and Matthey, Hatton Garden.

Electro-Plating Solution.—H. Wake.—Dissolve one ounce of fine silver in nitric acid, add three pints of water, and precipitate with common salt. Wash well, and dissolve the chloride in just enough cyanide of potassium to effect solution; filter and add water to one gallon. If the articles are required to come out bright, instead of dull, add a little bisulphuretted carbon to the solution.

P. M. H.—Received.

THE CHEMICAL NEWS.

VOL. III. No. 80.—June 15, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On a Fifth Element belonging to the Alkali Group, by Professor BUNSEN.

IN the investigation of the new metal having so close an affinity to potassium, and which has been called *cæsium* by Bunsen, there appears to exist, besides this *cæsium*, yet a fifth alkali metal, which has hitherto escaped notice, and which appears to resemble potassium quite as much as *cæsium* does.

The platinum salt of *cæsium* is with much more difficulty soluble in water than that of potassium. If we attempt to separate the latter from the former by repeated boiling with water, we find that in proportion as the quantity of potassium diminishes, and the continuous potassium spectrum between $K\alpha$ and $K\beta$ becomes faint, new lines appear, and among these in particular two very intense ones in the violet between $Sr\delta$ and $K\alpha\beta$. A point is soon reached at which the quantity of potassium is no longer diminished by boiling with water. This occurs when the united atomic weight of the metals combined with chlorine and platinum is 109 ($H = 1$). If, now, a mixture of the hydrates of potassium and *cæsium* be prepared from the platinum compounds, and about a fifth of this mixture be converted into carbonate, then absolute alcohol will abstract from the dried mixture almost exclusively hydrate of *cæsium*. If this operation be repeated, a limit is at length reached at which that which is dissolved in the alcohol has a constant composition. This occurs when the atomic weight has risen from 109 to 123.4 ($H = 1$). The substance which possesses this enormous atomic weight (next to gold and iodine, the greatest known) forms a deliquescent hydrate, as caustic as hydrate of potassium; it forms an equally deliquescent carbonate, which, at ordinary temperatures, is soluble to the extent of 10 parts in 100 of absolute alcohol; and an anhydrous nitrate, which crystallises, not rhombic, like nitre, but hexagonally, and by a hemiedral form, is isomorphous with nitrate of sodium, &c. The spectrum of this substance, purified up to an atomic weight of 123.4, shows the blue *cæsium* lines with great intensity, but the violet lines of the unpurified substances (atomic weight 109) so faintly, that a slight addition of chloride of potassium, which scarcely affects the lines $Cs\alpha$, makes them disappear at once, on account of the brightness of the ground produced by the potassium. The few grammes of material for this investigation were obtained from 44,000 kilogrammes of the Dürkheim mineral water. By a repetition of the process, from 150 kilogrammes of Saxon lepidolite, there was obtained by the first treatment with chloride of platinum, a product which exhibited the violet lines between $Sr\delta$ and $K\alpha\beta$ with the utmost intensity, but no traces of the lines $Cs\alpha$. If this platinum salt from lepidolite had been a mixture of the

cæsium and potassium compounds, the blue lines $Cs\alpha$ must have been visible along with the violet ones, because in the product obtained from the Dürkheim water, the violet lines disappear, first on the addition of chloride of potassium, and the *cæsium* lines much later, and, indeed, only when the potassium salt is in very great excess. It follows that there must exist, besides, potassium, sodium, lithium, and *cæsium*, yet a fifth alkali metal, which occurs in Dürkheim, Kreusnach, and other mineral springs, in small, but in lepidolite in larger quantities.—*Bericht der Akad. der Wissensch. zu Berlin*, 1861, S. 273—275.

On the Expansion of Metals and Alloys, by F. CRACE CALVERT, Ph.D., F.R.S., F.C.S., &c., R. JOHNSON F.C.S., and G. CLIFF LOWE.

(Continued from page 317.)

WE now give the details of our experiments to enable the reader to estimate or verify the co-ordination of our results.

	Division of the Scale read off in 25-cools of an inch in raising temperature from 10 to 90 degs.			Mean.	Divisions of same Scale read off in cooling from 90 to 100 degs.			Mean for 100 degs., calculated from these means, and corrected for expansion of vessel, &c., by deducting 20°.
	1st.	2nd.	3rd.		1st.	2nd.	3rd.	
Cadmium ..	174	171	172	172.3	176	173	172	173.7
Lead ..	155	156	157	156	161	160	159	160
Tin ..	142	143	145	143	147	148	147	147.3
Aluminium ..	120	120	120	120	122.5	121	122	121.8
Forged Zinc ..	119	121	120	120	120	119	120	119.7
Silver ..	110	109	109.5	109.5	111.5	110	110	110.5
Copper (pure), Cast..	106	105.5	106	105.8	103	103.5	107	104.5
Hammered ..	99	99	99	99	101	99	100	100
Gold ..	81	80.5	—	80.7	81.5	81	—	81.3
Bismuth ..	78	77	77.5	77.5	81.5	80	80	80.5
Wrought Iron ..	69	72	73	71.3	73.5	72	72.5	72.7
Cast Iron ..	67	68.5	68.5	68.0	68	70.5	70.5	69.7
Steel (soft) ..	66.5	62	63	63.8	66.5	65	66.5	66.1
Antimony ..	63	62	—	62.5	63	62	—	62.5
Platinum ..	57.5	—	—	57.5	58	—	—	58

From the above observations, we deduce the following Table of co-efficients of linear expansion from 0° to 100°:—

Cadmium (pure)	0.00332
Lead (pure)	0.00301
Tin (pure)	0.00273
Aluminium (commercial)	0.00222
Zinc, forged (pure)	0.00220
Silver (pure)	0.00199
Gold (pure)	0.00138
Bismuth (pure)	0.00133
Wrought iron	0.00119

Lead and Antimony.

	10° to 90°.				Mean for 100°, minus correction 20°.	90° to 10°.				Mean for 100°, minus correction 20°.
	1st.	2nd.	3rd.	Mean.		1st.	2nd.	3rd.	Mean.	
5. Pb = 88.92	146.0	148.0	—	147.0	163.7	149.0	—	—	149.0	166.2
6b = 11.08	—	—	—	—	—	—	—	—	—	—
4. Pb = 86.52	138.0	138.0	139.0	138.3	152.8	144.0	144.0	141.0	143.6	159.5
5b = 13.48	—	—	—	—	—	—	—	—	—	—
3. Pb = 82.80	134.0	137.0	—	135.5	149.3	140.0	134.0	139.0	137.6	152.0
4b = 17.20	—	—	—	—	—	—	—	—	—	—
2. Pb = 76.12	128.0	128.0	—	128.0	140.0	130.0	129.0	—	139.5	141.8
3b = 23.88	—	—	—	—	—	—	—	—	—	—
Pb = 61.61	107.0	108.0	—	107.5	114.3	111.0	111.0	—	111.0	118.7
5b = 38.39	—	—	—	—	—	—	—	—	—	—
Pb = 47.00	94.0	94.0	—	94.0	97.5	96.0	94.0	—	95.0	98.7
4b = 52.40	—	—	—	—	—	—	—	—	—	—
3. Sb = 34.86	90.0	—	—	90.0	94.5	90.0	91.0	—	90.5	93.1
5b = 65.14	—	—	—	—	—	—	—	—	—	—
Pb = 28.61	83.0	—	—	83.0	83.7	84.0	—	—	84.0	85.0
4b = 71.37	—	—	—	—	—	—	—	—	—	—
Pb = 24.10	77.0	—	—	77.0	76.2	79.0	78.0	—	78.5	78.1
5b = 75.70	—	—	—	—	—	—	—	—	—	—

Zinc and Copper.

	10° to 90°.				Mean for 100°, minus correction 20°.	90° to 10°.				Mean for 100°, minus correction 20°.
	1st.	2nd.	3rd.	Mean.		1st.	2nd.	3rd.	Mean.	
5. Zn = 83.70	128.0	128.0	—	128.0	140.0	130.0	130.0	—	130.0	144.5
Cu = 16.30	—	—	—	—	—	—	—	—	—	—
4. Zn = 80.43	129.0	130.5	—	129.7	142.1	131.0	131.5	—	131.2	144.0
Cu = 19.57	—	—	—	—	—	—	—	—	—	—
3. Zn = 75.76	125.6	—	—	125.5	136.8	125.0	—	—	125.0	136.2
Cu = 24.24	—	—	—	—	—	—	—	—	—	—
2. Zn = 67.26	119.0	118.0	—	118.5	128.1	121.0	119.0	—	120.0	130.0
Cu = 32.74	—	—	—	—	—	—	—	—	—	—
Zn = 50.68	118.0	—	—	118.0	127.5	118.0	—	—	118.0	127.5
Cu = 49.32	—	—	—	—	—	—	—	—	—	—
Zn = 33.94	107.0	107.0	—	107.0	113.7	110.0	110.0	—	110.0	117.5
Cu = 66.06	—	—	—	—	—	—	—	—	—	—
Zn = 25.12	100.0	101.0	—	100.5	105.6	103.0	102.5	—	102.7	108.2
Cu = 66.06	—	—	—	—	—	—	—	—	—	—
3. Zn = 20.44	104.5	—	—	104.5	110.6	105.0	—	—	105.5	111.8
Cu = 79.56	—	—	—	—	—	—	—	—	—	—
Zn = 17.05	102.0	—	—	102.0	107.5	102.0	—	—	102.0	107.5
Cu = 82.95	—	—	—	—	—	—	—	—	—	—

Copper and Tin.

	10° to 90°.				Mean for 100° C., minus correction 20°.	90° to 10°.				Mean for 100°, minus correction 20°.
	1st.	2nd.	3rd.	Mean.		1st.	2nd.	3rd.	Mean.	
5. Sn = 90.27	127.0	124.0	124.0	125.0	136.2	129.0	124.0	—	126.5	138.1
Cu = 9.73	—	—	—	—	—	—	—	—	—	—
4. Sn = 88.14	122.0	122.0	—	122.0	132.5	127.0	126.0	—	126.5	138.1
Cu = 11.86	—	—	—	—	—	—	—	—	—	—
3. Sn = 84.79	119.0	119.5	—	119.2	129.5	123.0	122.0	123.5	122.8	133.5
Cu = 15.21	—	—	—	—	—	—	—	—	—	—
2. Sn = 78.79	118.0	118.0	—	118.0	127.5	117.0	117.0	—	117.0	126.2
Cu = 21.21	—	—	—	—	—	—	—	—	—	—
Sn = 65.02	109.5	111.5	—	110.5	118.1	116.5	116.5	—	116.5	118.1
Cu = 34.98	—	—	—	—	—	—	—	—	—	—
Sn = 48.17	111.0	111.0	—	111.0	118.7	113.5	112.0	—	112.7	120.8
Cu = 51.83	—	—	—	—	—	—	—	—	—	—
Sn = 34.21	112.0	111.0	—	111.5	119.3	113.5	112.0	—	112.5	120.6
Cu = 65.79	—	—	—	—	—	—	—	—	—	—
Sn = 31.73	102.0	105.0	—	103.5	109.3	106.0	105.0	—	105.5	111.8
Cu = 68.27	—	—	—	—	—	—	—	—	—	—
Sn = 27.10	104.0	104.0	—	104.0	110.0	106.0	106.0	—	106.0	112.5
Cu = 72.90	—	—	—	—	—	—	—	—	—	—
Sn = 15.68	101.0	101.0	—	101.0	106.2	101.0	101.0	—	101.0	106.2
Cu = 84.32	—	—	—	—	—	—	—	—	—	—
Sn = 11.03	95.0	94.5	—	94.7	98.3	94.5	95.0	—	94.7	98.3
Cu = 88.97	—	—	—	—	—	—	—	—	—	—
Sn = 8.51	97.0	99.5	—	98.2	102.7	99.0	99.0	—	99.0	103.7
Cu = 91.49	—	—	—	—	—	—	—	—	—	—
Sn = 6.83	99.0	99.0	—	99.0	103.7	100.0	100.0	—	100.0	105.0
Cu = 93.17	—	—	—	—	—	—	—	—	—	—

The results obtained with the above series enable us to draw the following conclusions:—

1. That the various bronzes and brasses have a ratio of expansion less than that indicated by theory.

2. That, notwithstanding that the alloys Sn Cu₃ and Sn Cu₄ contain respectively 34.21 and 31.73 per cent. of a highly expansive metal—tin,—still their ratio of expansion is equal to that of pure copper, the presence of tin in these alloys having no influence on their rate of expansion.

(To be continued.)

On the Oxalates of Iron, by T. L. PHIPSON, Ph.D.,
Member of the Chemical Society of Paris, &c. &c.

THE two oxalates of iron of which I am about to speak have not yet been submitted to any investigation. They have been seen by several chemists, who have mentioned their existence in a cursory manner; but their composition and properties have never been examined.

In Nature there exists an oxalate of protoxide of iron, known by the name of *Humboldtite* (*Oxalite* or *Eisenresin* of some mineralogists), and which, according to the analysis of M. de Rivero, consists of one atom of oxalic acid and one atom of protoxide of iron. I have not yet had an opportunity of examining this mineral, nor have I endeavoured to form it artificially in the laboratory.

1. **Oxalate of Protoxide of Iron.**—When a dissolution of oxalic acid or of oxalate of ammonia is added to sulphate of protoxide of iron, and the mixture

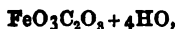
violently stirred, a considerable quantity of a yellow precipitate is obtained in a short time. This bright-coloured precipitate is very stable, even in contact with strong acids. Neither air nor light have any action upon this new salt at the ordinary temperature of the atmosphere; it is, moreover, insoluble in water and in dilute sulphuric and hydrochloric acids; it cannot either be dissolved in any excess of oxalic acid. When treated with concentrated nitric acid, it is suddenly decomposed, with an abundant evolution of rutilant vapours.

The best method of analysing this salt I found to be the following:—By calcining the salt the iron can be estimated accurately as peroxide of iron; the other elements, water and oxalic acid, must be determined by combustion of the salt with oxide of copper, as in an ordinary organic analysis. The water is received and weighed in a chloride of calcium tube, and the oxalic acid calculated from the amount of carbonic acid produced by combustion:—

Two analyses made in this manner gave me the following results:—

	I.	II.	Theory.
Protoxide of iron . . .	19'44	19'40	20'00
Oxalic acid (by diff.) . . .	59'56 (by combust.)	59'75	59'99
Water (direct) . . .	21'00 (by diff.)	20'85	20'00
	100'00	100'00	99'99

This composition corresponds, therefore, with the formula,—



or, if we assume that oxalic acid contains hydrogen with the formula,—



When this yellow powder is submitted to the action of heat, it takes fire when its temperature is very slightly elevated, and burns like so much tinder, leaving a residue of very pure peroxide of iron. When ferricyanide of potassium is poured upon it, in sufficient quantity to obtain a partial decomposition only, a fine pale-green colour is formed, which might be used as an oil-colour or a water-colour, and the tint of which may be varied with the quantity of ferricyanide employed. When the decomposition with the latter salt is complete, the ordinary blue is obtained.

If the green powder, procured in the manner just stated, be thoroughly desiccated and then heated a little, it also takes fire and burns like tinder, leaving nothing but peroxide of iron.

3. Oxalate of Peroxide of Iron.—I obtained this salt by dissolving hydrate of peroxide of iron in an excess of oxalic acid; it is best to perform this operation at night or in the shade. A fine green solution is thus obtained, which, evaporated and then left to itself, soon gives emerald-green crystals. These are oblique prisms, and are very soluble in water.

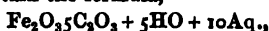
When these crystals are exposed to the light of the sun they blacken on their borders, like chloride of silver; and if they be then sprinkled over with water, a peculiar crackling noise is heard, after which they dissolve, leaving a residue of yellow oxalate of protoxide. This experiment is very striking if a capsule, the bottom of which is covered with the crystals in question, be submitted to the direct influence of the solar rays,—one-half of the crystals being protected from the light by the projecting border of the capsule remaining unaltered; the other half of the crystals blackens rapidly.

The same decomposition takes place with the green solution. When this solution is exposed to solar light it gradually deposits yellow oxalate of protoxide of iron in microscopic crystals, and, in the space of a week or two, the whole solution becomes perfectly colourless. If a bar of iron be placed in the solution the reduction takes place without the influence of light, and a yellow powder of oxalate of protoxide is thrown down.

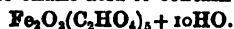
The analysis of these green crystals gave me the following result:—

	Found.	Calculated.
Peroxide of iron . . .	20'00	20'25
Oxalic acid . . .	45'00	45'58
Combined water . . .	24'00	22'76
Water of crystallisation . . .	11'00	11'41
	100'00	100'00

Whence we obtain the formula,—



or, if we suppose oxalic acid to contain hydrogen,—



The crystals lose their water of crystallisation at about 120° Centigrade, and, at the same time, their colour; when all this water of crystallisation is driven off they are perfectly white. The remainder of the water contained in the salt is only expelled by calcination. The same may be said of the greater portion of the water contained in the yellow oxalate of protoxide.

A curious fact connected with these two salts is, that the salt of protoxide is yellow, and the salt of peroxide green, contrary to what we generally remark with salts of iron. Generally, also, the salts of protoxide of iron are decomposed under the influence of exterior agents, and are converted into salts of peroxide, which are ordinarily more stable. In the cases before us precisely the reverse takes place.

The reduction of the peroxide salt into a salt of protoxide by the influence of light is very remarkable. It is, like all chemical actions of light, a phenomenon of reduction. I am not yet perfectly sure of what takes place in this circumstance. I have analysed the yellow salt deposited in this decomposition, and find it has for composition,—

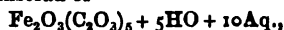


In fact, that it is the same oxalate of protoxide of iron described above, and from which it differs only by being crystalline, the other being amorphous.

When we come to consider the constitution of these oxalates of iron, we may admit for oxalic acid the formula,—



which is probably the most rational manner of representing its composition; the oxalate of peroxide of iron, for instance, instead of—



becomes,—



and the oxalate of protoxide,—



The presence of *formic acid* among the products of distillation of oxalic acid authorises us to look upon oxalic acid as containing hydrogen. But, whilst admitting this, we cannot double the formula, as M. Wurtz has been obliged to do in supposing oxalic acid to be derived from glycol. In this theory oxalic acid is represented by $\text{C}_4\text{H}_2\text{O}_8 = 2\text{C}_2\text{HO}_4$; and it is evident that M. Wurtz's theory is completely erroneous, since if $\text{C}_4\text{H}_2\text{O}_8$ were the real formula of oxalic acid, it would be impossible to form salts such as those described in this paper.

On a New Principle in Volumetric Analysis.

In the *Annal. der Chemie und Pharmacie*, cxvi. p. 128, Dr. Mohr, of Coblenz, propounded a problem in volumetric analysis, and invited those interested in the subject to solve it. The first who did so was to receive one of Dr. Mohr's burettes as a prize. Dr. Mohr asserted at the same time that he had himself already solved it.

The problem was,—“To perform quantitative determinations without the use of weights, with volumetric solutions of unknown strength, and the strength of which must not be ascertained, and regulated.” Dr. Mohr received a number of communications, in which, however, some condition was violated; either a weight of some kind was used, or the strength of the test liquid was indirectly determined by saturated solutions of

chloride of sodium (as in Liebig's urea test), or in some other way.

The problem was first solved by Dr. Pauli, of the Union Alkali Works, St. Helen's, Lancashire, on December 28, 1860, in the following way. In one of the pans of a balance is put a piece of chemically-pure carbonate of soda, and in the other an equal weight of an undetermined soda, and both are measured by an acid of unknown strength. Suppose that in the first case 15 cubic centimètres, and in the second 11 cubic centimètres have been used, then, $15 : 11 :: 100 : 73.33$. The soda, therefore, contains 73.33 per cent. carbonate of soda.

The solution of the question, for which Dr. Pauli only cites a special case, is generally this:—

Equal portions of the pure substance to be determined and the impure are to be weighed off, one against the other, and are then to be measured with the same fluid; then the cubic centimètres used for the pure substance represent 100 per cent, and the other number proportionately less. Suppose, for example, that it is proposed to determine the iron in an iron ore. A piece of pure iron wire is placed in the scale of a balance, and is exactly counterpoised by means of the powdered ore, both are then brought into solution as protoxide in the usual way, and are then treated with permanganate of unknown strength. If oxide of iron is to be determined, pure oxide must be used instead of the wire. If potashes are to be examined, pure carbonate of potash (recently heated to redness) should be used. It would appear from this, for every analysis the strength must be determined by the pure substance. This, however, is easily avoided if we put a sixpence into the scale-pan, and weigh with this both the pure and impure substance. The number of cubic centimètres of the fluid holds good as long as the same fluid and the same sixpence are used, and this number may be marked upon the bottle as expressing 100 per cent. for the same substance.

This method is capable of universal application, and eliminates possible errors in weights and variations of temperature. It is only necessary that the substance to be determined should be available in the pure state. But how is the problem to be solved, if it remains as before, but with the further condition, "when the substance to be determined is not available in a pure state?" Dr. Mohr received two other solutions of the problem from Dr. Hiller and Herr Dietrich, both students in Heidelberg. Dr. Hiller has also solved the question, even with the condition that the pure substances should not be available, and in the same way as Dr. Mohr had already done it. For example,—if no chemically pure peroxide of manganese can be obtained for a manganese determination, according to Dr. Hiller, pure permanganate, or pure bichromate of potash, can be employed to weigh off the manganese; both are then converted by distillation with hydrochloric acid into chlorine, and then into iodine, and both fluids are then to be determined with the same unknown solution of hyposulphite of soda. Instead of using pure permanganate, it would be better perhaps to use pure iodine to counterpoise the manganese, then to dissolve the iodine in iodide of potassium, and proceed as before. We have now to convert the value of the iodine into the value of an equal weight of pure peroxide of manganese, and express it in the cubic centimètres of hyposulphite used. As 43.57 MnO₂ set 127 iodine at liberty, therefore any given weight of MnO₂ would set at liberty $\frac{127}{43.57}$, or 2.915 times as much iodine as is employed to counterbalance it. We have, therefore, to multiply the number of cubic

centimètres of hyposulphite which have been used for a quantity of iodine equal to the manganese by 2.915, and then to proceed as if pure MnO₂ and common manganese had been weighed together.

If pure carbonate of soda cannot be obtained, pure carbonate of lime may be used; the cubic centimètres of the acid used must be multiplied by $\frac{40}{33}$, that is, 1 atom of carbonate of lime divided by an atom of carbonate of soda.—*Annalen der Chemie und Pharmacie*, cxvii. 386.

TECHNICAL CHEMISTRY.

On the Chemical Composition of Cast Iron and Steel,
by M. FREMY.

(Continued from page 346.)

To establish clearly the existence of a nitrogenised charcoal, I have had recourse to a synthetic experiment which clears up all doubt on the subject. I made choice of a very pure organic matter,—for instance the best quality of sugar, nitrogenising it according to the invaluable directions given in a recent communication by M. Paul Thenard, and then submitting the compound to prolonged calcination—the charcoal thus obtained is highly nitrogenised, and disengaged ammonia for a long time by the action of hydrogen.

These experiments prove that organic nitrogenised substances, such as those composing animal and even vegetable tissues, will by calcination yield a nitrogenised charcoal, which by slow combustion, like that produced in cementation cases, will then disengage its nitrogen in the state of ammonia under the influence of hydrogen or of aqueous vapour. Nitrogenised charcoal is then a sort of magazine of nitrogen suitable for cementation; it decomposes with a degree of slowness favourable to the operation itself, and is perhaps one of the conditions of the quality of the product.

The facts I have brought before the Academy will probably be utilised in manufacture, and will account for certain practical operations well known among steel-workers. Thus all the organic bodies which produce rapid acieration, such as horn, soot, leather, animal excreta, are precisely those which yield highly nitrogenised charcoal. Ammoniacal compounds can nitrogenise even ternary organic substances.

The utility of nitrogen in charcoal explains a very curious practical fact, which is, that the charcoal of the cementation cases, after having been heated for a certain time, becomes exhausted, and requires to be replaced by fresh charcoal. The old theory, which assigned to carbon an exclusive part in cementation, could not account for this fact, admitting that the nitrogen is in the end carried off by the action of hydrogenised bodies, which, according to my novel views, is perfectly intelligible. The nitrogen for converting iron into steel is then yielded both by air and by wood charcoal, which always contains nitrogen. A notable quantity of nitrogen may even pre-exist in iron; the purest sorts of iron, and which are most easily convertible into steel, are just those containing nitrogen.

By submitting certain kinds of iron to the action of essence of turpentine vapours, I have sometimes produced an evident degree of acieration. As an explanation of this phenomenon, I have always found on examining the metal afterwards, that it contained nitrogen. Thus, those chemists who deny the influence of nitrogen in producing steel with carburets of hydrogen or diamond, are greatly at fault, for ordinary iron often

retains sufficient nitrogen to form steel by the action of a simply carbonising body. Moreover, I shall by-and-by prove that nitrogen can be replaced in acieration by another metalloïd bearing some chemical analogy to it, such as phosphorus, and yield compounds which may easily be confounded with normal steel.

After showing that steel is always produced in presence of nitrogen, the influence of this body being proved by laboratory experiments and practical observations, I must now inquire in what way nitrogen influences acieration.

As I said in a preceding communication, the action of nitrogen in acieration is twofold; not only does it combine with the iron, but it renders the metal porous, and makes clear the hitherto obscure phenomenon of cementation, namely, how a solid body can penetrate a metallic mass.

The most distinguished savants engaged in enquiries touching acieration know that cementation is a fact difficult to comprehend. I am about to prove that the properties of nitride of iron permit the most simple explanation of this penetration of the metal. In fact, it has been shown that nitrogen furnished by ammonia can penetrate a mass of iron and produce the compound I have studied under the name of nitrogenised iron; this body submitted to the action of hydrogen is reduced, leaving the metal remarkably porous. Steel is produced by heating this nitrogenised iron in a current of carburetted hydrogen gas; a portion of nitrogen remains combined with the metal; carbon can then penetrate the pores produced by the departure of nitrogen and the disengagement of hydrogen or of ammonia.

All the mysteries of cementation are thus easily explained; the numerous blisters characterising cementation steel—blistered steel, as it is called—the formation of which it was impossible to explain by the old theory, are now intelligible: they are produced by gases resulting from the action of hydro-carburised compounds on nitride of iron. Thus, the chemical phenomena of cementation may be summarised in the following manner:—Ammonia produces nitrogenised iron in disengaging hydrogen, which renders the iron porous; the hydro-carburetted gases then decompose the nitrogenised iron, acting by their hydrogen and carbon; excess of nitrogen is disengaged in the state of ammonia or cyanide of ammonium, which increases the porosity of the metal; while the carbon unites itself to the remaining nitrogen, forming the nitro-carburetted compound which seems to be the element essential to the composition of steel. This twofold action of nitrogen, which M. Despretz's experiments foreshadowed, is in a theoretical point of view an exceedingly interesting fact.

Is it not curious to see nitrogen, whose chemical activity is usually so little developed, combining with two bodies present in cementation cases, playing the part of a shuttle, and becoming a powerful cementation agent? It is impossible to consider the important part which I make nitrogenised iron play in cementation as a theoretical idea unconfirmed by experience. It cannot be forgotten that nitrogenised iron is produced at red heat, and that I have maintained it at that temperature for ten hours without its decomposing, and that, when afterwards submitted to a carburising influence, it has formed steel. These are precisely the conditions which we find realised in cementation cases.

I have now to inquire whether, in the fabrication of steel, by refining cast iron, nitrogen exerts an influence as important as in cementation. I first prove that in steel produced by puddling, or by a small furnace, the

presence of nitrogen is easily shown, either by making a current of hydrogen act on the filings, and so disengaging ammonia, or by examining the nitrogenised residue resulting from the action of acids on this kind of steel.¹

Steel produced in a small furnace, in contact with wood charcoal, and, consequently, under circumstances in which cyanides are readily formed, the nitration of iron and its transformation into steel can easily be understood.

In puddling furnaces acieration is sometimes due to the nitrogen contained in cast iron, but chiefly to the action of nitrogenised compounds furnished by the combustible, and the air, which acts rapidly on the iron when it commences to become red, porous, and in a nascent state.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, March 8, 1861.

Discourse on some Phenomena attending Combustion in Rarefied Air, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

THE results which I shall endeavour to lay before you this evening originated in a series of observations which I undertook when accompanying Dr. Tyndall in the ascent of Mont Blanc in the autumn of 1859. I then took with me a number of candles—half-a-dozen stearine candles, in fact—which were carefully weighed in Chamounix before the ascent, and were burned there for the space of an hour. These candles were subsequently taken to the top of the mountain, and were there carefully burned again, with the complete exclusion of draft for the space of another hour. On returning from the summit the candles were again carefully weighed, and the difference between the two results would indicate, of course, whether an increase of atmospheric pressure had had the effect of causing the combustion to go on more rapidly; or, on the other hand, whether rarefied air had the effect of decreasing the rapidity of combustion. We might naturally suppose that, inasmuch as at the summit of Mont Blanc we had not much more than half the amount of air, in the same bulk that we had at the level of the sea, that we should meet with a diminution in the rate of combustion of such materials as candles. The results of those experiments which, as I have said, led me to the investigation forming the particular subject of my discourse this evening, are contained in this table, which shows the difference in the rate of combustion of the same candles at Chamounix and at the summit of Mont Blanc.

Rate of Combustion per Hour.

At Chamounix.			At the Summit of Mont Blanc.		
1st candle	lost	9.2 grammes	1st candle	lost	8.7 grammes
2nd	"	" 9.9 "	2nd	"	" 9.5 "
3rd	"	" 9.2 "	3rd	"	" 9.2 "
4th	"	" 10.4 "	4th	"	" 8.8 "
5th	"	" 9.5 "	5th	"	" 9.3 "
6th	"	" 9.2 "	6th	"	" 9.0 "

If we add up these results together and take the mean we shall find that the rate of combustion at Chamounix was

¹ Chemists wishing to prove the presence of nitrogen in steel by means of hydrogen must avoid the numerous influences which decompose ammonia. They must, besides, seek for nitrogen when not rendered evident by hydrogen, either in the liquids proceeding from the action of acids on steel, or in the brown, insoluble substance resulting from this action. Very pure hydrate of potash can also, when hot, disengage the nitrogen contained in steel.

9.6 grammes per hour; and at the summit of the mountain the rate of combustion was 9.1 grammes per hour. But you see that one of these candles No. 4 yielded abnormal results. There was evidently something the matter with this candle which caused it to burn an abnormal quantity of stearine at Chamounix, and the quantity burned was a trifle smaller at the summit of the mountain than that burned by the others. It would, therefore, I think, only be fair to leave this fourth candle out of the question, and then we arrive at this average result, omitting the fourth candle, that the quantity of stearine burned was 9.4 grammes at Chamounix, and at the summit of the mountain 9.2 grammes per hour. So you see we can assure ourselves by this result, that the rate of combustion is entirely unaffected by the pressure of the atmosphere, and that candles and combustibles of that kind burn with exactly the same consumption of combustible matter under one circumstance as under the other. Whilst making these experiments in the tent at the top of the mountain, where we remained twenty-two hours, I was very forcibly struck with the difference in the character of the flame. The flame of an ordinary stearine candle with which we are all very familiar, exhibits generally this phenomenon—we have at the lower part of the flame a blue portion which does not, however, rise to within a quarter of an inch of the apex of the wick, there it terminates, and at this point the luminous portion of the flame commences. When the same candles were burned at the summit of the mountain, I noticed that this blue portion of the flame now extended to about an eighth of an inch above the apex of the wick, consequently the candles gave a very much smaller amount of light. The luminous effect was obviously, to the most casual observer, considerably inferior to the luminosity of the same candles at the base of the mountain. This led me on returning to England to institute a series of experiments upon this subject, and I found that on burning a candle in air rarefied to the extent to which it was rarefied on the summit of Mont Blanc the illuminating power of the candle diminished rapidly, and to a very considerable extent. The experiment was made in the apparatus here upon the table, and I cannot do better than repeat it in the way in which it was made. Here we have our stearine candle enclosed in a glass cylinder in connection with a powerful air-pump, by means of which we can obtain any amount of rarefaction within the cylinder. External air can be admitted below, and the products of combustion be carried off by a glass chimney, or cylinder, which is about an inch and a-quarter in diameter, and is connected with this tube, so that the products of combustion are thus carefully removed, and the candle is kept constantly burning in pure air, a condition which is obviously quite essential to the proper performance of the experiment. Now we will endeavour to light our candle. [The exhaustion of the cylinder was then commenced.] We have already a rarefaction of air to the extent of some twenty-four or twenty-five inches I believe. We have lighted our candle. We have here a stop-cock, which will enable us at pleasure to regulate the supply of external air. The candle is now burning at the ordinary atmospheric pressure, and giving its ordinary amount of light. You know when we first ignite a candle, it burns with a dull flame, or a small flame at all events, until a certain quantity of stearine has got melted. Whilst the flame is improving I will project upon the screen the image of a vacuum gauge by which you will be able to see the tension of the air which we have in our exhausting apparatus during the different stages of combustion. [The lights were then turned down.] There [pointing to the image on the screen] we have our vacuum gauge, which I hope will be visible even to those at a distance. This is a column of mercury which is gradually rising. We will take our barometer to be at 30 inches, the usual atmospheric pressure. The gauge is

now, you see, at 22.5, the next figure being 22, therefore we have a pressure of about 8 inches; and at this pressure we will introduce our burning candle. You see now how I am reducing the pressure, and notice the effect upon the luminosity of the candle. Now our candle, you see, is burning under a pressure of about 8 inches of mercury, instead of about 30 inches, and if we had the means here of taking the illuminating power there can be no doubt that we should find we have now a very small fraction of the light given by this candle before the exhausting process took place. Now we will gradually allow this pressure to increase. In fact, we shall find it necessary to let a little air in. You see it is gradually rising—the mercury is gradually falling. We had first 22.5, now 22, and now it is going down to 21. Now, our candle is burning in a pressure of 10 inches, and you see the pressure is gradually increasing. You will notice that we have a corresponding increase in our illuminating effect. Our candle, which was barely luminous at all under a pressure of 8 inches of mercury, is now considerably more luminous when that pressure is increased to something like 18 inches, at which it now stands. Now we have a pressure of 18 inches—in fact, a little more. There we have now about the light that the candle gave at the summit of Mont Blanc, and which is obviously very different from the light of a candle, burning with its usual brilliancy. We will just restore the candle to its original atmospheric pressure, and without disturbing the equilibrium of its combustion. Now that our candle is burning under atmospheric pressure, you see what a very different result we have with regard to the light that is emitted.

I did not call your attention to a phenomenon which is perceptible at a short distance from the candle when the illuminating effect is diminished to its lowest point—a phenomenon which is very beautiful if it can be seen; but, unfortunately, it cannot be seen at a distance. It is this: when the luminous portion of the flame of the candle has almost entirely disappeared there suddenly appears, round the nucleus of the flame, another flame which had hitherto been invisible if it had existed at all; it extends to the width of about a quarter to half an inch on each side of the flame, and it is of a very beautiful purple tint. It can only be well seen when the light from the centre part of the flame is cut off by holding the finger or a piece of card before the flame sufficient to intercept the rays from the centre part. When that is done this beautiful colour in the external coating of flame comes out very beautifully. This coloured flame I have examined by means of the prism, and it appears to possess those rays which are permitted to pass by a solution of permanganate of potash, although it contains rather more blue rays, apparently, than the light transmitted by the permanganate; I will endeavour to show you the kind of rays which are perceptible in the flame of the candle by means of the prismatic spectrum. [The gas-lights were again turned down.] Now, we will endeavour to throw upon this screen, first of all, the prismatic spectrum itself containing all the usual colours and then we will cut off the necessary colours. There, we have this beautiful spectrum which, you see, contains at this extremity an intense red ray, then orange, then yellow, then green, then blue, then indigo, and finally violet. Now, the external flame of the candle which is seen so beautifully when you are near to the candle, and when, through the rarefaction of the air the luminosity of the flame has become almost extinct, contains a combination of the rays from the two extremities of the spectrum; it contains a portion of this red ray and it contains a portion of this violet ray. I will now introduce a solution of permanganate of potash which, as I have said, resembles the colour of this outer flame. [A beam of electric light was passed through a solution of permanganate of potash, the two colours of which were thereby shown upon the

screen.] These are the colours of which the external coating of the flame is composed. If we have a mixture of these two colours, red and violet, you will get something like a notion of the colour of this external flame of the candle which is seen when the luminosity has almost disappeared. Here we have something nearly approaching it. Now, this effect is very curiously connected with a result that was first observed by Mr. Gassiot in the transmission of electric sparks through highly rarified air or highly rarified gases. Mr. Gassiot found that when these gases contained even a trace of nitrogen that the colour at once became of this pinkish hue—became quite similar to what we have observed in the external flame of the candle. I cannot do better, perhaps, than just show you this colour, which is, perhaps, more than any other colour like that of the external flame; in fact, I believe the cause of the colour in both cases to be identical—namely, the ignition of nitrogen; and that this spectrum which I have been showing you on the screen, is really the spectrum of incandescent nitrogen. Here we have nitrogen in this tube which has been highly rarefied; it does not contain more, perhaps, than from one-tenth to two-tenths of an inch of mercurial pressure. [The rarefied nitrogen was contained in a long cylindrical glass jar, through which a series of electric sparks were then transmitted.] There is the light in question which is, I believe, due to exactly the same cause as that light surrounding the candle flame—namely, the incandescence of the nitrogen in both cases.

Upon commencing these experiments so as to bring them into strictly quantitative form, and ascertain with something like precision what is the actual diminution of light which takes place under these circumstances, I soon found that candles were perfectly useless for this purpose, and that when placed under these artificial conditions, candles, notwithstanding the quality of the materials of which they may be made, are liable to gutter; they burn with different lengths of wick, and the results, which occupied me several weeks in obtaining, were altogether unreliable. I therefore turned to gas, as being far more manageable and capable of uniform combustion under these, so to speak, abnormal conditions. We can, by means of a gas meter like this, regulate the supply of gas into this apparatus, and thus we have at our command a known quantity of combustible matter to be consumed in a certain time. The light emitted was measured by Bunsen's photometer, an instrument which is very well known. It consists of a central disc of paper, which is rendered transparent in the greater part of it by means of a solution of spermaceti in turpentine; and if this disc be placed between two sources of light in an otherwise dark room, and you move it until the illumination of both sides is equal, that equality of illumination will be manifested by the disappearance of that comparatively non-transparent spot of unsized paper in the centre of the disc; and by these means it is quite possible to determine, and with very considerable precision, the illuminating power of any flame. Here, to describe the apparatus employed, is a gas flame the combustion of which was rendered quite constant by passing the gas through a governor, regulating the pressure of the gas. Then the experimental gas flame was placed at the opposite extremity of the instrument, and this was graduated by a scale, so that on moving this disc, both sides of it could be simultaneously seen. This is, in fact, Bunsen's photometer. The first series of experiments was commenced with a pressure of 29.9 inches of air, and in each case I assumed the light of the experimental flame to be equal to 100, for the purpose of comparing the results. On reducing the pressure to 24.9 inches, the amount of light was decreased to 75. On reducing the pressure to 19.9 inches, the illuminating power was reduced to 52.9. Then, on reduction to 13.6 inches of pressure, the illuminating power was reduced to 20.2. On reducing it to 9.6,

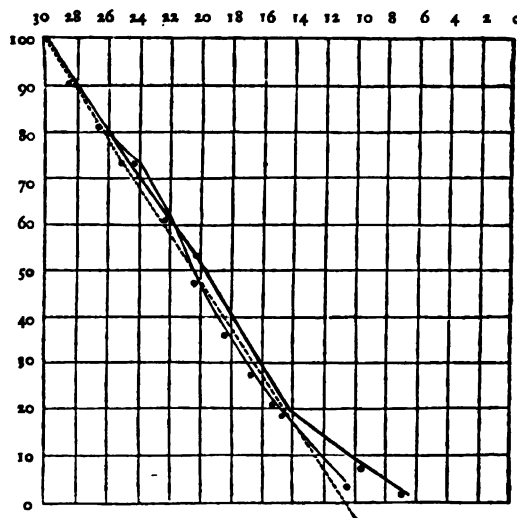
the illuminating power was reduced to 5.4, and finally, when the pressure was reduced down to 6.6, the illuminating power fell to 0.9, the fraction of a unit; so that you see we have decreased from 100 units of light down even to less than one unit. The light emitted by the combustion of the same kind of gas under this reduced pressure was not $\frac{1}{10}$ th of the light under the ordinary atmospheric pressure.

This is the second series of experiments which were made.

Second Series.

Pressure of Air.	Illuminating Power.	
	Observed.	Calculated.
30.2	100	100
28.2	91.4	89.8
26.2	80.6	79.6
24.2	73.0	69.4
22.2	61.4	59.2
20.2	47.8	49.0
18.2	37.4	38.8
16.2	29.4	28.6
14.2	19.8	18.4
12.2	12.5	8.2
10.2	3.6	— 2.0

Each number is the result of the average of twenty observations—not merely a single observation, but the average of twenty in each case. This table goes down to 10.2 inches of pressure. Now, the result of these observations was this—that beginning at atmospheric pressure, and with 100 units of light, we decrease almost exactly at 5.1 units of light for each inch of mercurial pressure we diminish in coming down the scale. The results of these two series of experiments are seen upon this diagram, which will give you a clearer idea of the nature of these results and the accordance with each other, than any mere inspection of the tables.



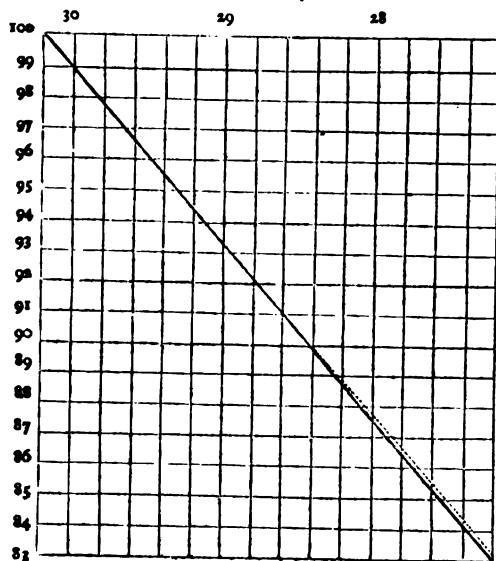
In this table the amount of light is represented by the horizontal lines. Here, beginning with no light at all, at the bottom of the table, you have 10, 20, 30, 40, 50. The perpendicular lines represent the amount of pressure—30 inches, 28, 26, 24, 22, 20, and so on. The black diagonal lines show the results of the two series of experiments. You see, therefore, that if the illuminating power of the flame had been constant for all pressures, then this line, instead of running down here diagonally, would run straight across the diagram. If, on the other hand, the illuminating power decreased

exactly in the same ratio as the pressure—for the distance from 100 to nothing is exactly the same as from 30 to nothing on the horizontal line here—then this line would proceed from this angle direct to the opposite angle [pointing]; but you see the light diminishes in a more rapid ratio than the pressure. You find, also, another peculiarity—that, when you get down to something like 15 inches of pressure, there is a sudden divergence in that direction [pointing to the right of the diagram], showing that the light does not then decrease in so rapid a ratio as at a higher pressure; in fact, that, from 14 or 15 inches downwards, the decrease in the illuminating power is considerably less rapid than from 30 inches down to about 14 or 15. Taking, however, the atmospheric pressure down from 14 inches, we have then a series of descending steps, so to speak, representing, for each unit in the diminution of pressure, a diminution of nearly 5·1 units of light. We have upon these tables the calculated results upon this assumption, and you see how nearly they coincide with the observed diminutions in the experiments. Now, as the diminution of light for a comparatively small diminution of pressure is so great when we are operating nearly at atmospheric pressure, it became interesting to ascertain, by as delicate a series of experiments as could be made, the variations within three inches of mercurial pressure, and they were made in the same way as the above, but with a water-pressure gauge, so that the readings were considerably more delicate. The following figures are the result of twenty observations under each pressure:—

Third Series.

Pressure of Air.	Illuminating Power.	
	Observed.	Calculated.
30·2	100	100
29·2	95·0	94·9
28·2	89·7	89·8
27·2	84·4	84·7

Here, in the second column, you have the observed numbers, and in the third the calculated numbers, at the rate of a diminution of 5·1 units of light for each inch diminution of pressure. You see the line is almost perfectly straight, and coincides so nearly with the divergence which is represented here by this dotted line that somewhere about half way up the diagram the two lines coalesce and remain together during the rest of their passage to the top corner of the diagram. In both diagrams the dotted line represents the calculated numbers.



These, then, being the results of the combustion of gas and candle flames in rarefied air, some questions naturally arise as to what can be the cause of these phenomena. Before answering these questions, it would be well for us, perhaps, for a moment or two to glance at the conditions under which light is evolved from flames in general. In gas and candle flames the light is due to two sources; namely, first, the incandescence of gaseous matter, and secondly, the incandescence or glow of solid matter deposited within the flame. Now the light due to the incandescence of gaseous matter is so slight—does not certainly amount to more than 1 per cent. of an ordinary gas or candle flame—that it may be left out of consideration altogether, and it only comes into prominent play when we get to a very high degree of rarefaction, but it is a light which suffers little diminution, and hence it causes the experimental lines in the diagrams to diverge at low pressures from the calculated line. The incandescent gaseous matter retains the total luminiferous effect higher than it otherwise would be if this light from gaseous incandescence were subject to diminution by the rarefaction of the air. Here is one of the incandescent gas flames—namely, hydrogen. We have two of them in coal gas—namely, hydrogen and carbonic oxide. You see the light from that large amount of flame is scarcely worthy of consideration at all; but if we introduce into the flame some solid matter which is capable of being heated to incandescence, we shall at once obtain a much more powerful light—we shall, in fact, feed this burner with exactly the same hydrogen gas from the same source as that from which the other burner is fed, and the only difference in the two experiments will be, that into this hydrogen flame we have introduced a quantity of platinum wire coated with a little film of lime, which is obtained by burning upon the wire paper that has been immersed in chloride of calcium. [The lights were then turned down.] Here we have now the same flame with this solid matter immersed in it, and you see the difference in the amount of light emitted at the same temperature, for the flame has not increased in temperature—it has perhaps rather diminished in temperature by the presence of this platinum gauze. [The lights were then turned up.] Thus the presence of solid matter is one condition for the production of a considerable amount of light in flame. But it may be objected that if we require solid particles of matter in a flame—if we assume that in ordinary flames we have solid matters present—those solid matters ought to be capable of being perceived, and that flame, in fact, ought to be to a certain extent, opaque, owing to the presence of those solid matters floating within it. Such is, however, not the case. Flame is almost perfectly transparent; if devoid of smoke, it is practically quite transparent. I will endeavour to show you this by ascertaining whether we cannot, through a considerable body of flame, actually read a newspaper. I will just illuminate a printed paper by means of the electric lamp, and project an image of it upon the screen. There we have, I think, a tolerably readable page. I think no one would have very great difficulty in reading that. Now, I will interpose this gas-flame between the paper and the screen. You see there is an image of the flame upon the screen, but it does not in the slightest degree interfere with the reading of the paper. The transparency is so nearly perfect as not to affect the legibility of these printed letters. So that this mass of flame which appears to have substance in it, is to all intents and purposes, transparent. You may make the experiment yourselves if you hold a piece of printed paper very near a gas-flame, and look very closely at it; you can read through the thickest portion of the flame without difficulty. But we may prove in another way, and by means of the electric lamp, that solid particles may be and really are carried across a perfectly transparent space, and if I project the image of these carbon points from which we

obtain our light, on the screen we shall see between them a perfectly transparent portion, apparently quite free from solid matter, but through which particles of solid matter in great quantities are constantly passing. Dr. Tyndall will be kind enough to throw these points upon the screen, and then we shall have the luminous image of the prisms of carbon which are brought nearly into contact, and between which the electric current is passing, bridging over the space; there we have them shown on the screen. I want you to notice the lower of these points; you see how it is growing—how there is a sort of branching out of that point towards the upper one, which is caused by the transference of the particles of carbon from one point to the other. The space between the points is perfectly transparent just as our gas-flame was, although these particles of carbon in the solid form are being carried over. See how they are growing upwards from the lower point, and how the upper one is wasting. That growth will take place through a considerable space of air, which, however, remains transparent all the time. The light of flame is also greatly dependent upon the temperature of the flame. With the same amount of solid matter floating in the flame the light augments by an increase of temperature, and *vice versa*. [The lights were then turned up.]

These being, then, the general conditions upon which the light of flames depends, the first question that will naturally arise to our minds will be this,—Is the combustion which takes place in rarefied air a perfect combustion; because if it be an imperfect combustion, for a given amount of combustible substance consumed, the quantity of light would be considerably less than that which would be otherwise obtained? I have therefore investigated the point with considerable care, and having extracted the gases that were passing off from the flame, both when the candle was burning under ordinary atmospheric pressure, and when under a pressure of eight inches of mercury, I found that in both cases the combustion was perfect. There was not a trace of carbon escaped unconsumed; therefore the combustion, both in atmospheric and rarefied air, was perfect. In fact, if I may be allowed the expression, the combustion in rarefied air was more perfect; for there was a more rapid conversion of the elements of combustible matter into their final oxidized products—carbonic acid and water. This question, therefore, as to whether the diminution of light is caused by an imperfect combustion, must be answered in the negative. The next question that will arise is this,—Is the diminution of light caused by a diminution of temperature? It is well known that if you allow air to escape out of a vessel into another vessel containing rarefied air, or into another vacuum vessel, there is a considerable diminution of pressure in the one from which the air escapes, and it might not be unfairly concluded that the temperature does suffer in this way. But I believe this diminution of temperature, from the cause just mentioned, is practically very small, and is neutralized to a great extent by the smaller cooling effect of the atmosphere; the atmosphere being more rare, the quantity of heat taken away from the flame, and the amount of heat carried away by convection, is smaller than in ordinary circumstances. That this is so, may be rendered evident by a very simple experiment. We will ignite a platinum wire in such a way under the receiver that the incandescence shall be scarcely apparent. [The platinum wire was then rendered slightly incandescent by a current of electricity from six of the battery-cells.] You will notice now the extent to which that wire glows while we extract the air, and I think it will be noticed that as the rarefaction goes on, the intensity of that glow will increase. If you notice the illuminating effect within the jar, you will find it very considerably increased by this rarefaction of the air. Here we have in this electric current a constant source of heat, but the cooling effect of the air is diminished by the rarefaction,

and consequently the wire becomes hotter, owing to the less amount of heat being extracted from it. This is the case with the flame of the candle, and this effect makes up nearly for the somewhat lower temperature that is produced by the combustion of the flame, so that this question must also be answered in the negative.

Now we come to a third question, namely, Is the diminution of light due to admission of atmospheric oxygen into the interior of the flame? If atmospheric oxygen could gain access to the interior of the flame then it would undoubtedly have the effect of destroying these incandescent particles of carbon which are now in the interior of the flame, and consequently of withdrawing the very source upon which $\frac{2}{3}$ ths of the light depends. It is well known that air introduced into a gas flame has the effect of gradually diminishing the light. We have here an ordinary gas-flame giving the usual amount of light, and an arrangement by which we can show the effect of atmospheric oxygen upon the flame. Our supply of gas is regulated by this stop-cock. The gas passes along this tube and into the burner. Here we have a second tube inserted into the gas-tube, through which we can throw atmospheric air contained in this air-holder. Notice the remarkable effect upon the flame. We have got now the maximum illuminating power. Now I have admitted the smallest possible quantity of air. It has not yet reached the flame, but it will presently. You see at once, when the air comes into the flame, how gradually the size of the flame is reduced. If we had the means of measuring the illuminating power, we should find that power reduced also. We will now admit a further quantity of air, and you see, as soon as it reaches the flame, how the light is reduced. There is no doubt that this is the principal, if not the only cause of the diminution of light in these gas and candle flames. But how does the oxygen gain access to the interior of the flame in greater quantity in rarefied air than it does in condensed air? The cause is very easily explained. If you have a very rare or light gas, the particles of that gas are capable of moving about in all directions with very much greater facility than the particles of a comparatively dense gas. Hydrogen is very remarkable for what is termed its "mobility"—its power of intermingling itself with other gases. A jar of hydrogen left for a few moments with its mouth open downwards, exposed to the air, will lose all the hydrogen contained in it. The gas will rapidly pass out to the external air, even though the mouth of the jar be downwards, and consequently the gas has to go out against its own comparative lightness, and to make its way against gravity downwards. So when you rarefy the air contained in this vessel, the particles of the air are far more mobile than when the air is at the ordinary pressure, and they gain access into the interior of the flame with far greater facility. It has been found that when a candle or gas flame burns in air at the ordinary pressure, that as much as from 60 to 70 per cent. of nitrogen gains admission into the interior of the flame. Now, if we withdraw the gases from the interior of the flame as far as the flame remains blue, you find oxygen present. It is only when you get higher into the luminiferous portion of the flame that there is nitrogen and no oxygen. The presence of oxygen in the flame determines at once the combustion of the carbon particles, which constitute the source from which the light emanates under ordinary circumstances.

I feel it necessary to apologise for having brought these simple experiments before you. I have done so in reliance upon that interest which I know those who weekly assemble here feel in any new scientific fact, however abstract and technically valueless it may appear to be. I do not know that any useful application can be made of this law regarding the reduction of illuminating power; but possibly, if we find that this effect extends upwards to higher pressures, some such application might be made.

I have only yet been able to make a few experiments in this direction, but so far as they go, they seem to indicate that the same law holds good for higher pressures, and that at two atmospheres we should have about $2\frac{1}{2}$ times the illuminating power which we have at the ordinary atmospheric pressure, thus greatly increasing the amount of light obtainable from a given quantity of combustible matter. These phenomena of flame did not escape Davy in his laborious and beautiful researches upon flame, although he does not appear to have paid more than a passing attention to the effects of pressure upon the light of combustion.

Weekly Evening Meeting, Friday, March 15, 1861.

A Lecture on Electrical Quantity and Intensity,
by LATIMER CLARK, Esq.

(Continued from page 355.)

The instances in which the quantity present is not simply dependent on the tension, are those in which other electrified bodies are present which, by their inductive influence, affect the quantity present in all bodies in their vicinity without necessarily affecting their tension. An insulated cylinder was connected with the positive pole of a Daniell's battery of 600 cells, its negative pole being connected with the earth; so that the cylinder was in a condition to give off a powerful and visible current to another wire connected with the ground; in this condition a positively electrified disc was approached to it and by its inductive influence was shown to render one end of the cylinder electrically negative so that a carrier ball applied to that end showed it to have a negative charge, thus presenting the apparent paradox of a negative electrified body giving off a positive current to the earth, or *vice versa*. One end was negatively electrified, and the other end positively, but the tension was the same everywhere.

According to the ordinary way of regarding this class of phenomena, it was usual to state that the ends of the cylinder acquired a state of positive or negative intensity, or that they had their intensity changed: it was contended that this gave an inaccurate idea of the real nature of the change, and that the approach of an electrified body, however near or however violently it might be excited, could not in the slightest degree affect the tension of a conducting body, which was in connection with the earth; the only influence it could have would be to alter the quantity in the second body, by driving a portion of its electricity downwards to the earth. It might be assumed as a law that the tension of the electricity in every part of a conducting body of moderate dimensions was the same, notwithstanding the vicinity of other electrified bodies. If a positively electrified body were brought near an insulated conductor, the distribution of the electricity in the second body was changed, and its whole tension was raised, but the tension remained everywhere uniform, and was as high at the negative as at the positive end.

The fall of tension in electricity was always accompanied by its conversion into heat; the ignition of wire by the voltaic current, the intense heat of the voltaic arc, and the heat and light of the electric discharge and of the spark, were all cases of the evolution of heat consequent on the fall of tension, and the quantity of heat evolved was apparently directly proportionate to the fall of tension within a given space and to the quantity of electricity passing.

In the case of electric telegraph conductors and submarine cables, it was shown, from a carefully-conducted and extensive series of experiments, that the tension falls with the most perfect regularity from the positive pole of the battery to the end in connection with the earth, in accordance with the law of Ohm; and since the quantity of electricity held under induction varies in the same ratio as the tension

the distribution of the charge in a cable follows precisely the same law. It results from this, that if a cable with a current flowing through it, be divided into any number of equal sections, and the quantity in the section connected with the earth be taken as unity, the quantities in all the other parts, whatever their number, will be in the ratio 1, 3, 5, 7, 9, 11, &c. So that if a cable be divided into two halves, the quantities will be in the ratio of one to three.

The Speaker stated that he had ascertained that, in the voltaic battery, the presence of two metals was not an essential condition—the negative metal was not necessary for the formation of the electric current, but only for its after detection and exhibition. A simple mass of copper, iron, zinc, or any oxidisable metal, when laid on the moist earth, formed a complete battery in itself, giving positive electricity to the earth, and quickly assuming a negative tension, which it would communicate to any other body resting on it or in contact with it, as for instance, a length of submarine cable. If a cable thus charged were afterwards removed and applied to any more electro-negative metal, such as platinum, or to carbon, the charge would return to the earth; and it was found by measurement that the charge thus acquired by a cable was exactly the same as if the two metals had been employed simultaneously in the ordinary form of a galvanic couple. If the mass of zinc were permanently connected with the non-oxidisable or less oxidisable metal, and thus with the earth the tension, being constantly destroyed and as constantly renewed, would form a constant current, becoming in fact a voltaic couple. If the connection with the earth, instead of being made through another metal, were made through any inert conducting substance or liquid, the same constant current would be produced, thus forming the well-known case of a voltaic battery with one metal and two liquids.

After the conclusion of the discourse, Mr. Clark stated, with reference to Faraday's discovery of specific inductive capacity, that in the course of some investigations conducted in conjunction with Professor Hughes, they had observed that every different dielectric possessed its own specific law of variation of inductive capacity with respect to distance. With air it varied as the distance, inversely; but with gutta-percha it was more nearly as the square root; with India-rubber and white wax it was intermediate between the two ratios, and the law of variation was different with every substance tried. From which it would result that observers who deduced the specific inductive capacity of any material from experiments on half-inch plates, would arrive at very different results from others who operated on inch plates.

CHEMICAL SOCIETY.

Thursday, May 2, 1861.

R. WARRINGTON, Esq., in the Chair.

T. Griffiths, Esq., was elected a Fellow.

Mr. JOHN DAVIDSON read a paper "On the Action of Dibromide of Ethylene on Pyridine." A mixture of the two substances with about one-sixth of its volume of alcohol was heated in sealed tubes to 100°C ., whereby a silky crystalline mass was obtained. Representing pyridine by the formula $(\text{C}_5\text{H}_5)'''\text{N}$, this new product would be the dibromide of ethylen-dipyridyl-diammonium, $(\text{C}_5\text{H}_5)''(\text{C}_5\text{H}_5)_2'''\text{N}_2\text{Br}_2$, corresponding to one of the principal products of the action of dibromide of ethylene on triethylamine, namely, the dibromide of ethylene, hexethyl, diammonium $(\text{C}_2\text{H}_4)''(\text{C}_2\text{H}_5)_6\text{N}_2\text{Br}_2$. By acting on the new dibromide with chloride of silver, the corresponding chloride was obtained, which formed with dichloride of platinum, the double salt $(\text{C}_5\text{H}_5)''(\text{C}_5\text{H}_5)_2'''\text{N}_2\text{Cl}_2 \cdot 2\text{PtCl}_2$. By treating the dibromide with freshly precipitated oxide

of silver, the corresponding dihydrate was obtained as a colourless powerfully alkaline but unstable liquid.

Professor BLOXAM read a paper "*On the Amounts of Water Expelled from the Hydrates of Potash, Soda, and Baryta, by Boracic and Silicic Acids.*" The Author referred to his former experiments, which showed that one atom of boracic acid BO_3 at a bright red heat expelled one atom of carbonic acid CO_2 from carbonate of potash, two atoms from carbonate of soda, two and a-half atoms from the carbonates of lithia and baryta, and three atoms from carbonate of strontia. Hence, he inferred that boracic acid was tribasic, but that in some cases it was unable to satisfy its saturating capacity, on account of the affinity of some of the bases for CO_2 . The affinity of these bases for HO , being less than those for CO_2 , he thought that by employing hydrates instead of carbonates he might be able to obtain tribasic salts with them also. The Author described minutely the precautions he had taken to insure accurate results, and to overcome the difficulty arising in the case of hydrate of potash from an absorption of oxygen. The boracic acid and hydrates of potash and soda were heated in a silver tray contained in a glass tube through which a current of pure air was passed. The boracic acid and hydrate of baryta were fused together in a silver crucible. It was found that one atom of boracic acid expelled two atoms of water HO from hydrate of potash, and three atoms from the hydrates of soda and baryta. The Author argued that inasmuch as the water of hydrate of baryta was more easily expelled than that of hydrate of soda, and inasmuch as boracic acid drove out no more water from the former than from the latter, it followed that its maximum capacity of saturation was satisfied by the three atoms of base with which it united in each instance. He observed that the formulæ of boracic ether $3\text{EtO} \cdot \text{BO}_3$, and octahedral borax $\text{NaO} \cdot 2\text{HO} \cdot \text{BO}_3 + 3\text{HO} \cdot \text{BO}_3$ were distinctly tribasic. Similar experiments were made with silicic acid, from which it appeared that one atom of SiO_2 drove out $1\frac{1}{2}$ atoms HO from hydrate of potash, and 2 atoms from the hydrates of soda and baryta respectively. Hence, the Author inferred that the capacity for saturation of silicic acid was satisfied by 2 atoms of base, and gave a list of well defined bibasic silicates in illustration. He also referred to the analogies of boracic and silicic acid, and observed that $\frac{1}{2}$ atom of boracic acid might be considered the representative of $\frac{1}{4}$ atom of silicic, whereby the weights of comparable quantities of boron and silicon would be to one another as 3.6 to 7, or as 1 to 2.

Col. YORKE was glad to find that Mr. Bloxam's experiments supported the conclusion at which he had formerly arrived, and which had also been advocated by Dr. Odling, as to the bioxygen formula for silica. Despite Schröder's late experiments, he could find no reason to look upon silica as a tetroxide.

Dr. ODLING said that according to his experiments 1 atom of boracic acid, BO_3 , drove out 3 atoms of CO_2 from carbonate of lithia, and considered that Mr. Bloxam had under-estimated the amount expelled, by a miscalculation of his results. He thought that boron and silicon did not belong to the same group of elements, but rather that they occupied somewhat comparable positions in different groups, and that as sulphur was related to tellurium, phosphorus to bismuth, and silicon to tin, so was boron related to aluminum. Representing 27 parts of aluminum by the symbol Al , we had the following comparable bodies:—

H_3BO_3 Boracic acid cryst.	H_2AlO_3 Alumina gelatinous.
HBO_2 Do. dried at 100°C .	HAlO_2 Diaspore.
MgBO_2 Boracite.	MgAlO_2 Spinel.

Boracite was isomorphous with spinelle, and BO_2 seemed to replace AlO_2 in several siliceous minerals. He considered that the two principal classes of borates were the monobasic, analogous to the metaphosphates, and the tribasic, analogous to the orthophosphates, but that inter-

mediate salts also existed. He preferred to double the atomic weight of silica, and consequently to represent the most highly metallised silicates as tetrabasic.

Professor CRACE CALVERT read a paper "*On the Graphite of Cast Iron.*" He showed that, when cast iron was slowly acted upon by dilute acids, and preferably by acetic acid, a black plumbago-like mass was left, so soft as to be cut with a knife, having a sp. gr. 2.5, and a constant composition. It contained about 80 per cent. iron, 11 per cent. carbon, $2\frac{1}{2}$ per cent. nitrogen, and 6 per cent. silicon. He adverted to the presence of nitrogen in cast iron, and thence inferred that mere nitrogen could not be the differentiating agent between iron and steel.

Mr. DE LA RUE and Mr. WARINGTON referred to the similar change of iron into a soft black substance which took place in an iron grating habitually exposed to the action of sour beer-dregs in a London Brewery. The grating became so soft that it eventually gave way by the mere weight of an empty cask.

The Author of the paper and Messrs. Riley and Malone then took part in a short discussion on the relation of nitrogen to acierification.

CORRESPONDENCE.

The Sale of Poisons.

To the Editor of the CHEMICAL NEWS.

SIR,—The discussion on the prevention of poisoning, caused by accident or otherwise, seems at length to have ended without any practical benefit having been derived from it.

It has been said of railway accidents, that no adequate means will be taken to prevent their recurrence until a Bishop has been sacrificed; the same may be perhaps, but too truly, said of poisoning, that until a Right Hon. Lord or a Right Rev. Prelate has, by ignorance or negligence, accident or design, come to an untimely end, we may look in vain for legislative aid or public interference.

Our own efforts with regard to this important subject are spasmodic in the extreme, chiefly confined to picking to pieces each successive measure as it comes before our Legislature, showing in the most glaring manner its impracticabilities and unreasonable clauses, bringing all our opposition to bear upon it, and ultimately crushing it; and then quietly return to our usual avocations, and gloat over our "success."

Is it not, then, a wonder that with such a perfect knowledge of what we do not want, and what will not be for the benefit of her Majesty's loving subjects; and with an almost unanimous knowledge of what we do want, and what will be for the public benefit, viz., the causing of all future chemists to pass an examination testing their ability and competency, and giving them the exclusive right of vending Poisons, that we do not more strenuously endeavour to obtain it?

These are the only legal restrictions necessary to prevent the recurrence of three-fourths, at least, of the accidents that have occurred, and it would be a guarantee to the public that entering a chemist's shop, that "the right man" would be found in "the right place." Of course I mean in the same way that we are certain that every F.R.C.P. or M.R.C.S. is perfectly competent,—the former to give advice regarding bodily ailments, the latter acquainted with the anatomy of our frame, and able to repair a broken limb or set right a dislocated joint.

To the objection that this would be a work of time, and that it would be no guarantee that those who are at present in business are competent to fulfil the duties required of them, I can only reply that all similar changes have been open to the same objection, and that change for the better is always a work of time.

The sale of poisons may be safely left in the hands of the educated chemist, no legal restriction is necessary with regard to the vending of this or that, for every chemist knows but too well that a summons before a coroner's inquest and a verdict censuring his conduct, is a certain forerunner to a presentation in all due form to the Commissioner of the nearest Court of Bankruptcy, and that he may as well put the shutters up at once. Enter any respectable chemist's shop, ask for two ounces of laudanum, and the intelligent proprietor or his assistant will, in all probability, ask you what it is required for, enquire regarding your knowledge of its properties, dose, &c., and unless the answers are perfectly satisfactory, he will, with the greatest politeness, decline to serve you, and the "word in season" has often, to my knowledge, proved of benefit. In a neighbouring town, a poor woman, a few months since, asked for two-pennyworth of oil of vitriol, and brought a cup to take it in: she was asked what purpose she required it for, and the answer was "her old man was very bad, he had a cough upon him for a long time, and she was told a teaspoonful in water was a very good thing for him,"—of course, she wanted elixir vitriol, which was given, and the proper dose told her; here, in all probability, a life was saved, yet the Humane Society presented no medal; I do not think he was even thanked; and he finds, as nearly all junior members of our fraternity find, quite enough to do to keep the wolf from the door. Innumerable instances of a similar nature may be given, but they are every-day occurrences, as every member of the profession knows.

But the man or woman who contemplates suicide by means of poison, does not procure it from a place questions are likely to be asked with regard to the purpose it is required for; they know full well that laudanum or oxalic acid, and kindred substances, may be obtained from almost any huckster's (whose knowledge does not even extend to the external difference of oxalic acid and Epsom salts, or the varied appearance of tincture of opium and tincture of rhubarb, and whose acquaintance with their properties is even less than that of their appearance), and that he will not be inquisitive, or even trouble himself with labelling the desired article. How often our efforts to prevent suicide are frustrated, the newspapers occasionally tell us; but in the great majority of cases there is an impossibility, from the absence of labels, to find out the vendor. Of course the nearest chemist gets the benefit of the blame: and I do think that, having to bear the blame, he has a right to claim the exclusive sale of the poison.

It would be, I believe, a fallacy to suppose that in a fallible world we can ever attain infallibility, and that as long as there are poisons people will be poisoned, in the same manner that as long as there are razors they will be used for other purposes beside the legitimate one of shaving; but we may reduce them to the minimum, and all suggestions aiming, or tending to attain that desired object, will ever be received and adopted by the majority of chemists.

The bottle invented by Messrs. Savory and Moore, or some practical modification, will prove, when better known, of great service. Labels are a great preventive of error, but labels do not fulfil every requirement, as all chemists know; two bottles were sent out, the one containing hartshorn and oil, the other a mixture, both properly labelled, but of the same size and shape, the hartshorn and oil was given instead of the mixture, the latter being of the same colour and composed of confection of almond, much the same in appearance as the former. In the South of England, a gentleman living in the country, and in the habit of keeping a few simple remedies by him, ordered his servant to take out an aperient draught from the medicine chest; but here again the laudanum was kept in a two-ounce vial, and the black draughts in bottles of the same size and form, and being very similar in appearance, laudanum was, through the carelessness of the man, given

instead of the draught, although the bottles were properly labelled, and the laudanum marked "Poison." The accident being immediately discovered, and proper remedies applied, both cases fortunately recovered.

I could give instances *ad infinitum*, but will conclude with one more for the benefit of those who consider the label a certain preventive of error.

In the suburbs of a neighbouring and one of the largest manufacturing towns there lives a retired minister of one of the numerous Dissenting denominations, who had an only son, who promised to be the solace and support of his aged parents, holding a responsible position in a large metropolitan warehouse. But over-work, bodily and mental combined, with a naturally weak constitution, so much enfeebled him that during the winter of 1859 he was obliged to go home, hoping the rest and purer air of his birthplace would be the means of re-establishing his health. But the work was done; that arch fiend Consumption had struck vitality to its very core. Medical aid was of no avail, except by the help of sedatives to palliate the distressing sufferings. The old man had taken upon himself the duty of administering the medicine at the proper time, and kept the bottles containing the physic on a shelf in an occupied room. Unthinkingly a bottle of bug poison was placed among them by the servant, unknown to the old man, and, like all men arrived at his age, his perceptive faculties were blunted, and so in the dark he poured out a wineglassful of the bug poison, and gave it to his son. The mistake was soon discovered from the odour, naphtha being mixed with the solution of bichloride of mercury, so as to render the taking by accident almost impossible. But the fatal work was done. A constitution weakened by disease was unable to bear the shock, and so he died. A pitiable sight it was to see and hear that old man tell his sad, unvarnished tale before the coroner and his jury, his white hair bleached by the sun of seventy summers, and big tears rolling down his furrowed cheeks; his useless but bitter regrets that any act of his should have hastened, if but for an hour, his son "where all have gone, and all must go." A verdict in accordance with the evidence was given. The bottle was properly labelled "Poison." But I do not think this accident would have occurred if the contents had been placed in a bottle similar in principle to Savory and Moore's.

I am well aware that what I have written has been frequently said before much more ably and more forcibly; but, with the immortal Locke, I think "the pictures drawn in our minds are laid in fading colours, and, if not sometimes refreshed, vanish and disappear." This is strikingly illustrated with regard to the important subject of poisoning. Hoping you will accept this as my apology for troubling you,—I am, &c.

WM. BENNINGTON.

30, Oxford-street, Liverpool.

Magenta Dye.

To the Editor of the CHEMICAL NEWS.

SIR,—Having, till now, overlooked "Magenta's" reply of the 18th May, and as that communication has not altered the opinions I expressed in my last, perhaps "Magenta" would have no objection to make his discovery advantageous to himself, by forwarding me a sample, with quotations, for a quantity, or I should have pleasure in affording "Magenta" every facility for trying it on Coburgs and Orleans on a large scale, and, if found a *bond fide* and practicable article, I need not say the advantage will be great, and no one would have greater pleasure to bear testimony to the fact than—Yours, &c.

JAMES SHARP.

Botterill's Dyeworks, Leeds.

Chemical Notices from Foreign Sources.

I. TECHNICAL CHEMISTRY.

Aich's Metal.—An alloy of iron, copper, and zinc, possessing extraordinary strength is described under this name (*Mittheilungen d. Gewerbever. f. d. Konigr. Hannover*: 1861, s. 59), the inventor being an Austrian marine officer. The specific gravity of the compound is said to be 8.40, but the proportions of the metals are not given. The colour of the metal is darker than brass. It can be worked more easily than iron, and almost as easily as copper. Its ductility is extraordinary, but its most valuable quality is its strength. Tested at the Vienna Arsenal, it was found, when cast, to be nearly as strong as Armstrong gun iron, but by hammering and rolling, its strength was almost doubled. Made into a tube and tested with gunpowder, it was found to have twice the strength of wrought iron, and five times that of bronze gun-metal.

Sterre-metal is another alloy somewhat resembling the foregoing, which it is proposed to use for ships-bolts (it not being acted on by sea-water) engraver's plates, bells, vacuum pans, &c. The composition of this alloy is not given. Both this and the foregoing may deserve the attention of English metallurgists.

Manufacture of Ultramarine.—C. Furstenau gives (*Dingler's Polytech. Journal*, Bd. cliv, s. 63) the following as a good method of making a dark ultramarine. He takes of

Lightly-burnt kaolin	100 parts
Calcined soda	90 "
Refined roll sulphur	100 "
Rosin	6 "
Dry pinewood charcoal	4 "

All these with the exception of the rosin are ground very fine. The rosin is then added in pieces the size of nuts, and the whole is ground together for four hours. The resulting grey powder is then placed in a clay oven and heated quickly to the melting point of an alloy of equal parts of gold and silver. After having been exposed to this heat for five or six hours, the oven is allowed to cool. After a couple of days the bluish-green mass is removed and ground fine in a mill. It is then roasted again in a cast-iron retort, where it assumes the blue colour. It is then finely levigated and dried for sale. The amount of product obtained from the proportions given is 122 of ultramarine.

II. ORGANIC CHEMISTRY.

Myronate of Potash.—Prof. Ludwig and W. Lange extracted this salt from black mustard, by the following modification of Bussy's process. (*Archiv. d. Pharm.* ii. Reihe. Bd. clv. s. 20.) They first extracted the fresh powdered mustard twice with hot alcohol, then treated the powder several times with cold water, evaporated the watery solution on a water bath, extracted the residue with hot dilute spirit of wine, and concentrated the filtered extract. From this the myronate of potash crystallizes out on standing. 500 grammes of mustard yielded one gramme of the salt, the analysis of which gave the formula $\text{KO C}_{20} \text{H}_{19} \text{NS}_2 \text{O}_{18}$, and appeared to be constituted as follows:— $\text{KO S}_2 \text{O}_4$ acid sulphite of potash + $\text{C}_6 \text{H}_5$ + Cy S_2 ; mustard oil, + $\text{C}_{12} \text{H}_{14} \text{O}_4$ sugar; or $\text{KO S}_4 \text{O}_4 \text{H}_2 \text{O}_2$ + $\text{C}_6 \text{H}_5$ Cy cyanide of allyle + $\text{C}_{12} \text{H}_{12} \text{O}_{12}$ sugar.

Myronate of potash boiled with dilute sulphuric acid, the acid being afterwards separated by lime, did not ferment on the addition of yeast. Trommer's test showed, however, the presence of sugar in the solution. When boiled with potash the solution of the myronate turns yellow, evolves ammonia, and, on the addition of hydrochloric acid, sulphuretted hydrogen. Rendered alkaline again, the solution reduces oxide of copper. Caustic

ammonia decomposes myronate of potash, forming a sulphate. Cold baryta water does not act on the myronate, but on boiling the mixture it becomes cloudy, and when hydrochloric acid is added, evolves sulphuretted hydrogen and carbonic acid, sulphate of baryta remaining undissolved. Some of the other reactions of myronate of potash are interesting. For instance, nitrate of silver at first produces no change, but after some minutes the mixture becomes cloudy, and in time a white precipitate falls. On boiling this precipitate dissolves, and sulphide of silver soon separates, a strong smell of the volatile oil of mustard being evolved showing the development of this body from myronate of potash, without the intervention of myrosine. Myronate of potash is easily soluble in water; the clear solution mixed with a colourless solution of myrosine soon becomes cloudy from the separation of sulphur, the smell of the volatile mustard oil being developed.

Composition of Pus.—Giesecke gives (*Ann. d. Chem. und Pharm.*, Bd. cxvii. s. 110) a very complete analysis of what may be called healthy pus. It was thick, pale yellow, faintly alkaline, had the sp. gr. 1022, and no smell. The composition was as follows:—

Water	88.76
Solid matters	11.24
organic matter	10.12
inorganic	1.12

100.00

The solid matter was composed of

4.38	albumen dissolved in pus serum
4.65	pus globules, mucus, with a little leucin and glutin
1.09	cholesterin, with some other neutral fat
0.59	chloride of sodium
0.32	soda (from albuminate of soda), a little phosphate of soda, and a very little sulphate of potash
0.21	phosphate of magnesia, lime, and oxide of iron.

11.24

MISCELLANEOUS.

Chemical Society.—The next meeting of this Society will take place on Thursday next, June 20, when a discourse will be delivered "On the Application of the Induction Coil to Steinheil's Apparatus for Spectrum Analysis," by H. E. Roscoe, Ph.D.

ANSWERS TO CORRESPONDENTS.

Q. S. T.—A notice was given in our first volume of a patent for obtaining a very pure salt of potash in quantity from Sulnit.

J. S.—They will be concluded if possible in the present volume.

P. T. de Tomas.—Consult an elementary work on decimals. 21.4 is a greater number than 21.15. The degree of temperature at which platinum melts is not known: it has been estimated at different times at from 3000° Fahr. to any temperature you like.

S.—In a week or two.

D. Thomas.—We can suggest no plan better than advertising your instruments for sale in our columns.

J. S. Pearson.—They all ultimately find their way to the sea. It will be some years, however, before all the fishes are poisoned, they will last our time.

Ammonia.—1. Liq. Vol. C. C. is generally made by diluting Nq. ammonia with distilled water. 2. We have had no experience in the preparation of spt. ammon. arom. with nutmeg, so cannot say whether it would colour as much as cloves.

The Solar Eclipse.—The following letter from a Correspondent may amuse if it does not instruct our readers:—"I hasten to offer a theory suggested by Professor Faraday's beautiful discourse on the Eclipse of July, 1860. (*CHEMICAL NEWS*, June 1.) The prominence 28,000 miles in length and 74,000 miles above the sun was a planet which struck the embossed surface of the sun and rebounded to that height, where it is now dissolving for the supply of the chromogeneous ether, which is the sun's great atmosphere, enabling him to speed on in his meteoric course, as our own atmosphere enables sound to move."—Kofe."

THE CHEMICAL NEWS.

VOL. III. No. 81.—June 22, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Expansion of Metals and Alloys, by F. CRACE CALVERT, Ph.D., F.R.S., F.C.S., &c., R. JOHNSON, F.C.S., and G. CLIFF LOWE.

(Continued from page 359.)

In order to test the correctness of our estimate of the expansion of the stone upon which some part of the apparatus was placed, we deemed it advisable to submit a specimen of the same stone to direct experiment, and, finding that its expansion exceeded that of several of the metals and alloys, we were induced to make further experiments in the same direction, and found that several minerals had a much greater amount of expansion than had been generally supposed. We are induced, therefore, to give here the results we have arrived at.

Chalk.

10° = 220	90° = 251	10° = 219	90° = 251	10° = 218.5
90° = 251	10° = 219	90° = 251	11° = 218.5	90° = 249.5
31	32	32	32.5	31.0
Mean for 80° = 31.7				
Mean for 100° = 39.6 — 20° correction = 19.6				

Lithographic Stone.

92° = 350	12° = 298	92° = 351
12° = 298	92° = 351	12° = 300
52	53	51
Mean for 80° = 52		
Mean for 100° = 65 — correction 20° = 45.		

Building Stone (Millstone Grit).

91° = 200	91° = 212	91° = 212	11° = 149	91° = 211.5
11° = 135	11° = 149	11° = 149	91° = 211.5	11° = 148.5
65	63	63	64.5	63
Mean for 80° = 63.2				
Mean for 100° = 79.0 — 20° correction = 59.0.				

Stalactite.

99° = 200	99° = 200	16° = 128	99° = 200
16° = 126	16° = 128	99° = 200	16° = 127
74	72	72	73
Mean for 83° = 77.75 (?) 73.0			
Mean for 100° = 87.6 — 20° correction = 67.6.			

Marble (Statuary).

91° = 300	91° = 300	91° = 309
11° = 225	11° = 227	11° = 237
75	73	72
91° = 315	11° = 243	11° = 244
11° = 243	91° = 317	91° = 317
72	74	73
Mean for 80° = 73.1		
Mean for 100° = 91.3 — 20° correction = 71.3.		

These results present two points of interest,—first, the influence which the molecular condition of a substance seems to exert upon its ratio of expansion; thus, between chalk and lithographic stone, on the one hand, and marble on the other, there is a difference of from 100 to 350 per cent.

The increase of expansion of the various carbonates of lime appears to correspond with their increased crystalline condition.

A further instance of this influence we have found in the sulphate of lime.

Plaster of Paris.

99° = 200	99° = 200	99° = 200	99° = 200
16° = 102	16° = 107	16° = 105	16° = 104
98	93	95	96
Mean for 83° = 95.5			
Mean for 100° = 115.0 — 20° correction = 95.			

Lenticular Gypsum.

99° = 350	99° = 350	16° = 240
16° = 235	16° = 240	99° = 348
115	110	108
99° = 348	99° = 350	16° = 247
16° = 245	16° = 247	99° = 350
103	103	103
Mean for 83° = 107		
Mean for 100° = 128.9 — 20° correction = 108.9.		

This influence seems to be general; for, besides the above series, we have drawn attention to it in the body of this paper in reference to the experiments made upon iron, steel, zinc, and the alloys generally.

The second point of interest to which we would draw attention is the high ratio of expansion which these salts of lime possess; for marble and gypsum considerably exceed the expansion of cast iron, steel, antimony and platinum.

We also obtained these unexpected results in the case of rock crystal:—

Rock Crystal.

99° = 250	99° = 250	12° = 163
12° = 158	12° = 163	99° = 250
92	87	87
99° = 250	12° = 163	99° = 251
12° = 164	99° = 250	12° = 163
86	87	88
Mean for 87° = 88.0		
Mean for 100° = 101.2 — correction 20° = 81.2.		

We think it right, before concluding this paper, to give the ratio of expansion of slate and glass, as they have been referred to as being employed by us in determining the amount of correction required to be applied to our mode of conducting the foregoing experiments.

English Tube of Flint Glass (Double Silicate of Potash and Lead).

12° = 200	92° = 264	12° = 198	92° = 261
92° = 264	12° = 198	92° = 261	12° = 195
64	66	63	66
12° = 195	92° = 258	12° = 194	92° = 256
92° = 258	12° = 194	92° = 256	12° = 192
63	64	62	64

Mean for 80° = 64

Mean for 100° = 80° — 20° correction = 60.

French Tubing Glass (Double Silicate of Soda and Lime).

11° = 250	91° = 312	91° = 311	11° = 248
91° = 312	11° = 248	91° = 248	11° = 311
62	64	63	63
91° = 311	11° = 247	91° = 309	
11° = 247	91° = 309	11° = 241	
64	62	62	

Mean for 80° = 62.8

Mean for 100° = 78.5 — 20° correction = 58.5.

Slate.

90° = 200	90.5° = 195	90.5° = 192	91° = 200	11° = 138
10° = 139	10.5° = 136	10.5° = 135	11° = 138	91° = 198
61	59	57	62	60

Mean for 80° = 59.8

Mean for 100° = 74.7 — 20° correction = 54.7.

Gerhardt's Equivalents and Notation.

ABOUT twelve years ago a distinguished French chemist, Charles Gerhardt,¹ proposed a system of chemical notation differing from that which is in general use. This system, which received at first but little favour, has gradually gained the adhesion of many distinguished scientific men, and is now a subject of general discussion among chemists. What the result of the discussion will be is scarcely to be doubted; but it would have been premature to adopt this system in an elementary exposition of chemistry like the one given in this Manual. Nevertheless, the new system has already acquired such importance that a short outline of Gerhardt's notation, and of some of the most important results which flow from it, will become necessary in this Appendix, the perusal of which is recommended to the reader after he has studied the text of the Manual.

In the new system, as in the old one, the combining numbers are referred to that of hydrogen as unity. The new numbers coincide with those adopted in this Manual, except in the cases of oxygen, sulphur, selenium, tellurium, and carbon. The equivalents of these few elements are doubled, and the symbols representing these double quantities are barred thus,— $\bar{O}$, $\bar{S}$, &c.

The new numbers are chiefly founded on the consideration of the proportion in which the gaseous and gasifiable elements combine by volume.

¹ Extracted by permission from "Fownes' Manual of Chemistry," Eighth Edition.

² "Introduction à l'étude de la Chimie par le Système Unitaire." Par M. Charles Gerhardt. 1848.

Name.	Old Notation.		New Notation.	
	Combining Number.	Symbol.	Combining Number.	Symbol.
Hydrogen . .	1	H	1	H
Oxygen . .	8	O	16	$\bar{O}$
Sulphur . .	16	S	32	$\bar{S}$
Selenium . .	39.5	Se	79	$\bar{Se}$
Tellurium . .	64.2	Te	128.4	$\bar{Te}$
Carbon . .	6	C	12	$\bar{C}$

Gerhardt assumes that equal volumes of the elementary gases and vapours, when compared, under similar conditions of temperature and pressure, contain the same number of atoms. If this hypothesis, which cannot be proved directly, be true, it is obvious that the relative weights of the atoms must be represented by the specific gravities of the elementary gases and vapours. In the following Table the specific gravities of elementary gases and vapours which have been experimentally determined are compared with the atomic weights according to the old and new systems, and, in order to facilitate this comparison, the specific gravities are referred to hydrogen as unity, instead of, as is usually done, to atmospheric air.

Name of Element.	Old Equivalent.	New Equivalent.	Specific gravity referred to Hydrogen as unity.
Hydrogen . .	1	1	1
Oxygen . .	8	16	15.936
Sulphur . .	16	32	31.705
Chlorine . .	35.5	35.5	34.940
Bromine . .	80	80	79.806
Iodine . .	127	127	125.607
Nitrogen . .	14	14	13.969
Phosphorus . .	31	31	63.711
Arsenic . .	75	75	152.792
Mercury . .	100	100	100.555

This Table shows that the specific gravities of the elementary gases and vapours referred to hydrogen as unity represent the atomic weight of these elements. Exceptions, however, are observed in the cases of phosphorus and arsenic, the specific gravities of which are double the atomic weights.

The atomic weights of the gaseous or gasifiable elements, when expressed by the new numbers, yield, with the sole exception of phosphorus and arsenic,² equal volumes of gas or vapour; and if we take the volume occupied by the atomic weight of hydrogen as unity, we may say that the atomic weights of the elements form 1 vol. of gas or vapour. It is supposed that the atomic weights of the other elements, could these elements be conveniently volatilised, would likewise form 1 vol. of vapour.

² There is still some hope that a more elaborate examination of the specific gravities of phosphorus and arsenic may remove this anomaly. This hope is based upon the following considerations:—Earlier experiments had given the specific gravity of sulphur vapour = 95.3, the specific gravity of hydrogen = 1; when the equivalent volume of this element became $\frac{32}{2} = 16$ (or about $\frac{1}{2}$) or $\frac{32}{2.3} = 13.9$ (about 1), according as the old or new equivalent of sulphur be adopted. More recently M. Deville has determined the density of the vapour of sulphur at higher temperatures (at the boiling-points of cadmium and zinc), when a specific gravity was found yielding the equivalent volume $\frac{32}{2.3} = 13.9$, or very little more than 1. Deville has also recently again determined the specific gravities of phosphorus and arsenic, and his observations confirm the numbers given in the text. Nevertheless, on further experiment at still higher temperatures, the anomalies at present observed in the case of phosphorus and arsenic may likewise disappear.

Compounds expressed by the new numbers exhibit a similar constancy of relation between atomic weight and specific gravity, and a similar uniformity in their vapour-volumes.

The specific gravity of a compound in the state of gas or vapour = $\frac{1}{2}$ the atomic weight, or the atomic weights of compounds occupy 2 vols. of vapour.

A few examples will illustrate this point:—

1 atom of hydrogen ($H = 1$ vol.) unites with 1 atom of chlorine ($Cl = 35.5$ vol.), 1 atom of bromine ($Br = 1$ vol.), and one atom of iodine ($I = 1$ vol.).

Combination takes place without any condensation, and the resulting compounds occupy a volume equal to the sum of the volumes of the constituents. It is, therefore, obvious that the atomic weight of hydrochloric acid, HCl , hydrobromic acid, HBr , and hydriodic acid, HI , must each form 2 vols. of vapour, and that their specific gravities must be equal to half their atomic weight.

$$\begin{array}{rcl} H & = & 1 = 1 \text{ vol.} \\ Cl & = & 35.5 = 1 \text{ vol.} \\ \hline H \text{ Cl} & = & 36.5 = 2 \text{ vol.} \end{array}$$

In the compound of hydrogen with oxygen and sulphur, 2 atoms of hydrogen ($H_2 = 2$ vol.) unite respectively with 1 atom of oxygen ($O = 1$ vol.) and 1 atom of sulphur ($S = 1$ vol.). In water and in sulphuretted hydrogen the sum of the volumes of the constituents is 3, but the combination takes place with condensation, the bulk of the constituents (2 vol. of hydrogen and 1 vol. of oxygen = 3 vol.) being reduced to $\frac{3}{2}$ (2 vol.) of water-vapour, or of sulphuretted hydrogen. Here again the atom of water H_2O or of sulphuretted hydrogen, H_2S , yields 2 vols. of gas. The specific gravities of these compounds in the state of vapour are equal to half their atomic weights.

$$\begin{array}{rcl} H_2 & = & 2 = 2 \text{ vol.} \\ O & = & 16 = 1 \text{ vol.} \\ \hline H_2 O & = & 18 = 2 \text{ vol.} \end{array}$$

In the compound of hydrogen with nitrogen, in ammonia, lastly, 3 atoms of hydrogen ($H_2 = 3$ vol.) are united with 1 atom of nitrogen ($N = 1$ vol.). In this case the sum of the volumes of the constituents is 4, but the combination likewise takes place with condensation, the bulk of the constituents (3 vol. of hydrogen and 1 vol. of nitrogen = 4 vol.) being reduced to $\frac{4}{2}$ (2 vol. of ammonia.)

$$\begin{array}{rcl} H_2 & = & 3 = 3 \text{ vol.} \\ N & = & 14 = 1 \text{ vol.} \\ \hline H_2 N & = & 17 = 2 \text{ vol.} \end{array}$$

Thus, we find that, represented in the new system, the atoms of three classes of compounds, as differently constituted as those represented by hydrochloric acid, water, and ammonia all yield 2 vols. of gas or vapour, the specific gravities of these compounds being, consequently, equal to one-half their atomic weights.

(To be continued.)

On Passive Steel, by M. SAINT-EDME.

CONTINUING my researches on the passivity of iron, I found that there exists a difference between the affinity of steel and that of iron for the passive state. I have the honour to present to the Academy the results which led me to this conclusion:—

1. If a steel rod is plunged into ordinary nitric acid (36° Baumé, density 1.34), a rapid and tumultuous boiling takes place round the metal, showing, in the first instance, a very marked action of the acid, but in a very short time—generally less than twenty seconds—the disengagement of gas suddenly ceases and the steel becomes passive. If a rod of iron is placed under the same conditions the action is continuous. This phenomenon takes place with all steels, whether English, German, cast, forged, &c., and the reaction is so decided that it may be considered as a distinctive and unequivocal characteristic of steel.

2. It is a well-known fact that iron wire rendered passive resumes its active condition if made to communicate with a piece of active wire, if both are plunged into the same nitric acid. Several years ago M. Schöenbein noticed that iron wire made passive by immersion in fuming nitric acid had the property of rendering an active piece passive by contact. He could not then account for this variation in the stability of the chemical indifference of iron. The following experiment clears up that which then seemed so puzzling:—Place a rod of steel in ordinary nitric acid, leaving a part above the liquid; the metal becomes passive, as before described; place beside it a rod of iron, which undergoes a continuous action; then, as soon as the two rods are put in communication, by joining the portions above the liquid, or by uniting them in the acid itself, the iron instantaneously becomes passive. Passive steel, then, like platinum, renders iron instantaneously passive.

3. Steel preserves its passiveness under conditions which would entirely destroy that of iron. Iron loses its passiveness when the temperature of the acid reaches 40° C., while a rod of passive steel may remain any length of time in boiling nitric acid without losing its passivity. This experiment has been several times repeated, and is found to be necessary to make the steel passive by contact with cold nitric acid, and then to plunge it in the boiling acid.

4. Iron cannot remain passive in nitric acid containing a certain proportion of hypo-nitric acid. If a galvanic couple is prepared, of which the positive electrode is a rod of passive iron plunged in nitric acid (density 1.34), in about half-an-hour after the circuit is completed the attack on the iron is determined. Under the same circumstances, steel remains passive a longer time, but once a certain point is rapidly reached, it again becomes active.

The passive state is usually explained by the formation of a layer of non-salifiable oxide, which forms electro-chemically on the surface of the metal. It must, then, be admitted that iron, in the state of steel, acquires a greater affinity for nascent oxygen, and that the stratum of non-salifiable oxide which is instantaneously produced on the surface is more adherent, more impermeable to acids, than simple iron. It appears, then, necessary to make a distinction between passive steel and passive iron.—*Comptes-Rendus.*

TECHNICAL CHEMISTRY.

New Theory of Cementation, by M. H. CARON.

THE question I am about to treat is so complex, that I must request the Academy to allow me to establish clearly the facts on which I found my views before I develop them freely.

In a paper which made some sensation, Mr. Sanderson,

an ingenious English manufacturer, concludes,¹ from his experience, that pure and isolated charcoal, oxide of carbon, ammonia, and bicarburetted hydrogen are unsuited for the manufacture of steel; but he shows that iron can be converted into steel by the simultaneous action of ammonia and olefiant gas. After showing that the well known cementation agents, cyanides and ferrocyanides act only by their metalloidal principle, he says:—

1. The transformation of iron into steel only takes place under the combined influence of carbon and nitrogen.
2. If, in analyses of steel, no mention of nitrogen has hitherto been made, the cause of the omission is that the analyses were either ill-done, or that they were made under the influence of a preconceived idea.

May I be permitted to say that Mr. Saunderson has not done justice to his predecessors, Berzelius, Shaffhaütl, Marchand, &c. As a proof, I transcribe an exceedingly judicious note by M. Nickles, Mr. Saunderson's translator:—

"This assertion is not quite correct. There exists a series of analyses of forge iron, cast iron, and steel, attesting the presence of nitrogen in these metals. It is true they do not all contain it, and the greatest quantity that has been found in them is 0.0002 (*Annuaire de Chimie*, 1851, p. 107). These analyses are so much the more reliable since their author, the late M. Marchand, of Halle, experimented from a point of view entirely foreign to the theory of cementation. After it had been established that the titanium of blast furnaces is not a simple body, but a mixture of cyanide and nitride of titanium, M. Marchand surmised the case might be the same with cast iron and steel. Nothing would have pleased him better than to find nitrogen in these carburets, and no one can say that the negative results were obtained under the influence of a preconceived idea. If, then, steel can be considered as free from nitrogen, it is nothing to the purpose that this gaseous metalloidal plays no part in the transformation."

Guided by these trials, which had revealed to me the real part nitrogen plays in cementation, I established, in October, 1860, that one of the most powerful and quickest steeling agents is cyanide of ammonium, a gaseous matter, which, by penetrating to the centre of the iron bars, speedily transforms them into steel of great perfection. And it is remarkable that in every instance of Mr. Saunderson's cementing with hydrocarburetted and ammoniacal gases, he unwittingly produced cyanide of ammonium. The same observation applies to M. Fremy's experiment, who also brought into contact ammonia and carburetted hydrogen, successively, it is true, but under such circumstances that at the moment of reaction the two elements were again found together, and, combining, formed cyanide of ammonium.

It remains for me to prove definitely that whenever iron is converted it is always put into contact with gaseous cyanide of ammonium or with volatile cyanides. This is not a difficult task, since Mr. Saunderson has shown that pure charcoal will not convert, and, according to my own experiments, it is the presence of nitrogen concurrently with the alkali of ashes, and, consequently, the formation of cyanide of potassium, which determines the acieration in the cementation cases.

What, then, is the part these cyanides play? If pure or almost pure charcoal is given to iron,—for example, that of the carburetted hydrogen,—as Mr. Saunderson, M. Fremy, and myself have done, and the operation is conducted at the high temperature generally employed,

the metal becomes too quickly saturated with the carbon, and cast iron only is the result. But if the metal be placed in contact with a carburetted matter, the elements of which are combined in an affinity so close that the iron can only overcome it by prolonged contact, the acieration produced on the surface of the bars will not exceed the desirable limit before the centre of the iron is cemented. On reflecting over this matter, it will be seen that the only undecomposable and volatile combinations of carbon are the alkaline cyanides. Then, cyanides only are the cementing agents, at least, with the temperatures used in manufacture. This restriction is important, as will presently be established.

But a too prolonged contact or too high a temperature will soon modify the effects produced. Thus, cyanide of ammonium, instead of cementing, can transform iron into cast iron,—a fact I have several times verified. It is not so easy to produce this result with cyanide of potassium, because it is less volatile and more difficult to decompose, from which it may be concluded that the most effective agent in acieration should be the least volatile cyanide,—that is to say, cyanide of barium,² a compound which I have already pointed out in another paper. I shall explain more precisely in regard to this substance in another communication on a new cementation process which has been for some months put in practice in a large manufactory in the environs of Paris, by order and at the expense of the Emperor.

All this will become still more clear if it is made apparent that other substances besides cyanides, containing carbon without nitrogen, can convert iron into steel, provided the temperature is not high enough to decompose them, and that their action is not too prolonged. Pure, light carburetted hydrogen gas passed at red-white heat on iron produces not so speedily, but quite as perfect a cementation as cyanides. It is the same with lighting gas,³ which contains a considerable proportion of light carburetted hydrogen gas; and if M. Fremy has been unable to convert iron into steel by means of this agent, it is because he operated at too high a temperature, and protracted too long the contact by the re-agents. Besides, in Berzelius' first edition (vol. iii. p. 279, 1831), there will be found some details on the fabrication of steel by lighting gas, as shown in England by Macintosh. Nevertheless, I agree perfectly with Mr. Saunderson respecting olefiant gas. I have not succeeded in cementing with this gas, though I have operated at the lowest possible temperature, it being too readily decomposed by heat. The tube in which the operation was conducted was found filled with charcoal, and the iron, in spite of tempering, remained soft and malleable.

Strictly speaking, cyanogen is also capable of cementing, but not so well as light carburetted hydrogen gas. These experiments show that to convert iron into steel it is requisite that the cementation agent should be able to carry the charcoal in combination into the pores of the iron when this metal appropriates it in the nascent state. Under no other conditions can cementation be produced.⁴

² Cyanide of barium is easily produced by simply mixing charcoal dust and natural carbonate of baryta. The nitrogen is furnished either by the charcoal itself or by the air which penetrates the sides of the cementation cases.

³ Purified by phosphoric acid and solid potassa.

⁴ On this subject I will remark, that it is impossible to suppose that nitride of iron can be formed at any moment during cementation in manufacturing operations. M. Despretz's nitride of iron has never been produced by any other means than by ammonia, which does not exist in cementation cases; and, even if it did, it would be decomposed by the temperature at which the operation is performed. As to

¹ *Journal de Pharmacie et de Chimie*, 1859, vol. xxxvi. p. 301.

After what I have said, it is useless for me to refer to the existence of nitrogen in steels. My theory is perfectly independent. The quotation from M. Nickles' translation, and the cementations so many, including myself, have effected without the presence of nitrogen, ought, I think, to decide this point. Let it be remembered also that Marchand, in his careful and delicate analyses, could find only such quantities of nitrogen as were either inappreciable or insignificant; that M. Shaffhäutl, the great partisan of the presence of nitrogen in steels, has been obliged to recognise the exactness of Marchand's observations, the same conclusion will naturally be formed as that of this celebrated German chemist,—"If there is any nitrogen it must necessarily belong to the substances mingled with the iron,—substances which no more form an integral part of the metal than does the scoria found with it.—*Comptes-Rendus*.

On the Chemical Composition of Cast Iron and Steel,
by M. FREMY.

(Continued from page 36a.)

I now arrive at one of the most important points in the fabrication of steel. Does there exist, as many metallurgists now admit, native ores containing a body unknown to chemists which would impart their valuable qualities to steel? Shall France be condemned to depend on other countries for the best cemented and cast steels? Can we produce cast irons which, when puddled, will yield steels comparable to those of Germany? Numerous analyses and synthetic operations performed for several years in my laboratory provide me with answers to these different questions.

I know that iron has for certain metalloids preferences excluding other combinations. From synthetic experiments I learn that it is useless to try to give iron an excess of carbon, and to change it into grey cast iron while phosphorus or sulphur remains in combination with the metal. Graphite, the presence of which characterises soft cast iron, appears only after the phosphorus and sulphur have been expelled from the cast iron by means of appropriate purifying agents. And, reciprocally, when grey cast iron is heated in a brasque yielding sulphur or phosphorus it becomes white, and then loses its excess of carbon, which crystallises in large flakes on the surface of the metallic bath.

Silicium only, which, as we know, is allied to carbon by its chemical properties, can simultaneously exist with it in grey cast irons. What I have said of the exclusion of carbon by phosphorus and sulphur applies also to nitrogen. I have found it impossible to make nitrogen react on iron previously impregnated with sulphur or phosphorus. These bodies cannot then assist in cementation; and it is thus intelligible why steel cannot be formed in a blast furnace. The experiments adduced appear to me to imply exactly the conditions of acieration. The best cemented and puddled steels are obtainable only with products almost absolutely pure; nitrogen can only exert its steeling influence on a metal entirely free from silicium, and above all from phosphorus or sulphur.

nitrogen, it is well known that it does not combine directly with iron. The existence of a nitride of iron before the formation of steel is, then, inadmissible; but atmospheric nitrogen, in contact with charcoal and potash of the ashes, yields cyanide of potassium, which is the reason why the presence of this agent in the atmosphere of the cases is absolutely necessary.

* *Journal für praktische Chemie*, v. Erdmann und Marchand.

Many French manufacturers already produce excellent steel, but those who do not yet excel I would urge not to think that certain countries possess exclusively the privilege of making steel of exceptional quality, this perfection being due to the employment of very pure materials. In France we have minerals perfectly suited to the fabrication of steel; but its irons need purifying, and the fabrication of cast iron needs revision. A mixture of iron and cast iron should not be considered as steel. It should not be attempted to convert impure irons into steel, nor should cast irons charged with foreign bodies be refined, for then there is no certainty regarding the stability of the acieration. Such steels easily decompose under the influence of combustible gases, losing their nitrogen and carbon, and returning to the state of iron.

After showing that purity of the metal is an essential condition of the formation and stability of steel, I think it necessary to add that, as has been long since proved practically, the addition of certain metals can improve the quality of certain steels.

Manganese, nickel, titanium, tungsten, &c., are often employed advantageously. These metals, by combining with iron, usefully modify the properties of the steel, yielding true alloys. But I shall presently demonstrate that the metals which appear principally to facilitate acieration are those which, like titanium and tungsten, form with nitrogen fixed compounds.

These special methods of producing steel are perfectly explained, according to my views just set forth; they are included in the general principle which I propounded when commencing my papers on steel. I said, in effect, that steels formed a true family of bodies in which iron is found combined with nitrogenised compounds, capable of containing carbon or other simple bodies.

Theory can now explain the constitution of a large class of steels resulting from these combinations, but practice only can realise their importance.

The effect of manganese, whose influence on acieration is known to all manufacturers, is probably due to its rapid oxidation, thus producing a metallic oxide capable of completing the fining of the iron, and this, by eliminating any foreign body, facilitates the acierating process.

The facts contained in this exposition lead to the following conclusions:—

1. Down to the present time I had established the constitution of steel in proving that this body always contains nitrogen, to be found in the insoluble matter in acids or disengaged by hydrogen. I had proved that steel is easily formed under nitrogenising influence, and that acieration depends on the proportion of nitrogen given to the metal; but I had not yet proved that steel can be restored to the state of iron if its nitrogen is taken away by hydrogen. It is this analytical demonstration that I have placed as the title of this Memoir.

2. In answer to the supporters of the old theory of acieration, who cannot understand how iron can nitrogenise itself in a case apparently containing nothing but carbon, I have proved that in cementation-cases iron can obtain nitrogen not only from the gaseous products circulating in the apparatus, but also from the charcoal, which is always nitrogenised.

3. The undeniable influence of all nitrogenised organic compounds on acieration tends to confirm my theory. I have obtained very active nitrogenised charcoals by previously nitrogenising a ternary organic body, such as sugar.

4. In cementation, nitrogen acts not only chemically,

by combining with carbon to form a sort of cyanuretted compound, which appears to be the essential element of steel, but it exercises a mechanical action in becoming a carburising agent; an excess of carburised gas reduces nitride of iron, and a real phenomenon of substitution is thus produced; nitrogen then plays the part of the shuttle, first combining with the iron, and then making way for carbon, whilst rendering the metal porous. The way in which carbon penetrates iron and the formation of the blisters characterising blistered steel may thus be explained.

5. In the formation of steel by fining cast iron, nitrogenisation is effected at the moment the iron becomes red hot. Nitrogen may be supplied by the cast iron, the combustible gases, and even by atmospheric air.

6. I have proved that the quality of steels does not depend upon the chemical nature of minerals belonging to some privileged locality; it depends entirely on the purity of the iron and cast iron employed. French manufacturers, by purifying their products, may, then, obtain the best steels. My experiments show that, with respect to their combination with iron, certain metalloids have in some sort the rights of precedence. I am convinced that it is impossible to nitrogenise iron when the metal is silicious, phosphoric, or sulphurous. Steel manufacturers will vainly endeavour to produce steel with impure iron or white sulphurous cast iron.

7. The metals appearing to exercise a useful influence on acieration, such as tungsten, are precisely those which form nitrogenised metallic compounds. The different bodies composing the steel family have for their base a nitride of carbon or metallic nitrides.

Thus, laboratory researches and practical observations prove the utility of nitrogen in acieration, and show that steel is not merely a carburet of iron.

The Academy will not misunderstand the direction of my researches; they are, as may be seen, exclusively scientific. It must not be thought that my papers on steel deprive manufacturers who employ cyanides, lighting gas, animal excreta, &c., of the merit of their discoveries. On the contrary, I shall always be happy to acknowledge acierations produced previously to me by nitrogenised bodies. I know too well the ability of our manufacturers not to be persuaded that they will make excellent steels when science demonstrates the constitution of the body they wish to produce. All my efforts have this important object.—*Comptes-Rendus*.

On the Means of Estimating the Value of Gilded and Silvered Articles.

THE following methods have been employed for some time past in the German Revenue Offices for testing the value of gilding and silvering, and may be of interest to our readers from the circumstance that it is often of importance to have a test by which real silver plating on articles manufactured from similarly-coloured metals,—*e. g.*, German silver and Britannia metal, may be distinguished from those, which, to give them a better appearance, are only slightly silvered. The thin coating of silver on this latter class of goods is, of course, soon rubbed off, and then the real colour of the metal becomes apparent.

The value of a test of this kind consists in its easy execution, and the certainty of its indications.

Testing of Gold.—The ordinary method of testing gold by the touchstone is founded upon the insolubility of this metal in nitric acid. If a mark be made on the touchstone with the article under examination, the gold

is not dissolved by this acid, whereas golden-coloured alloys of inferior value are dissolved and disappear immediately.

When articles are very thinly gilded, the detection of the gold in this manner is uncertain, in which case the following method may be used with advantage. This process depends upon the fact that an aqueous solution of chloride of copper is without action on gold, whereas on golden-coloured alloys, as brass, pinchbeck, &c., it produces a black spot. The testing fluid prepared by Loeve's prescription, by dissolving copper in fuming nitric acid and adding a solution of common salt, does not give very certain results, for if the article be but slightly gilded a black spot is nevertheless produced. The concentration of the testing fluid, for instance, exerts a very important influence; for a strong solution of chloride of copper will blacken, although a more dilute form of the same is a reliable test. In order to exclude all traces of aqua regia formed by the action of the nitric acid on the common salt, and which easily dissolves traces of gold, the testing fluid is recommended to be made in the following manner:—

A little carbonate of copper is put into a test-tube, and to this is added, drop by drop, pure hydrochloric acid till the blue powder has dissolved to a clear green fluid, occasionally warming it over a spirit-lamp. This concentrated solution of chloride of copper is diluted for use with from ten to eleven times its volume of distilled water. Before testing, the metallic surface must be well cleaned. This can be done effectually by brushing it for a minute or two with a little spirits of wine, or better, with absolute alcohol.

The surface having dried, a little of the testing fluid is dropped on and allowed to remain in contact for about a minute. The fluid is then removed by means of a small pipette, and the surface of the metal completely dried with bibulous paper; if no dark spot be then visible, the article is coated with pure gold. If the metallic surface is but lightly gilded, a very slight blackening is sometimes remarked, which may throw a doubt upon the result. In such a case, to make quite certain, a little of the surface may be scraped off, and then the testing fluid again applied. If a dark spot is then perceived, the article may be considered as very thinly gilded.

If a further and more direct proof of the presence of gold is required, the article to be examined, or a piece of it, may be put into a porcelain cup, and as much pure nitric acid poured over as will half cover it. The thin layer of gold covering the surface does not prevent the metal from being attacked by the acid, and the gold becoming separated floats in thin films on the top of the liquid. The green metallic solution is now removed, and more nitric acid poured over the gold spangles: it is then somewhat warmed, and water finally added. The gold has now been fully tested by its insolubility in nitric acid, and it only remains to ascertain that it dissolves to a yellow solution in warm aqua regia.

Thin gilding of this description is often met with in the French mock jewellery; the coating is sometimes so thin that it not only deceives the eye, but is difficult to test by the ordinary methods. Instead of putting the entire article into the acid, and thus risking its demolition, a portion of the surface may be scraped off with a knife, and tested with the nitric acid.

When an article appears to be made of massive gold, the testing by means of the touchstone should be first resorted to.

Testing of Silver.—The ordinary and very accurate

method of testing silver is founded upon the insolubility of chloride of silver in dilute acids and in water. This otherwise satisfactory test is, however, difficult to carry out when an article is very thinly plated with silver; but in all these cases a simple and very accurate test can be used, which is based upon the reaction of chromic acid upon metallic silver. For this purpose a testing fluid is prepared by adding pure nitric acid to powdered red chromate of potash, and mixing them in such a manner that a part of the latter remains in suspension, the whole being kept well stirred during the mixing. Equal parts by weight of each may be taken. The nitric must be quite free from hydrochloric acid, and have the proper degree of concentration, being neither too fuming nor too dilute; it should have a specific gravity between 1.20 and 1.25. When the mixture has been prepared for a few hours, and been stirred several times, the reddish-coloured liquid is poured off from the residue and kept in a stoppered bottle. A drop of this liquid is then brought in contact with the metal to be tested, and immediately washed off again with water. If a visible blood-red spot remains, silver is present.

This method requires only the following precautions.—Firstly, the metallic surface must have been quite cleaned from grease or varnish with spirits of wine; Secondly, water must be poured over the treated surface before judging of the colour, as that of the testing fluid is altered by the metal, and the red precipitate is not distinctly visible until the coloured liquid has been washed off. The red spot can afterwards be very easily removed with the finger.

By this method the slightest trace of silver in an alloy may be ascertained. When an article is suspected to be only thinly plated, a very minute drop of the testing fluid should be used. With no other metal or alloy does this red spot, so characteristic of silver, appear; in some cases the testing fluid only corrodes the surface of the metal, whilst in others coloured precipitates are formed, which, however, cannot be confounded with those of silver.

German silver brought into contact with the testing fluid affords no red spot after being washed. The spot will, however, have been strongly corroded.

Britannia metal yields a black spot; zinc is strongly corroded; platinum is not attacked; lead gives a yellow precipitate; tin is strongly affected by the fluid; when the brownish-coloured testing fluid is washed off, a yellow precipitate is perceived, which adheres tightly to the metal; copper is strongly attacked, a tarnished surface of this metal is brightened by the action of the acid. Bismuth yields a yellow precipitate; antimony does not; by this means, therefore, these two metals, somewhat similar in many respects, can easily be distinguished. Mercury, or an amalgamated surface, yields a reddish brown precipitate, which, however, is entirely washed away by the water, and is not likely to be confounded with the silver reaction.—*Deutscher Ingenieur.*

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE VIII.

Charcoal—Compounds of Carbon with Oxygen—Carbonic Acid.

We have seen that the element Carbon is capable of existing in three distinct conditions; two of them crystalline,

namely, the diamond and graphite, and one of them amorphous, or devoid of crystalline structure, and to which we generally give the name of Charcoal. This latter body, although less interesting in many respects, is to the chemist of more importance than the other two forms of carbon; and to this form we shall have to confine our attention on the present occasion.

We know of several varieties of charcoal obtained from different sources. Animal or vegetable substances when heated to high temperatures, to full redness for instance, in closed vessels such as iron or earthenware retorts, have their volatile constituents driven out of them, and a black mass remains behind, which is this element carbon, associated with more or less of the inorganic impurities which have been present in the organic substance. If we take care to employ an organic substance which is perfectly free from these inorganic impurities, we obtain a perfectly pure specimen of carbon by this process. The carbon prepared in this way, leaving out the very impure form of it which we know as coke, may be divided, perhaps conveniently, into two classes of carbonaceous matters, namely, wood-charcoal and animal-charcoal, which are used for two distinct purposes, and which have, in fact, very different properties. Wood-charcoal is obtained, as its name imports, by the carbonising of wood; wood is heated to redness in closed iron cylinders, or it is sometimes piled in great heaps. The heaps are covered over with turf so as to prevent the access of air to them, and a very slow combustion is carried on until all the volatile constituents are driven off, and a mass remains behind which you all know as wood-charcoal, such as we have here. One of the most prominent properties of this wood-charcoal is its power of absorbing gases. Introduced into different gases, it condenses within its pores a very large amount of some of them. Here we have a table showing the extent of its absorbing power:—

Volume of Different Gases absorbed by one Volume of Charcoal.

Ammonia	90	Bicarburetted hydrogen	35
Hydrochloric acid . . .	85	Carbonic oxide	9.4
Sulphurous acid	65	Oxygen	9.2
Sulphuretted hydrogen	55	Nitrogen	7.5
Nitrous oxide	40	Carburetted hydrogen	5.0
Carbonic acid	35	Hydrogen	1.7

Suppose we were to take a cubic inch of charcoal, then that cubic inch of charcoal would absorb 90 inches of ammoniacal gas. This property of absorbing gases was thought at one time to be the cause of the power that this form of charcoal possesses of depriving putrefying and decaying organic substances of their odour; but Dr. Stenhouse, who has recently investigated this subject, has proved that the power of destroying these noxious odours does not depend, at least wholly, on this property of the charcoal. To this peculiar effect of charcoal I must now direct your special attention.

Charcoal, as we have seen from this table, possesses the property of condensing in its pores a certain amount of oxygen, namely, 9.2 times its volume; and the oxygen which it thus condenses, possesses greater powers of combining chemically at common temperatures, than oxygen under ordinary circumstances, when it is free in the atmosphere. The consequence of this is that when wood charcoal, which has thus been impregnated with oxygen, is brought into contact with combustible substances, especially carbonaceous substances, or compounds of carbon and hydrogen, it actually possesses the power of burning up those substances at ordinary temperature, and effecting their combustion by resolving them into their ultimate products—into the same products, in fact, that we should get by passing these materials through a common fire. This property of thus burning up organic matters passing through the pores of the charcoal, which pores have been previously saturated by oxygen, is of very great im-

portance in the processes of deodorisation and disinfection of atmospheric air. It is only in the dry state that wood charcoal can be successfully employed in this way; when once wetted this power is very materially diminished. We have here a vessel containing in the lower part of it some animal matter—some flesh which has been kept there now for some months, and which is in the most advanced stage of decomposition. It is emitting gases at the present moment of the most intolerable odour, so much so that if they were not completely burnt up before they came out into the room the whole theatre would be rendered quite unbearable from the stench that would be emitted from this decomposing animal matter; and yet if you test the slight odour from the surface of this jar, you will perceive nothing but the most pure ammoniacal odour. Now, what is the process that is going on here? We have a quantity of decomposing animal matter giving off gaseous emanations, which are well known to be very injurious and deleterious to health. These materials, however, have to permeate a layer of some two inches of this roughly crushed charcoal before they can get into the atmosphere. These emanations consist of compounds of carbon, hydrogen, and nitrogen chiefly, though there is also some phosphorus and sulphur present in them; but carbon, hydrogen, and nitrogen, and possibly also oxygen, are the chief ingredients of the emanations. The passage of the gas through the oxygenised pores of this charcoal causes the conversion of the carbon of the noxious gases into carbonic acid, and the hydrogen into water, whilst the nitrogen is transformed, at the same time, chiefly into ammonia, and partly, perhaps, into nitric acid; so that we get the final products of decay, which I have already mentioned as not being noxious. It is only in the intermediate stages that these compounds are noxious. The original flesh before its putrefaction was of course innocuous; and when the elements have finished their oxidation or change and have become converted into their final products—carbonic acid, water, and ammonia—these are also harmless. Now, charcoal finishes, in the course of a few minutes, the process which would take place in the atmosphere in the course of days; so that we get the same effect as if we were passing the noxious substances through a furnace burning brightly, where they would come into contact at a high temperature with atmospheric oxygen. I may possibly be able to show you the ammonia which is passing off from this jar. We can render it visible by a test that I have already applied on a former occasion for rendering this body visible—that of placing above the jar from which it is issuing some dilute hydrochloric acid. You see we have now very copious white vapours produced; I am not sure whether they will be very visible at a distance; perhaps by holding a piece of black paper behind them you may be able to see them. There they are, curling away from the mouth of the jar. White vapours of chloride of ammonium are there being evolved, and they are produced by the ammonia issuing from the jar coming into contact with hydrochloric acid.

Now, this property of charcoal which was first explained by Dr. Stenhouse, has been rendered available, in the first place, for the construction of respirators—charcoal respirators,—some specimens of which I have upon the table. Here is an orinaal respirator which is intended to protect the wearer from the effects of noxious emanations and effluvia. It is intended to be worn in hospitals by nurses who are exposed to contagious and infectious diseases, and under other circumstances of that kind. There are various forms here of these respirators. Charcoal has also been applied to the filtration of atmospheric air in densely-populated districts, where the air is rendered unwholesome by the presence of such noxious emanations. All the air entering a dwelling may be made, by a very simple contrivance, to pass through a charcoal filter, which consists of two surfaces

of perforated zinc, between which there is a layer of coarsely powdered charcoal. The air filters itself through this charcoal in passing inwards towards the dwelling, and in thus being filtered all the organic matter which it contains is burned up by the process we have going on here [referring to the vessel containing the putrefying flesh]. These filters have been used at the Mansion House and Guildhall for several years—four or five years at least. The charcoal never requires changing: it continually renews its supply of oxygen, and continually carries on this process of combustion. You may regard it as a fire which is constantly burning, but does not consume any fuel, or, at all events, all the fuel it consumes is this noxious vapour in the atmosphere; the charcoal itself lasts apparently for ever.

The other form of charcoal—animal charcoal—is used to operate in a similar way upon liquids. It is used for decolorising purposes. It is prepared by carbonising the bones of animals in close vessels—iron retorts, for instance; and we get then a black mass, which, on being reduced to powder, presents the appearance of this specimen before me. Now, here is a decoction of logwood which is very deeply coloured. We will take a portion of it and mix it with some of this animal charcoal. I dare say we shall have sufficient there; and I think if our animal charcoal be good—I have not tried this particular specimen, and the quality varies considerably according to the mode in which it has been manufactured and the material from which it has been prepared; but if the charcoal be good, we shall have, upon filtering this liquid through the filter-paper upon which this charcoal is placed, a perfectly colourless solution passing through. [A portion of the decoction was filtered accordingly.] You see the first few drops of the liquid passing through are a little turbid, by some of the finely powdered charcoal passing through with it; we will first put that back again into the filter. Now you see the liquid is filtering through as clear and colourless as distilled water. On account of this property of decolorising vegetable solutions, animal charcoal is employed for the preparation of organic alkaloids. It is also very extensively employed in the refining of sugar. The syrup being considerably coloured, to begin with, is passed through a thick mass of this animal charcoal; it then comes out almost perfectly colourless, when it only requires evaporation to be converted into crystalline lump sugar of perfect whiteness.

This effect of charcoal in absorbing oxygen is possessed also by other substances, and especially by spongy platinum, which possesses the power to a still greater extent. Here we have a little jet of hydrogen, and here is a particle of this spongy platinum, which is obtained by igniting the ammonio-chloride of platinum so as to leave the metal in a very finely divided state. I will bring this spongy platinum over the jet; you see it begins to ignite; it has ignited our jet of hydrogen. Here you see the platinum is far more potent than the charcoal, and there is no doubt, if we could employ this metal in the same way as charcoal, we should get a much better effect; but this would be impossible on account of the price of the platinum, which is too expensive. There is a greater condensation of oxygen in the pores of the platinum, which causes so rapid an action upon this hydrogen that the temperature rises to the igniting point, and the consequence is the ignition of the jet.

Carbon forms two inorganic compounds with oxygen—namely, carbonic acid and carbonic oxide. The first of these is found in nature both free and combined. It is free in the atmosphere as one of the constituents of the air. Its functions there we shall have to examine, however, in the next lecture, but in this form carbonic acid constitutes only a very small proportion of the atmosphere. A very minute fraction, only about 0.04 per cent. of atmospheric air consists of carbonic acid, but it is calculated that in the total atmosphere surrounding our globe we have no

less than 2,800 billions of pounds of this carbonic acid present. Nevertheless, the great mass of carbonic acid in nature exists in combination, and in combination almost exclusively with lime and magnesia. Those immense lime stone and chalk deposits, and such like strata, which surround us here on every hand consist essentially of one of these bases—lime or magnesia united with carbonic acid, chalk being a carbonate of lime, and carbonate of magnesia being sometimes associated with it. Dolomite, the material with which the new Houses of Parliament are built, is such a solidified form of carbonic acid associated with lime and magnesia.

We can obtain carbonic acid from the air, if we please, by an indirect process similar to that which we employed for the extraction of oxygen from the air. We can cause it to combine with some substance for which it has an affinity, and then extract it from that substance. We take some lime that has been for some length of time exposed to the air—a specimen of old mortar, for instance, which is a material which has been extracting for years this carbonic acid from the air: and from this mortar we can at once obtain a supply of carbonic acid by pouring upon it some strong acid. When we pour hydrochloric acid upon this mortar you see a strong effervescence takes place, caused by the disengagement of the carbonic acid which the mortar has taken up from the air, and which we now extract from the mortar. Carbonic acid is also formed largely during the processes of combustion, respiration, putrefaction, decay, and fermentation. The products of respiration—the breath of animals—passing through any solution which is capable of absorbing carbonic acid reveals the presence of a considerable percentage of this gas. In addition to this source of the gas, we have also a continual production of it in the formation of coal. In the process by which woody fibre is transformed into coal, large quantities of carbonic acid gas are evolved. In volcanic districts, also—in the ancient volcanic districts of the Rhine, for instance—vast quantities of this gas are evolved, and the spring water coming into contact with the carbonic acid saturates itself with it, forming those delicious springs from which the Seltzer water is derived.

None of these processes, however, are readily available for the preparation of carbonic acid for experimental purposes. For such purposes we employ the natural compounds of carbonic acid to which I have just alluded—the carbonates of lime or magnesia, but especially the former. Carbonate of lime, in the form of white marble, constitutes the best material for the production of this gas. If we pour upon white marble some hydrochloric acid it disengages the carbonic acid which was previously in combination with the calcium or lime, and we obtain the latter in combination with chlorine as chloride of calcium; that is, we transform carbonate of lime into chloride of calcium and thus the carbonic acid is evolved. Here is a convenient form of apparatus for the preparation of the gas by this process; in fact it is the apparatus which we have previously described for the preparation of hydrogen. In the place of zinc in our central vessel we have now carbonate of lime or white marble; and the liquid we are employing is dilute hydrochloric acid which comes into contact with the marble in the central vessel. A violent effervescence is the result, the carbonic acid being expelled from the lime by the hydrochloric acid. I can at once get a current of this gas, and can collect it at the pneumatic trough by simply plunging the extremity of this tube beneath the jar. Here is our gas coming off in a continuous stream. As the liquid falls in the upper bulb it rises in the central globe, producing accordingly a continuous stream of gas for a very considerable length of time.

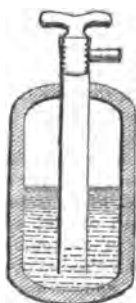
Carbonic acid possesses the following properties:—It is a colourless, transparent, and consequently invisible gas, and vessels filled with it are not distinguishable in

appearance from similar vessels filled with atmospheric air. It possesses a high specific gravity, being rather more than half as heavy again as atmospheric air, its specific gravity being 1.52; and on this account a balloon filled with atmospheric air readily floats in an atmosphere of carbonic acid. We can readily prove this by means of our apparatus. We have here a jar, at the bottom of which is a collodion balloon filled with atmospheric air, and I think you will find that the balloon, which is now resting quietly at the bottom of the jar, will be elevated as the carbonic acid is allowed to pass into the jar. Here is a similar arrangement in this vessel for generating carbonic acid. The gas passing up through this tube will be collected in the jar in which the collodion balloon is resting; and the gradual displacement of the air in the jar by the carbonic acid as it passes in will be rendered evident by the elevation of that collodion balloon. The height of the stratum of carbonic acid will be indicated by the position in which the balloon floats. [A stream of carbonic acid from the generating apparatus was passed into the jar.] You see the balloon is beginning already to manifest some levity, and I think we shall presently have it gradually ascending from the bottom of the jar, and if we continue the supply of carbonic acid we shall bring it up to the mouth of the jar. Carbonic acid possesses a pungent and peculiar odour, and a somewhat acid taste; but is only very slightly acid. It is a very feeble acid, and reddens litmus paper, and manifests an acid reaction only to a very slight extent. It is soluble in water, bulk for bulk, that is, one volume of water will dissolve one volume of carbonic acid at any pressure. If you double the pressure upon the carbonic acid, you reduce the gas to one-half its volume, and the water will then dissolve almost exactly double as much carbonic acid as it did at the lower pressure; thus showing that the water dissolves carbonic acid at all pressures in a volume equal to its own. Therefore, if you reduce your carbonic acid to one-sixth of its volume, you will get six times as much gas dissolved in the water as you would at ordinary atmospheric pressure. Upon this property depend the processes for making those effervescing beverages, such as soda-water, lemonade, and champagne. All these are fluids in which the carbonic acid has been dissolved at higher than ordinary pressures, and on removing the pressure the excess of the gas in the bottle makes its escape, and thus produces that effervescence which we all admire so much in those liquids.

You see our balloon is making some progress upwards; the carbonic acid is now rising in the jar, and as it gradually ascends, the balloon is carried up with it.

Carbonic acid is one of those gases which are capable of being transformed into liquids by exposure either to very intense cold or to great pressure. We were first indebted for this fact in relation to carbonic acid, and a number of other gases, to Professor Faraday, who made a number of very delicate and difficult determinations regarding the pressure required to liquefy many of these gases, and also the temperature required for the solidification of the liquids thus produced. If we take carbonic acid at ordinary temperatures—we will say at the temperature of this theatre—we require to apply to it a pressure of something like sixty atmospheres in order to reduce it to the liquid form, that is, a pressure of something like 900 pounds to the square inch; and, therefore, we require very strong vessels in order to perform this operation. It can, however, be accomplished, and we have here in this vessel a quantity of the liquefied gas under this enormous pressure of 900 pounds to the square inch. It is obtained by compressing the gas by means of a forcing-pump into this cylinder, or it may also be liquefied by disengaging the gas from marble, for instance, by means of a stronger acid in a close vessel, and connecting this vessel then with the cylindrical receiver, so as to distil over the carbonic acid which is produced by the chemical

reaction in the other vessel. The particular specimen of liquefied acid we have here has been obtained simply by pumping into this vessel a large quantity of carbonic acid. Liquefaction was thus effected, and a quantity of liquid acid obtained by this compression. The construction of the iron cylinder is represented in this diagram.



The liquid acid occupies, of course, the lower part of the cylinder, and there passes a tube, nearly to the bottom of the cylinder, and consequently nearly to the bottom of the liquid acid. This tube is closed above by means of a screw. The screw, by means of this lever, can be turned at pleasure, and you will, therefore, see from an inspection of this diagram, that when this screw is elevated a little, so as to allow communication between this exit tube, which is here, and the interior of our cylinder, it will then admit of the passage of the liquid acid upwards through this tube, and outwards into the atmosphere, or into any vessel in which we wish to collect it; so that we have the power of easily transferring the contents of this cylinder into any other vessel, or of letting it freely escape into the atmosphere. Now, if we were to allow it freely to escape into the air we should observe this—that the liquid acid the moment it came into contact with the air produced an enormous volume of carbonic acid gas, but at the same time a solid substance like snow would be produced, and would gradually fall down upon the floor. This snow-like substance is solidified carbonic acid, and it is rendered solid by the loss of heat which is abstracted from the liquid acid in the sudden conversion of this liquid into gas on reducing the pressure. From sixty atmospheres the pressure is thus reduced to one atmosphere; the carbonic acid very greatly expands, enters into violent ebullition, and absorbs a large quantity of heat which it renders latent. The effect of this is to reduce the temperature of the remaining liquid below the solidifying temperature, which is given in this diagram at -135 degrees (*minus* 135 degrees).

Liquefaction and Solidification of Gases.

Name of Gas.	Melting point.	Pressure in atmospheres of Liquefied Gas at 32°F .
Sulphurous acid.	-105°	1.53
Cyanogen.	-30°	2.37
Ammonia.	-103°	4.40
Chlorine.	-103°	8.95
Sulphuretted hydrogen.	-122°	10.00
Protoxide of nitrogen.	-150°	32.00
Hydrochloric acid.	-135°	26.20
Carbonic acid.	-135°	38.50

A temperature of -135 degrees is the solidifying temperature of carbonic acid as determined by Mr. Faraday. This sudden rushing of the gas from the liquid when the latter is relieved of pressure, reduces the temperature of the remainder from 60 degrees down to something like -135 . Now, instead of allowing the liquid to escape into the air, and thus losing the snow-like substance which is produced under the circumstances, we will allow it to escape into this brass box which can be fitted upon the nozzle of

the iron cylinder, and then the liquid will impinge upon a curved strip of brass and make its circuit round this box a number of times. These handles are both hollow, and there is provision made for the passage of the gas through both of them. The gas escaping through these minute apertures is sifted almost completely from the solid particles of carbonic acid, which are deposited within this box where the solid mass finds itself free and under atmospheric pressure. In this way I dare say we shall be able to collect nearly a box full of this solidified carbonic acid. [The small brass box was fixed on the nozzle of the apparatus, and the screw of the cylinder being loosened, the liquid passed into the brass box and afterwards escaped into the atmosphere, a hissing sound resembling the escape of steam through a valve accompanying the passage of the gas.] The liquid is now escaping into the box. There we have an enormous stream of this gas passing into the air, and a little of this snow-like matter escapes with it, and the small vessel becomes so cold that it is covered on the outside with hoar frost. We shall soon have a sufficient amount in the box to assure ourselves of its very low temperature. [After the operation had been continued for a short time the box was detached from the iron cylinder and opened.] Here we have the box very nicely filled with this beautiful solidified carbonic acid. We must not, however, expose it more to the air than is absolutely necessary; we will wrap it up in flannel, not to keep it warm, but to keep it cold. By means of this acid in this solid form we can readily obtain very powerful cooling effects. We can take some of it on the hand, as we shall presently see, without any inconvenience whatever, unless it be squeezed powerfully upon the skin when it produces a very severe burn, or at all events an effect quite similar to that caused by touching red-hot metal. If we dissolve the carbonic acid in a liquid in which it is soluble then that liquid can be applied readily for the production of a very low temperature. The best liquid for this purpose is ether, in which the solidified carbonic acid is very readily soluble. We will place this copper vessel here upon a stool, and beneath it we will place a stratum of water, and we shall see that a little of this acid dissolved in the ether will freeze the vessel to the stool. There you see we can solidify the water beneath this upper vessel which is frozen to the stool immediately. But in addition to this—in addition to the freezing of water—we may solidify other substances which are frozen at a much lower temperature, Mercury, for instance, can be solidified in this way. I will solidify a small quantity first, and perhaps Mr. Adams will, in the meantime, solidify a larger quantity while I am manipulating with this. I want to show you a considerable amount of mercury solidified in this way. [The solidification of a small quantity of mercury was then commenced by the lecturer.] Now our mercury is, I think, perfectly solidified. Yes, there it is a solid mass. The feel of it is exactly like that of red hot iron. Here we will solidify a much larger quantity of this metal, and I will try to show you that when it is thus solidified it possesses malleability; it is capable of being hammered out. We shall not succeed perhaps in getting it hammered into thin plates at this temperature, for it very soon rises in temperature and enters into fusion. This hammer makes the ethereal solution of carbonic acid boil, it is so hot in comparison with the cool contents of this vessel. I will therefore cool the hammer a little so as to have it in good condition for our experiment. We have at the present moment a solid piece of mercury, and we will now see whether we can extend it. [The solidified mercury was then hammered.] You see it is being extended, but it soon begins to fuse; however, before the fusion took place the size of the piece was considerably augmented. Now here [referring to the mercury frozen by Mr. Adams] we have a mass of some four or five pounds of mercury perfectly solid; and I will get Mr. Adams to

hold it in a jar of water elevated here, whilst I throw upon it a ray of electric light so that you may see a very curious effect which is sometimes observed—generally, I may say—when this solidified mercury is suspended in water. [The room was then darkened.] You see the solid mercury is first put into a net-work basket, and then gradually lowered into the water. The effect is, that the comparative warmth of the water will fuse the mercury, and the metal will soon begin to descend in little streams through the gauze; these streams of mercury freeze the water as they pass through it, and form a number of long icicles. There are the little icicles suspended beneath the basket in the water, and the brilliant streams of mercury descending from them. The mercury in melting has frozen a corresponding quantity of water to the solid state. There we have icicles in very considerable quantity, and the mercury is still running out of the mass. [The light was again admitted to the room.] Now we have seen this striking effect, the freezing of fluid mercury which requires a temperature something like *minus* 40° to freeze it; but, really, there is not so much in this experiment as would appear. We have to extract a far less amount of heat from the mercury in order to freeze it than we should have to abstract from the same weight of water to freeze that. It is true we require a lower temperature in one case than in the other; water freezes at 32°, and mercury at *minus* 40°; still I think we shall be able to freeze some of this mercury even in a red hot vessel by means of our powerful refrigerator. Here we have a platinum crucible fixed in this ring (of the retort stand), and we will render the crucible red hot by means of this spirit lamp. We will take a spoonful of mercury for this purpose. We will make our crucible red hot. Now its sides are just heated to redness. [A quantity of solidified carbonic acid was placed in the hot crucible.] I will lower this mercury into the crucible. You see this excessively cold body seems to be very well contented in the red hot vessel; it does not manifest any very extraordinary activity. The fact is, it is not in contact with the vessel at all; there is a stratum of carbonic acid gas surrounding it; and now I will pour a little ether upon it. I believe our mercury is already solidified, but we will make sure of it by giving it somewhat more ether. Now it has solidified. [The edge of a capsule was dipped into the hot crucible, and the frozen mercury was withdrawn and dropped upon a plate as a solid button to the capsule.] Yes, it is now solidified; you see it still adheres to the capsule, but in a moment it will get a little warm around the edges, and I dare say it will drop off. There it is, a solidified mass, which is evidently formed almost with as much ease in this red hot vessel as in an ordinary cold vessel.

Now, if we cast some of this solidified carbonic acid into a jar filled with water, we shall have its gradual disintegration and the formation of the same gaseous carbonic acid as we are disengaging there. We will put some of this solid into a little tin box perforated above, and introduce it beneath one of these jars on the pneumatic trough. There is our carbonic acid, you see, being disengaged from that white solid in a beautiful regular stream; and we will preserve this specimen, for we may possibly require to test it afterwards. The heat of the water is communicated to the solid mass of the acid, which is thereby gasified, and issues through the upper apertures of the box into the jar. We here have a jar rather more than half filled with oxygen, which I think will enable us to see the widely different properties of these two gases in regard to supporting combustion. I will allow some of this gaseous acid to pass up into that jar so as to fill it completely; and as carbonic acid is almost one half heavier than oxygen, I hope here to get a stratum of carbonic acid beneath, and the stratum of oxygen above. We shall have the vessel full in a moment, and then we will remove it from the trough, in order that you may see it better. Now we have what I expected—what will prove to be two strata of gas

in this vessel,—an upper one of oxygen, and a lower one of carbonic acid. The carbonic acid is not capable of supporting combustion under ordinary circumstances; while oxygen, as we are aware, supports it with very great brilliancy. Here we have a brilliant combustion of a taper when held in the upper part of the jar. The taper is extinguished when it descends to the lower part. It is relighted when again brought into the upper stratum of gas. The flame of the taper is extinguished by the carbonic acid in the lower stratum, but the wick remains at a sufficiently high temperature to enable the oxygen in the upper stratum to re-inflame it as soon as it comes in contact with it.

The remaining properties of carbonic acid must be very briefly described. As I have already stated, it is capable of uniting with alkalies, forming salts, which are called carbonates. It is, however, as we have just proved, incapable of supporting combustion, and it is also equally incapable of maintaining animal life. But when we speak of carbonic acid as being incapable of supporting combustion we must receive this announcement with some reservation—with the same kind of reservation with which we received the announcement that protoxide and binoxide of nitrogen were incapable of supporting combustion. We saw that those two gases were capable of supporting the combustion of bodies, the temperature of which was sufficiently high to decompose those gases and convert them into nitrogen and free oxygen. Now, a combustible having a temperature high enough to do this burns in those gases, for it first decomposes them, and then burns in the mixture of oxygen and nitrogen. A similar effect takes place with the carbonic acid. If we have any combustible which burns at a sufficiently high temperature or has sufficient affinity to decompose the carbonic acid, that combustible will then prolong its combustion in the oxygen which it thus supplies to itself. A substance of this kind we have in that powerfully positive metal, potassium, which is capable of burning even in carbonic acid. We will place our potassium in the bulb of this tube. You see we have a stream of the gas coming out there, sufficiently concentrated and pure to extinguish the flame of that spirit-lamp. Now, we require to heat the potassium first before it will ignite, but when we have once got it in a state of perfect fusion it will burn brilliantly in the carbonic acid. There it is burning, you see, in an atmosphere of carbonic acid with a very brilliant light, thus showing that even this gas,—carbonic acid—because it contains oxygen, and because it is capable of decomposition, may be regarded as a supporter of combustion, although we generally look upon it as a type of those gases which possess in a high degree the power of extinguishing combustion.

Weekly Evening Meeting, Friday, March 22, 1861.

*The Rev. JOHN BARLOW, M.A. F.R.S., Vice-President,
in the Chair.*

*On the Origin of the Parallel Roads of Lochaber (Glen Roy),
Scotland, by Professor H. D. ROGERS, F.R.S. F.G.S. &c.*

THE speaker prefaced his account of these curious features in the scenery of Lochaber, by stating that he was induced to recall attention to them from having during four recent visits to the ground, discovered certain phenomena not hitherto noticed or theoretically considered by any of the able and distinguished observers who have preceded him. Though nearly all the more prominent peculiar characters of the scene have been very skilfully described and discussed by Dr. MacCulloch, Sir Thomas Dick Lauder, Charles Darwin, Esq., David Milne Home, Esq., Professor Agassiz, Sir George S. Mackenzie, Robert Chambers, Esq., and others, Professor Rogers has been led by a careful

study of the structure of the so-called Parallel Roads, and a perusal of the views of those eminent geologists, to reject all the hypotheses thus far offered in explanation of the terraces as inadequate, and to recognize in the facts about to be developed, a key to a solution of the problem of their origin, which he thinks may prove satisfactory.

The geographical area of the parallel roads may be defined as embraced between Loch Laggan and Loch Lochy, east of the Great Caledonian Valley. They are chiefly restricted indeed to Glen Spean, Glen Roy, and two or three immediately adjacent smaller glens. One belt of them ranges from near Spean Bridge up the Spean Valley to beyond the head of Loch Laggan; another up Glen Roy to the water-sheds at its very head, and a third through Glen Gluoi to its head.

The "Roads," or Shelves, themselves, are of various heights above the sea, the lowest of the three conspicuous ones in Glen Roy having an elevation of about 850 feet, the middle one a height of about 1060 feet, and the highest a level of nearly 1140 feet. Other much fainter, still more elevated shelves, are discernable in Glen Gluoi, but all hitherto seen lie below a horizon of 1500 feet above the ocean. These Parallel Roads, as they are called, are apparently level, and therefore parallel, but further instrumental measurements are necessary before the question of their absolute horizontality can be regarded as satisfactorily settled.

They constitute a most impressive feature in the scenery of the lonely, treeless glens containing them. Winding into all the recesses and round the shoulders of the mountains which they imprint, they present at first view a striking likeness to a succession of raised beaches deserted by their waters.

Seen in profile, as when looked at horizontally, they resemble so many artificial hill-side cuttings, the back of each terrace lying with the general profile of the mountain slope while the front or outer edge is protuberant beyond it. Each is indeed a nearly level, wide, deep groove, in the easily eroded boulder drift, or diluvium, which to a greater or less thickness everywhere clothes the sides of these mountains. They vary greatly in their relative distinctness, being in some places vaguely discernable, while in other spots they indent the surface very plainly, just as they happen to be narrow and to coincide in slope with the hill, or to be broad and apparently level from front to back. Where most indistinct they are frequently not discernable at all when we stand upon them; though we may in a favourable light have detected their position and course from the opposite side of the glen, or, better still, from the bed of the valley. The conditions which influence this fluctuation in distinctness promise if carefully observed, to dispel much of the obscurity which has hitherto invested the origin of the terraces. The modifying circumstances seem to be all referable to one general condition, that of exposure to a current or inundation, supposed by the speaker to have rushed through these glens from their mouths to their heads, or upper ends. Thus it would appear: 1st, With scarcely an exception, that each terrace or shelf is most deeply imprinted in the hill-side, and is broadest where the surface thus grooved has its aspect down the glen on towards the Atlantic and is faintest where the ground fronts towards the head of the valley on the German Ocean. 2nd, While conspicuous on the open sides and the westward sloping shoulders of the hills, the terraces disappear altogether in the recesses or deeper corries which scollop the flanks of the mountains. 3rd, Each shelf, or "road," grows usually more and more distinct as it approaches the head of its own special glen, until those of the two opposite sides meet in a round spoon-like point.

A fact obviously material to a true theory of the origin of the terraces, is that each of them coincides accurately in level with some water-shed or notch in the hills leading out from its glen into some other glen or valley adjoining,

a coincidence suggestive of the notion that they were formed by the grooving agency of a flood pouring through the glens while it was embayed at the respective levels of these natural waste weirs. In confirmation of this view that they were transiently caused by erosive currents held successively at the heights of the barriers on whose levels the terraces terminate, we have as another interesting general feature, a remarkable ruggedness of the bed of each external glen just outside the water-shed or barrier closing the glen which contains the terrace. These rough and deep ravines, contrasting strikingly with the smooth spoon-like terminations of the terrace-lined glens which head against them, strengthen the suggestion already awakened by the marks of horizontal erosion in the terraces themselves, that the notches or passes which determined the grooving of the hill sides on their one hand were externally the sites of so many stupendous cataracts.

The internal structure or disposition of the matter composing each terrace, affords a further and striking corroboration of this hypothesis of the passage of an erosive flood. It consists in an "oblique lamination," or slant bedding of the constituents of the shelves; viz., the layers of gravel, sand, and other sediment, such as geologists familiarly recognize as the result of a strong current pushing forward the fragmentary material which it is depositing, and which is held by them to indicate in the direction towards which the laminae dip, the direction towards which the current has moved. Now, it is a most suggestive peculiarity in the oblique bedding of these terraces, that the "dip," or downward slant, is almost invariably up the glen, or towards its head, and not down the glen, or towards the Atlantic, as we must suppose it would have been had the glen been a bay of the sea, and these materials but portions of ordinary sea beaches. Indeed, this feature is of itself enough to suggest an origin due to a strong current sweeping inward from the Atlantic, and across the water-shed of the island to the opposite sea.

The speaker next proceeded to examine the hypotheses of his predecessors in this inquiry respecting the origin of the Parallel Roads. They all assume the agency, in one form or another, of standing water, either the ocean in its ordinary state of repose, or lakes pent within the glens.

The notion that a quietly resting sea has fashioned these level shelves is refuted by the fact that they are not true marine beaches; they exhibit none of the distinctive features of genuine sea-shores, not a vestige of any marine organic remains, no rippled sands, no shingle, and no sea cliffs. They display in like manner a total absence of the distinctive marks of lake sides; not one lacustrine organism, neither fresh-water plant, nor animal having ever been discovered imbedded in them. A further difficulty attends the lake-hypothesis in the necessity it imposes of discovering a feasible cause of blockage of the glens at different stations above their mouths, to pond the waters to the respective heights of the terraces. Though much ingenuity has been expended upon this part of the problem no suggestions yet offered of barriers of gravel, accumulated by currents of glaciers from Ben Nevis, can be regarded as admissible, inasmuch as there are no traces of any such in any of those localities where alone we can assume them to have existed to produce the required embaying of the waters. In this entire absence of all remnants of the supposed natural dams across the glens, it is most unphilosophical to take for granted their total obliteration, where no cause has or can be assigned which can have so effaced them.

On the other hand, the hypothesis of successive "sea margins," or sea levels, is overthrown by the now well-established deduction from the speaker's own recent measurements, that none of the several shelves, or "roads," of Glen Roy correspond in level with any of those seen in

the adjacent valley Glen Gluoi, a marked discrepancy separating the two groups of terraces into two independently produced systems. It can be shown, moreover, that these discordances of interval between the shelves of the glens respectively, are such as cannot be accounted for on any supposition of "faults," or dislocation of the earth's crust, in the ground between the two glens. Equally incompatible are all the facts of the relative levels of the shelves, with the notion that they are possibly sea beaches which may have undergone an unequal amount of elevation by an oblique secular rise of the land, such as is known to be very gradually taking place on some coasts at the present day. The individual terraces are too nearly level to admit of this explanation; since so wide a warping of the crust from horizontality within so limited a space as separates the two glens, would have left them conspicuously sloping. Besides, the two systems of shelves are wholly insulated from each other, and the notion of their origin as sea beaches gradually elevated implies a continuity between them, together with certain agreements in their directions of derivation from levelness which we wholly fail to perceive.

In conclusion, the speaker proceeded to sketch the action to which he ascribes the formation of all these shelves, or parallel roads. He supposes the several terraces to have been cut or grooved in the sides of the hills by a great inundation from the Atlantic, engendered by some wide earthquake disturbance of the ocean's bed, and forced against the western slope of Scotland. The features of the country indicate that, while a portion of such a vast sea-tide entering the Firth of Linnhe rushed straight across the island through the deep natural trench, Glen Mor, or the Great Caledonian Valley, a branch current was deflected from this, and turned by the Spean valley and its tributary glens Glen Roy and Glen Gluoi, into the valley of the Spey, and so across to the German Ocean. In this transit, the deflected waters first embayed in these glens, and then filling and pouring through them, would, upon rising to the levels of the successive water-sheds, or low passes, which open a way to the eastern slope of the island, take on a swift current through each notch, and as long as the outpour nearly balanced the influx, this current temporarily stationary in height, would carve or groove the soft "drift," of the hill side. But the influx increasing, the stationary level and grooving power of the surface stream would cease, and would only recommence when the flood rising to the brim of another natural dam a new temporary equilibrium would be established, a new horizontal superficial current set in motion, and a second shelf or terrace begin to be eroded at the higher level. So each of the parallel roads is conceived to have been produced in the successive stages of the rising of one vast steady incursion of the sea. The lapsing back of the waters, unaccompanied by any sharp localized surface currents, through the passes, could imprint no such defined marks on the surface, nor accomplish more than a faint and partial obliteration of the terraces just previously excavated during their incursion. This procedure was elucidated by likening it to what takes place when we allow a steady but gradually increasing jet of water to flow into the tank, perforated laterally with several orifices at successive elevations, the outlets permitting a somewhat less rapid rate of discharge than is equivalent to the influx. If such a tank be smeared internally with soft clay, the inpour can be so regulated in respect to its acceleration, that the water, as it rises successively to the levels of the several orifices will take on a horizontal motion or current, through, first the lower hole, and then the second, and so on, and, remaining approximately stationary for a brief while on the level of each, will groove the soft clay as it passes out, until it swells above the orifice to reach the next. Some such process as this at the notches which terminate the glens will, it is believed, account for the terraces and all the features which belong to them.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Carbonate of Potash.—The amount of water which carbonate of potash contains has been debated by several chemists. Wackenroder, Phillips, Bérard and Giese, and now Dr. J. J. Pohl (*Sitzungsber. d. Akad. der Wissensch. zu Wien.*, bd. xli. s. 630) have severally examined the salt with a view to its determination. The last found in a saturated solution of potash which had been kept in a stoppered bottle for more than a year, some six-sided crystals which, removed into the air, quickly attracted moisture and liquefied. The qualitative analysis of these crystals showed them to be composed of potash, carbonic acid, and water, with mere traces of chlorine and sulphuric acid. When heated to 100° C. they lost 5.180 per cent. of water. Further investigations showed that they contained considerably more water, which could only be expelled by a much higher temperature. The salt dried over sulphuric acid lost, when heated to redness, 15.994 of water, and had the following composition:—

Carbonate of potash	83.517
Water	15.994
Chlorine, sulphuric acid, and loss	0.489
	100.000

This closely approaches the formula $2(KO, CO_2) 3HO$, which would give in a hundred parts carbonate of potash 83.676, water 16.324. As, however, the water expelled above 100° was only 10.814, the author decides that the salt was KO, CO_2, HO and the excess of water hygroscopic moisture. When exposed to the air this salt quickly attracts 4.5 more water, and then has the formula $2(KO, CO_2) 3HO$. By taking up a very small quantity more water the salt liquefies to an oily fluid.

II. ORGANIC CHEMISTRY.

Phenamine.—Phenamine is the name given by Scheurer Kestner (*Polytech. Centralblatt.*, 1861, s. 395) to aniline violet, the composition and constitution of which he has discussed in a review of a work by Willm. In the preparation of his aniline violet the author used hydrochlorate of aniline and chloride of lime. In this process, besides the violet precipitate, a brown colouring matter and a resinous body are produced. The brown matter is present in quantity, and mostly remains in solution. It dyes silk and wool a chestnut brown, and is easily separated from the violet matter in consequence of its solubility in the alkaline liquor. The resinous matter is insoluble in weak spirit, water, and acetic acid, but is easily taken up by pure alcohol, sulphide of carbon, benzole, and ether. With the general properties of aniline violet our readers are well acquainted, and we need not repeat them. When it is dissolved in strong acetic acid, and the solution is allowed to stand for a time, a crystalline crust forms, and the sides of the dish become covered with small crystals. When these are washed free from the acetic acid and dried, they have a greener colour and more lustre than the amorphous mass obtained by evaporating an alcoholic solution. The author analysed both the amorphous mass and the crystals, and obtained the following numbers:—

	Amorphous.		Crystals.	
	I.	II.	I.	II.
Carbon	73.30	74.06	73.58	74.60
Hydrogen	5.96	5.71	6.10	6.10
Nitrogen	11.31	11.46	—	11.92

(The first specimens in both cases were dried at 100°, and the second at 110°.) Aniline violet, therefore, contains less nitrogen than aniline. From the above numbers the author derives the formula $C_{30}H_{14}N_2O_2$ for the crystalline product dried at 110°. The formation of aniline violet

from aniline cannot be satisfactorily explained as long as the composition of the secondary products remains unknown. The author gives the following equation, which represents the change as involving the loss of water and the elements of ammonia:— $5(C_{12}H_9N) + 8O = 2(C_{20}H_{14}N_2O_2) + NH_3 + 4HO$. If this be correct, the change resembles the formation of naphthamein from naphthalidin, whence the author proposes the name phenamein for aniline violet. There is this difficulty, however: ammonia is found in the liquid separated from the precipitated naphthamein, but none can be discovered in that separated from the aniline violet. The author, in the course of his experiments, determined that to change 100 grammes of aniline into aniline violet a quantity of chloride of lime was required which accorded to 12 grammes of oxygen; that is, nearly $1\frac{1}{2}$ equivalents of oxygen to 1 equivalent of aniline.

Alkapton.—Alkapton is the name which Professor Boedecker, of Gottingen (*Annalen der Chem. und Pharm.* bd. cxvii. s. 98), has given to a body he has extracted from urine, which reduces oxide of copper like sugar, but which will not ferment. It may be separated from the urine in the same way as diabetic sugar. When purified and dried, it forms a yellow transparent mass without taste or smell. When mixed with soda lime the author says no ammonia is evolved, but when heated alone in a glass tube an alkaline vapour with a disgusting smell is developed. Alkapton is soluble in alcohol and water, but almost insoluble in pure ether. The yellow aqueous solution reddens litmus. An alkaline solution exposed to the air rapidly absorbs oxygen, and becomes of a dark colour. The other reactions have nothing remarkable. The author, unfortunately, leaves us quite in the dark as to the composition of alkapton.

MISCELLANEOUS.

THE POLYTECHNIC INSTITUTION.

PROBABLY few of our Metropolitan readers know the danger which has threatened this useful institution within the last few months. The important locality in which it is situated and the convenient arrangement of its halls and theatres are too tempting for those whose mission it is to provide the public with a less intellectual kind of amusement; the present Directors have, in fact, only a month or two since, found a competitor for the purchase of the Institution in one of the most extensive projectors of modern music-hall entertainments, and it was for some time a doubtful question as to who would become the proprietor. Science, however, is not yet driven from its popular resort, and, under the energetic management of Mr. J. S. Phené, who has been re-appointed managing director, we hope that the vitality and progress of this Institution will be secured for many years to come. On Saturday last there was a private view of a new gallery of modern paintings which has just been opened. We heartily wish success to this new feature in the Polytechnic Institution, the attractions of which, in an amusing and instructive form, have latterly been of a class to demand public support to a greater extent than, we fear, has been accorded it. We understand it is to be succeeded in the winter by an exhibition of the works of deceased artists. At a meeting of some friends of the Institution, who adjourned after the entertainments to Willis's Rooms, to a cold collation given by the managing director, he pledged himself to raise the credit and usefulness of the Institution to a high pitch, if fairly supported by the public, which he certainly ought to be.

Everything seems now to promise that the Polytechnic will rapidly regain that position in public estimation which it has so long and deservedly enjoyed. We have no reason to imagine that such an institution, under judicious

management, should not become one of the most attractive places of entertainment in the Metropolis. It is quite an error to suppose that the public do not care for science. On the contrary, no subject is capable of exciting more genuine interest, and, if it has the reputation of being dry and difficult to understand, it shows that the expounder has been in fault, not Science herself.

We think, nevertheless, that some improvement might be made in the character of the entertainments offered. For some years they have sadly lacked novelty and variety. We look in vain for illustrations or expositions of the latest discoveries in science; these are seldom introduced to the notice of the frequenters of the Institution until they have ceased to be novelties. Every now and then some startling discovery is made. It attracts considerable interest even amongst the general public, and most literary journals devote a column or two of their space to its popularisation. The gorgeous colouring matters extracted from coal tar, and the recent brilliant spectrum discoveries are instances which at once occur to our minds. It should be the manager's duty to lay wait for such novelties as these and instantly to present them to the public in such a form that their leading features could be readily understood. If, when the interest in these commenced to flag, other more recent discoveries were illustrated in a similar manner, the public would be gradually led to resort to the Polytechnic whenever an important scientific discovery was announced with the certainty of finding it there fully illustrated. The Polytechnic would then be to the middle what the Royal Institution is to the upper classes, and would be patronised in as generous a manner. The lecture on Professor Wheatstone's new Universal Private Telegraph is a step in the right direction, and we hope to see the everlasting diver and diving-bell and the "electric fluid," which is daily "made to assume volumes of beautiful colours," replaced before long by some less hacknied scientific exhibition. The change which has just been effected by the appointment of so enterprising a manager as Mr. Phené will be a good opportunity of getting rid of some of these time-honoured traditions. We shall watch with interest the steps which will, doubtless, be taken by this gentleman to keep up with the progress of scientific discovery.

ANSWERS TO CORRESPONDENTS.

. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

. All Editorial Communications are to be addressed to Mr. CROOKES, and Advertisements and Business Communications to the PUBLISHERS GRIFFIN, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the CHEMICAL NEWS, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can still be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

H. Sherman.—Already published.

Kemas.—Received.

M. B., Macclesfield.—Several good formulæ have already been given for black ink.

J. A. D.—Your communication contains nothing new; the first suggestion is impracticable, the second has been accomplished for many years.

J. E.—The specific gravity of pure benzol is 0.885. It boils at 176° F. The specific gravity of the nitric acid used in converting it into nitrobenzol is 1.5. In practice, however, a mixture of strong commercial nitric acid and oil of vitriol is used in equal quantities.

A Subscriber.—1. It is supposed to be loss of water. 2. It has, but not so thoroughly as to decide the points you name. 3. Such analyses could not be done. It requires an experienced analyst with all the resources of a laboratory to detect many adulterations.

THE CHEMICAL NEWS.

VOL. III. No. 82.—June 29, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Property of Platinum rendered Incandescent by an Electric Current to Produce Gaseous Combinations, by M. E. SAINT-EDMÉ.

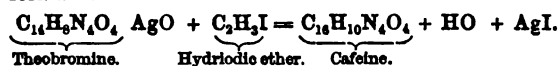
I WILL first draw attention to the sensibility of iodised starch paper as a re-agent for oxygen compounds of nitrogen, hyponitric, and nitric acids, infinitely minuter traces of which may be indicated by sensibly blueing the starched paper.

M. Leroux's experiment, which consists in obtaining ozone by passing a current of air on a platinum wire made red-hot by electricity, has led me to inquire whether oxygen only can be modified under the same conditions. I find that pure oxygen passing in contact with a spiral rendered incandescent by an electrical current produces no action on iodised starched paper, and consequently does not appear to be modified. If, on the contrary, oxygen and nitrogen are made to arrive at the same time in contact with the red-hot spiral, the issuing gas colours strongly the iodised starched paper blue, and reddens litmus paper. From this I infer that, under these conditions, nitric acid is formed. I am at present continuing these researches with various gases.—*Comptes-Rendus.*

Transformation of Theobromine into Caffeine, by M. STRECKER.

THEOBROMINE and caffeine are homologues, differing only by C_2H_2 , of which the latter contains the larger amount. M. Strecker has now prepared caffeine artificially, by giving to theobromine the elements it lacked. This was effected by heating the combination formed by theobromine and oxide of silver, in a closed vessel, with methylhydriodic ether. The product, treated with alcohol, yielded crystallizable caffeine in silky threads.

This reaction is easily intelligible by the following formula:—



—*Neues Repert. für Pharmacie*, vol. x. p. 32.

On the Estimation of Aluminium by Standard Solutions, by MM. ERLÉNMEYER and LEWENSTEIN.

By employing a standard solution of ammonia or potassa to estimate the aluminium contained in alum, it has been found that the weaker the alum solution, the more exact is the result; doubtless because the sulphate becomes more basic according to the weakness of the solution. However, as it is impossible to completely avoid the formation of this basic salt, the difficulty is evaded by

transforming the double sulphate into chloride of aluminium by means of chloride of barium.

Potassa, as well as ammonia, entirely decomposes chloride of aluminium, whether neutral or basic. When in this state, aluminium can be estimated easily by standard solutions, and the authors bring forward numerous instances in support of their opinion.—*Zeitschrift für Chem. und Pharm.*

TECHNICAL CHEMISTRY.

On the Employment of Lighting Gas in Acieration, by M. GRUNER.

In the *Comptes-Rendus* of the 11th of last March, M. Fremy put the question to metallurgists whether his experiments relating to the conversion of iron into steel by means of lighting gas could not be practically utilized. In the name of metallurgists, let me be permitted to reply, that practice has long since positively decided this point.

Twenty-five years ago, Mr. Macintosh, an ingenious Glasgow manufacturer, made several tons of cemented steel by submitting iron, at a dull red heat, to the action of lighting gas, operating with from 100 to 150 lbs. at a time, the iron bars being two inches broad and six lines thick. The cementation took from eighteen to twenty hours, and when the operation exceeded that time supercarburization took place.

M. Dufrénoy published these details in the third series of the "Annales des Mines," vol. v. p. 171. He had himself seen specimens of this steel, a portion of which was melted and then worked by the ordinary means. M. Dufrénoy says that the supercarburated thin bars nearly resembled graphite. Thus, then, by the sole action of lighting gas, without mixture of any foreign body, it is possible to obtain either steel or cast iron; it is only a question of time or temperature. To obtain steel there is no need to add ammonia previously in order to nitrogenise the iron. In fact, as M. Caron judiciously remarked, at the meeting of March 18th, coal gas always contains ammonia, and it is not my intention to deny its influence on cementation. I will not further attempt to solve the question of the presence or absence of nitrogen in steel: but it appears to me certain that if nitrogen exists in steel it exists equally in cast iron. Moreover, it is twenty years since Dr. Schafhäütl, of Munich, stated positively that he had found nitrogen in cast iron.

It must be remembered that in ordinary cementation with wood charcoal, as in Mr. Macintosh's experiment, iron is *gradually* brought to the state of steel, and then to that of cast iron. There is no precise limit between these three stages. At what period of the operation, and by what reaction will the nitrogen previously absorbed, again quit the iron? In malleable cast iron,

whence comes the nitrogen if not contained in the cast iron itself? And in puddled steel, how can nitrogen combine with iron or carbon if cast iron does not contain it? I showed in a paper on puddled steel, published a year ago in the "Annales des Mines," vol. xv., that the fining of cast iron in a reverberatory furnace takes place under a coating of slag containing iron and manganese when puddled steel is to be obtained. I ask, then, how the hot atmospheric nitrogen of a furnace can combine with iron and carbon across this coating of scoria? Certainly, if puddled steel contains nitrogen, it can only proceed from cast iron, and it appears to me as interesting to prove its presence in cast iron as in steel itself. But let me be allowed to raise some doubts on the possibility of proving the presence of nitrogen in steel by hydrogen. At red heat iron takes away the nitrogen from ammonia and sets the hydrogen at liberty; and at the same temperature will this hydrogen again take away the nitrogen from the iron,—from the iron which is always in excess relatively to the gaseous molecules which can react on it? It is more difficult to conceive the production of ammonia under these circumstances than the direct combination of hydrogen with free nitrogen.

Another fact which proves that steel and cast iron differ only in containing diverse proportions of the same elements is, that pure white cast irons can be tempered and even forged like steel; witness the white cast iron of Sieges, used for making screw-plates.

Finally, if forged natural steel contains nitrogen, this element ought also to be found in cast iron; and in the second place, it has long been proved that iron can be transformed at will, either into cast iron or steel, by ordinary or coal gas cementation. To effect either, a difference only of time and temperature suffices.—*Comptes-Rendus*.

PHARMACY, TOXICOLOGY, &c.

Note on Blood-stains, by M. GUIBOUT.

ONE of the problems of chemical jurisprudence most frequently presenting itself is that of determining the nature of supposed spots of blood, and one of the most usual methods consists in treating the spots in a manner accurately described in special works with a small quantity of distilled water. This water, in the case supposed, dissolves the colouring matter of the blood, leaving the fibrin on the spot. The liquid, which is red and transparent, loses its colour by boiling, and forms a grey coagulum, which a small quantity of caustic potash will re-dissolve. The liquid, again become transparent, appears either reddish or green, according to the manner of looking at it.

Let us suppose an operation carried on in a straight glass tube closed at one end. The alkaline liquid prepared as is about to be described, will appear green by transmitted light; that is to say, when the tube is placed between the eye and the day light. If, on the contrary, the experimenter stands between the tube and the light, the liquid appears reddish, the colour being perceived from the same side as the reflected rays.

M. Picquot, junior, a pharmacist of Bar-le-Duc, when called upon to decide whether the spots found on some planks in the house of a suspected murderer were spots of blood, after having obtained the preceding results, was much puzzled by reading in M. Devergie's "Médecine Légale" that the sanguineo-alkaline liquid is

green seen by reflection and rose coloured by refraction (vol. iii. p. 822). M. Gaultier, of Claubry, is even more positive. In his "Chimie Légale," p. 7, he says:—"The liquid takes then a green tint seen by reflection and rose by refraction. . . . The green colouration by reflection, rose or reddish by refraction, is a certain proof of the presence of blood." M. Picquot finds here a confusion of words, and I quite agree with him. First, the green colour seen by transmitted light on placing the tube between the eye and the light is evidently not a reflected colour. Again, this same green colour is refracted, since the luminous rays cannot traverse the liquid without undergoing refraction on entering the liquid and again on emerging from it.

Only the reddish colour seen from the side whence the light proceeds simultaneously with the reflected rays can be said to be seen by reflected light; but it must be remarked that it is not reflected directly by the exterior surface of the liquid. It proceeds from all the internal points, undergoes a multitude of internal reflections, and is finally refracted on its exit before reaching the eye.

For the sake of accuracy, it is necessary to substitute in place of the characteristic phrase of MM. Devergie and Gaultier, of Claubry, the following proposition:—The sanguineous liquid (obtained after re-dissolving in caustic potash the coagulum produced by heat), if observed in a straight tube, appears green when placed between the eye and the sun, and reddish if the observer is placed between the sun and the tube.

A straight tube is requisite, because if a flask or any other vessel of a certain diameter is used, the liquid appears rose coloured under all circumstances. If a half-filled flask is shaken the lower portion appears rosy, but the supernatant froth appears green. In a full flask, seen by reflected light, the whole appears rose coloured; when seen by transmitted light the liquid contained in the spherical part of the flask always appears red; whilst that in the straight neck appears green. It is astonishing to observe the same liquid in one part of the vessel appear green and in another part reddish. These variations in no degree invalidate the certainty of the conclusion that no other substance than blood unites the characteristics successively observed in the experiment, namely,—

1. The red colour of the liquid.
2. Its decoloration and coagulation at boiling heat.
3. The grey colour of the coagulum, and its complete solubility in a small quantity of caustic potash.
4. The rose colour of the alkaline coagulum seen by reflected light in a straight glass tube, and its green colour under transmitted light.

5. The distinct and simultaneous appearance of two colours in a small straight-necked flask when the quantity of liquid is large enough to admit of its observation.

All this is without prejudice to the microscopic observation of spots and search for blood globules, which constitute the essential characteristic and most certain indication of the presence of blood.—*Journal de Pharmacie et de Chimie*.

On the Commercial Purification of Bitter Almond Oil, by JOHN S. BLOCKEY.

THE poisonous nature of this substance in its crude state warrants the conclusion that some method of perfectly freeing it from prussic acid or combined cyanogen should be universally adopted, either in its first formation from the almonds or in the subsequent rectification.

Whether even then it is quite harmless is, I think, yet to be proved, and the experiments detailed below tend to show that it possesses very powerful properties, although not so poisonous as the crude oil. Possibly, like many essential oils, it may be found available in medicine. Gerhardt states that the oxide of mercury will free the crude oil from cyanogen, but I have repeatedly tried the efficacy of this agent both alone and with water at various temperatures and during various durations without any success. I also assured myself that the HCy is not in a free state, for no effervescence takes place on the addition of NaO CO₂, either in solution or otherwise. I next tried caustic potassa, but the only effect was to produce a quantity of benzoate of potassa equivalent to the amount of alkali employed. The hydrated oxide of mercury appears to have no action. I concluded, therefore, that some plan must be adopted to free the cyanogen from the oil, which would possess sufficient affinity for that purpose, and at the same time not exert any action on the free hydride of benzoyl. After many attempts, one of which consisted in trying chloride of iron and lime, I finally adopted the following plan. I had found that on mixing a small quantity of oxide of mercury and water, and agitating the oil, and then adding a little caustic potassa and filtering, that the oil passed through clear and colourless and entirely free from cyanogen; but I found that the purified oil dissolves appreciable quantities of the mercury, and if chemically pure hydride of benzole be digested on dry oxide of mercury and filtered it will be found to dissolve it to a certain extent. I therefore procured a wrought iron still having a very long neck, around which a stream of cold water was allowed to pass, so as to cool the vapour sufficiently in order not to crack the glass tubes of the condenser. I found that an ordinary metal worm was speedily dissolved by the distillation of mercury. The pure oil does not dissolve metallic mercury as it does its oxide. In the retort of the still I placed a quantity of crude oil, a small quantity of solution of caustic potassa, and lastly, the quantity of oxide necessary. The actual weight of the latter has to be varied. Some kinds of oil contain more cyanogen than others, and the requisite quantity of oxide was determined on a small quantity and filtration. The quantity of potassa required is, strange to say, very small in all cases, and I always found that, although distillation with HgO and water alone never succeeds in freeing the cyanogen, a small quantity of caustic added produces perfect separation. More alkali must not be added than necessary, or the high heat of the distillation will cause the formation of benzoate; and if the operation be effected by means of a current of steam, the water dissolves large quantities of the oil. For this reason as little water must be added as will suffice to dissolve the caustic alkali, and by careful management of the heat nearly all the water comes over before any oil distils.

Towards the close of the operation, large quantities of a thick smoke suddenly pour over smelling strongly of some of the acryls compounds. I have been unable to determine at present to what these fumes are due, but I have noticed that as the oil distills it gets darker in colour, and at last a tarry matter containing nitrogen comes over, and I have sometimes, by very cautious management, been able to obtain all the pure oil some seconds before the sudden evolution of fumes. The oil then requires filtration from small globules of mercury, and one-tenth of rectified alcohol to be added,—the use of spirit is to prevent the gradual decomposition of the

pure oil, as it becomes turbid and speedily deposits crystals, especially if not perfectly secured from the air. I have never yet had an opportunity of determining the composition of this substance, although I have kept a quantity for some years till an occasion should offer. It sublimes, leaving carbonaceous residue, melts at a low heat, and solidifies again to a crystalline mass.

I intended pursuing this investigation further, but other matters have since occupied me. I should have liked to determine the composition of the tarry residue in the retort, of the acrid vapours, and of these crystals; also to ascertain whether the oil dissolves other oxides, and to what extent. As I cannot foresee any opportunity for so doing for some little time, I am compelled to postpone a paper on these subjects. Meantime, I send the result of some experiments on the effects of the pure and impure oil on the living animal.

Fifteen fluid minims of crude oil administered to a full-grown cat, produced all the poisoning symptoms of prussic acid: violent, spasmodic contractions, &c.; the slightest touch producing a contraction in the limb touched; in twenty-four hours the cat was alive and apparently well.

Twice the above dose killed another cat almost instantly.

Thirty fluid minims of pure oil seemed to act powerfully at first as a stimulant, terminating, after a minute or two, in loss of all power of motion, gradually diminishing, and the animal eventually got well.

The above doses were all administered in olive oil.

Thirty fluid minims of pure oil administered alone killed a cat in some hours.

Ninety minims of pure oil alone killed a cat instantly.

Further experiments suggested themselves, but the destruction of life is not a pleasing subject for experiment, and the above are sufficient to show the poisonous nature even of pure oil, and that this effect is greatly modified by the presence of olive oil.

Heckmondwike, Yorkshire.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting, Friday, May 17, 1861.

*The Duke of NORTHUMBERLAND, K.G. F.R.S., President,
in the Chair.*

*On the Theory of Three Primary Colours,
by Professor J. CLEEK MAXWELL.*

THE speaker commenced by showing that our power of vision depends entirely on our being able to distinguish the intensity and quality of colours. The forms of visible objects are indicated to us only by differences in colour or brightness between them and surrounding objects. To classify and arrange these colours, to ascertain the physical conditions on which the differences of coloured rays depend, and to trace, as far as we are able, the physiological process by which these different rays excite in us various sensations of colour, we must avail ourselves of the united experience of painters, opticians, and physiologists. The speaker then proceeded to state the results obtained by these three classes of enquirers, to explain their apparent inconsistency by means of Young's Theory of Primary Colours, and to describe the tests to which he had subjected that theory.

Painters have studied the relations of colours, in order to imitate them by means of pigments. As there are only a limited number of coloured substances adapted for painting,

while the number of tints in nature is infinite, painters are obliged to produce the tints they require by mixing their pigments in proper proportions. This leads them to regard these tints as actually compounded of other colours, corresponding to the pure pigments in the mixture. It is found that by using three pigments only, we can produce all colours lying within certain limits of intensity and purity. For instance, if we take carmine (red), chrome yellow, and ultramarine (blue), we get by mixing the carmine and the chrome, all varieties of orange, passing through scarlet to crimson on the one side, and to yellow on the other; by mixing chrome and ultramarine we get all hues of green; and by mixing ultramarine with carmine, we get all hues of purple, from violet to mauve and crimson. Now these are all the strong colours that we ever see or can imagine: all others are like these, only less pure in tint. Our three colours can be mixed so as to form a neutral grey; and if this grey be mixed with any of the hues produced by mixing two colours only, all the tints of that hue will be exhibited, from the pure colour to neutral grey. If we could assume that the colour of a mixture of different kinds of paint is a true mixture of the colours of the pigments, and in the same proportion, then an analysis of colour might be made with the same ease as a chemical analysis of a mixture of substances.

The colour of a mixture of pigments, however, is often very different from a true mixture of the colours of the pure pigments. It is found to depend on the size of the particles, a finely ground pigment producing more effect than one coarsely ground. It has also been shown by Professor Helmholtz, that when light falls on a mixture of pigments, part of it is acted on by one pigment only, and part of it by another; while a third portion is acted on by both pigments in succession before it is sent back to the eye. The two parts reflected directly from the pure pigments enter the eye together, and form a true mixture of colours; but the third portion, which has suffered absorption from both pigments, is often so considerable as to give its own character to the resulting tint. This is the explanation of the green tint produced by mixing most blue and yellow pigments.

In studying the mixture of colours, we must avoid these sources of error, either by mixing the rays of light themselves, or by combining the impressions of colours within the eye by the rotation of coloured papers on a disc.

The speaker then stated what the opticians had discovered about colour. White light, according to Newton, consists of a great number of different kinds of coloured light which can be separated by a prism. Newton divided these into seven classes, but we now recognize many thousand distinct kinds of light in the spectrum, none of which can be shown to be a compound of more elementary rays. If we accept the theory that light is an undulation, then, as there are undulations of every different period from the one end of the spectrum to the other, there are an infinite number of possible kinds of light, no one of which can be regarded as compounded of any others.

Physical optics does not lead us to any theory of three primary colours, but leaves us in possession of an infinite number of pure rays with an infinitely more infinite number of compound beams of light, each containing any proportions of any number of the pure rays.

These beams of light, passing through the transparent parts of the eye, fall on a sensitive membrane, and we become aware of various colours. We know that the colour we see depends on the nature of the light; but the opticians say there are an infinite number of kinds of light; while the painters, and all who pay attention to what they see, tell us that they account for all actual colours by supposing them mixtures of three primary colours.

The speaker then next drew attention to the physiological difficulties in accounting for the perception of colour. Some have supposed that the different kinds of light are

distinguished by the time of their vibration. There are about 447 billions of vibrations of red light in a second; and 577 billions of vibrations of green light in the same time. It is certainly not by any mental process of which we are conscious that we distinguish between these infinitesimal portions of time, and it is difficult to conceive any mechanism by which the vibrations could be counted so that we should become conscious of the results, especially when many rays of different periods of vibration act on the same part of the eye at once.

Besides, all the evidence we have on the nature of nervous action goes to prove that whatever be the nature of the agent which excites a nerve, the sensation will differ only in being more or less acute. By acting on a nerve in various ways, we may produce the faintest sensation or the most violent pain; but if the intensity of the sensation is the same, its quality must be the same.

Now, we may perceive by our eyes a faint red light which may be made stronger and stronger till our eyes are dazzled. We may then perform the same experiment with a green light or a blue light. We shall thus see that our sensation of colour may differ in other ways, besides in being stronger or fainter. The sensation of colour, therefore, cannot be due to one nerve only.

The speaker then proceeded to state the theory of Dr. Thomas Young, as the only theory which completely reconciles these difficulties in accounting for the perception of colour.

Young supposes that the eye is provided with three distinct sets of nervous fibres, each set extending over the whole sensitive surface of the eye. Each of these three systems of nerves, when excited, gives us a different sensation. One of them, which gives us the sensation we call red, is excited most by the red rays, but also by the orange and yellow, and slightly by the violet; another is acted on by the green rays, but also by the orange and yellow and part of the blue; while the third is acted on by the blue and violet rays.

If we could excite one of these sets of nerves without acting on the others, we should have the pure sensation corresponding to that set of nerves. This would be truly a primary colour, whether the nerve were excited by pure or by compound light, or even by the action of pressure or disease.

If such experiments could be made, we should be able to see the primary colours separately, and to describe their appearance by reference to the scale of colours in the spectrum.

But we have no direct consciousness of the contrivances of our own bodies, and we never feel any sensation which is not infinitely complex, so that we can never know directly how many sensations are combined when we see a colour. Still less can we isolate one or more sensations by artificial means, so that in general when a ray enters the eye, though it should be one of the pure rays of the spectrum, it may excite more than one of the three sets of nerves, and thus produce a compound sensation.

The terms simple and compound, therefore, as applied to colour sensation, have by no means the same meaning as they have when applied to a ray of light.

The speaker then stated some of the consequences of Young's theory, and described the tests to which he had subjected it:—

- 1st. There are three primary colours.
- 2nd. Every colour is either a primary colour, or a mixture of primary colours.
- 3rd. Four colours may always be arranged in one of two ways. Either one of them is a mixture of the other three, or a mixture of two of them can be found, identical with a mixture of the other two.
- 4th. These results may be stated in the form of colour-equations, giving the numerical value of the amount of each colour entering into any mixture. By means of the

Colour Top,¹ such equations can be obtained for coloured papers, and they may be obtained with a degree of accuracy showing that the colour-judgment of the eye may be rendered very perfect.

The speaker had tested in this way more than 100 different pigments and mixtures, and had found the results agree with the theory of three primaries in every case. He had also examined all the colours of the spectrum with the same result.

The experiments with pigments do not indicate what colours are to be considered as primary; but experiments on the prismatic spectrum show that all the colours of the spectrum, and therefore all the colours in nature, are equivalent to mixtures of three colours of the spectrum itself, namely, red, green (near the line E), and blue (near the line G). Yellow was found to be a mixture of red and green.

The speaker, assuming red, green, and blue, as primary colours, then exhibited them on a screen by means of three magic lanterns, before which were placed glass troughs containing respectively sulphocyanide of iron, chloride of copper, and ammoniated copper.

A triangle was thus illuminated, so that the pure colours appeared at its angles, while the rest of the triangle contained the various mixtures of the colours as in Young's triangle of colour.

The graduated intensity of the primary colours in different parts of the spectrum was exhibited by three coloured images, which, when superposed on the screen, gave an artificial representation of the spectrum.

Three photographs of a coloured ribbon taken through the three coloured solutions respectively were introduced into the camera, giving images representing the red, the green, and the blue parts separately, as they would be seen by each of Young's three sets of nerves separately. When these were superposed, a coloured image was seen, which, if the red and green images had been as fully photographed as the blue, would have been a truly-coloured image of the ribbon. By finding photographic materials more sensitive to the less refrangible rays, the representation of the colours of objects might be greatly improved.

The speaker then proceeded to exhibit mixtures of the colours of the pure spectrum. Light from the electric lamp was passed through a narrow slit, a lens and a prism, so as to throw a pure spectrum on a screen containing three moveable slits, through which three distinct portions of the spectrum were suffered to pass. These portions were concentrated by a lens on a screen at a distance, forming a large, uniformly coloured image of the prism.

When the whole spectrum was allowed to pass, this image was white, as in Newton's experiment of combining the rays of the spectrum. When portions of the spectrum were allowed to pass through the moveable slits, the image was uniformly illuminated with a mixture of the corresponding colours. In order to see these colours separately, another lens was placed between the moveable slits and the screen. A magnified image of the slits was thus thrown on the screen, each slit showing, by its colour and its breadth, the quality and quantity of the colour which it suffered to pass. Several colours were thus exhibited, first separately, and then in combination. Red and blue for instance, produced purple; red and green produced yellow; blue and yellow produced a pale pink; red, blue, and green produced white; and red and a bluish green near the line F produced a colour which appears very different to different eyes.

The speaker concluded by stating the peculiarities of colour-blind vision, and by showing that the investigation into the theory of colour is truly a physiological inquiry, and that it requires the observations and testimony of persons of every kind in order to discover and explain the various peculiarities of vision.

¹ Described in the Transactions of the Royal Society of Edinburgh, Vol. XXI., and in the *Philosophical Magazine*.

NOTICES OF BOOKS.

A Familiar Introduction to the Study of Polarised Light. By C. WOODWARD, F.R.S., President of the Islington Literary and Scientific Society. Third Edition, carefully revised. London: J. Van Voorst. 1861.

We feel sure that chemists might make more frequent and more serviceable use of the polariscope than they have yet done. When the crystals of the substance under examination can be obtained of sufficient size, sections may often be advantageously used in the experiments, and will frequently afford more information than the confused mass of small crystals obtained by evaporating a drop of the solution on a glass slide under the microscope. In using the microscopic goniometer also the margins of crystals may be conveniently made distinctly visible by the colours which they transmit when viewed in polarized light.

Mr. Woodward's "Familiar Introduction" does not attempt much, but what it attempts it certainly accomplishes. Nothing can be more satisfactory than the way in which the mysteries of this difficult subject are cleared up. The verbal descriptions and illustrations, as well as the diagrams themselves, are easily intelligible to any reader, even to one who approaches the subject for the first time.

Mr. Woodward divides his little work into two parts: the first of these contains an account of the phenomena of polarised light, and the various theories which have been propounded to explain these phenomena; while the second part is devoted to a detailed description of the table polariscope and microscope. These instruments were devised by the author, and are admirably adapted for showing in a conspicuous manner some of the most intricate and beautiful of optical phenomena.

The few remarks on the historical development of this branch of science are interesting, and though brief, serve well as an introduction for the student to the fundamental experiment of Dr. Young on the interference of the waves of light. This experiment is explained with admirable precision and clearness. The next few pages give a very satisfactory view, by means of diagrams and descriptions, of the apparatus by means of which the primary phenomena of waves may be shown. Newton's experimental researches on the cause of the colours of thin plates follows and then the description of the way in which colours are produced by the different lengths of the paths which the rays from grooved surfaces or those which pass through doubly-refracting substances travel. We are thus introduced to polarisation proper. It would not be doing the author justice were we to quote from these sections of his work—the explanations of the various phenomena cannot suffer abbreviation without detriment to their perfection, for not a redundant word is to be found. We can only say to our young readers, and to those who are anxious to have a clear and connected idea of how the various phenomena of polarisation may be satisfactorily explained on the undulatory theory of light, read this third edition of Mr. Woodward's book, studying especially such illustrations as are to be found on pages 29, 30, 31, and 34.

CORRESPONDENCE.

Magenta Powder.

To the Editor of the CHEMICAL NEWS.

SIR,—Practical manufacturers know very well how to prepare a powder which will dissolve freely in the dye-bath, and produce a magnificent shade, either the blue Solferino or red Magenta; and I could now supply solbs. of such a preparation. It is found that dyers will not use it, they prefer a solution of which they can measure a

quantity sufficient for their purpose instead of weighing a powder. There are various methods of producing such preparations—a bright green bronze powder or a red one at about 8os. the lb., which may be diluted in strength and price to any extent, or the pure colour itself may be used. No advantage results in practice from preparing the powder for sale, as it merely saves the expense of spirit, but very little trouble. As to dyeing cotton without a mordant, I cannot discover any means of dyeing that will stand soap—although the mordant does not improve the colour, far from it.—I am, &c. "MARENEST."

"Batopia," June 24.

Transferring the Colours of Marbled Paper.

To the Editor of the CHEMICAL NEWS.

SIR,—I have been experimenting upon paper similar to the enclosed to transfer its colours to other paper and book-edges, but, unfortunately, I am no practical chemist, and have only been able to transfer the reds on the sheet, and that by steeping it in weak, warm soda and water, and afterwards applying a hot iron to it when covered with a wet cloth. Even this changes the colour of it materially, but the blues and other colours will not move. I have seen them transferred quite perfectly, every colour distinct, and believe some acid is employed. I believe there are but two persons in this country who can do it, and they will not part with the secret,—though I think it must be quite simple,—even if paid for it.—I am, &c.

W. LANG.

Sulphuric Acid.

To the Editor of the CHEMICAL NEWS.

SIR,—The writer would feel obliged if any of your readers would inform him if (and where) pure sulphuric acid is to be obtained in quantity,—that is, acid free from lead, arsenic, iron, chlorine, &c. The writer needs a pure acid for certain manufacturing operations wherein even traces of the above foreign matters are injurious.

Can any one inform him of the cause of the universal prevalence of these impurities in the English acid of commerce of the present day? He is informed (but whether correctly so or not he has been unable to determine) that a pure acid, at a moderate cost, is obtainable somewhere on the Continent.

An answer will greatly oblige

A SUBSCRIBER.

MISCELLANEOUS.

Ventilation.—The subject of efficient ventilation of dwelling-houses without creating a draught or admitting dust is a problem of more difficulty than many imagine. It is true that there are several ways of accomplishing this, provided *Paterfamilias* has a long purse and does not object to workmen in his house for several weeks in the year; but to effect the desired end with a trifling expense and without interfering with any present arrangement of the room or building is no easy task. On Saturday last Professor Pepper gave a lecture, at the Marylebone Literary and Scientific Institution, on a novel means of ventilation, illustrating Cooke's patent. After describing experimentally the properties of air, pure and vitiated, hot and cold, and the various plans which had been hitherto devised for replacing foul air by fresh, the lecturer described Cooke's arrangement, by which it is proposed to obtain ventilation in public buildings, dwelling houses, carriages, and ships, without admitting dust or draught. It consists of a series of folds of fine wire gauze, fastened to the top of the upper sash of the window, and made to fold up, so that when the window is closed nothing is seen. When ven-

tilation is required, the top of the window is opened, and the gauze screen comes down as much as is desired, affording means of egress for the vitiated with a gentle influx of pure air. The ventilator may be of two or more folds, of any size or width, and can be readily fixed or detached so as to permit the window sash being opened to the full extent for cleaning or any other purpose. It is simple and inexpensive, excludes dust and dirt, and, the admission of air being imperceptible, may be kept in use with safety in sick rooms and sleeping apartments during the night, when a constant supply of fresh air is mostly required. By its use the inconvenience and danger of admitting currents or draughts of air is entirely avoided.

Conversations at Bath House.—On Wednesday, the 20th inst., Lord Ashburton held a scientific conversation at his town residence, Bath House, Piccadilly, which was numerous and very fashionably attended. Among other scientific instruments which were collected together and experiments shown, Mr. Darker contributed an apparatus illustrating several points in the beautiful phenomena of the polarisation of light. Messrs. Powell and Lealand, Smith and Beck, and Baker exhibited several binocular microscopes, under which, among other objects, were seen the "volvox globata," the circulation of the blood in the branchiae of the newt, and the circulation of the sap in *Valeriana*. Among other inventions shown were Professor Wheatstone's new printing telegraph, and wave machine, for explaining the theory of the undulations of light, the *Bulldog* deep sea sounding machine, and Johnson's sounding apparatus and thermometer. Some large articles in aluminium attracted much attention, especially an elaborately-ornamented helmet, which, though massive in appearance, scarcely exceeded in weight that of an ordinary hat. Messrs. Spencer Browning and Co. exhibited their new spectroscope, for the purpose of analysis by light, and during the evening exhibited the various spectra of barium, strontium, calcium, potassium, lithium, and sodium, and also, by means of an adjusting prism, showed the spectra of barium and lithium, in the field of view of one of the instruments at the same time for the purpose of comparison. Three different sizes of the instrument were exhibited, the smallest, which is intended for tourists, when closed being capable of packing in a morocco case, the outside dimensions of which were five inches by four and three-quarter inches, the thickness being only one and a-half inches. Even with this small instrument the sodium line D could be distinctly seen double. The portability and efficiency of the apparatus were highly commended by all who inspected it. Mr. Ladd also exhibited spectrum apparatus, and Messrs. Elliott Brothers some interesting experiments in the diffraction of light by means of the oxyhydrogen lime light.

ANSWERS TO CORRESPONDENTS.

*. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Gleason—Outlines of Analysis.—We do not know the publisher of this work. A London bookseller could doubtless get it for you.

E. J. Rutter.—Received with thanks.

A. D. M. D.—No letters have yet arrived. We will forward any that come.

F. Chadwick.—No such maps are published in this country. A few metals only have been copied.

C. Eades.—We will gladly insert the practical hints you suggest. Will you forward those which you say you could give? and we will insert others as soon as they can be obtained.

Books Received.—"Dictionnaire de Chimie Industrielle," par MM. Barreuil et Aimé Girard. Introduction and first volume. Paris: Dezobry, E. Magdeleine et Cie., 78, Rue des Ecoles. "On Poisons and the best Means for Preventing Accidents," in connection with the patent safeguard bottles originated by Messrs. Savory and Moore.

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END OF VOL. III

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CHEMISTRY OF THE COMET.

PROBABLY few of our readers are ignorant that a most brilliant member of this erratic celestial family is now a conspicuous object in the northern heavens. It first made its appearance on Sunday evening last, after sunset, when, as the clouds cleared away, it was suddenly discovered shining as bright as a star of the first magnitude, and almost rivalling the magnificent comet of 1858 in brilliancy and development of tail, whilst it far surpassed it in the diameter of nucleus.

An opportunity which perhaps may never occur again is thus allowed for physicists to become acquainted with the intimate constitution of these mysterious visitors. On the occasion of the last appearance of so brilliant an object, in 1858, philosophers could do little more than examine it through their telescopes, and wonder, and speculate on its constitution. Since then, however, science has made rapid strides, and we are at the present time in possession of methods of analysis vastly more searching and powerful than any known previously. Chemists can now analyse, in the most rapid and accurate manner, almost everything which is visible to the human eye. It matters not whether the body possesses a tangible substance or not, whether it is close at hand in a test-tube or millions of miles away; if it only fulfils the one condition of emitting light, it is almost certain to reveal the secrets of its composition when submitted to the new development of *spectrum analysis*.

The actual metallic constitution of the sun has already been shown by this means; that of the fixed stars is likewise being ascertained in the same manner, and we really hope that the present opportunity will not be allowed to pass without the beautiful method of spectrum analysis being applied to determine the elementary constitution of cometary bodies. The apparatus required would not be more than is already in the hands of many scientific men. A large telescope, equatorially mounted, would, from some experiments of our own upon this subject, seem to be necessary, in order to obtain sufficient light to illuminate the field of view. The spectrum apparatus being placed in proper adjustment, the luminous image only requires to fall on the slit and along the axis of the collimating tube for the fixed lines in the cometic spectrum to be instantly visible. A small and temporary arrangement, hurriedly fitted up, has been sufficient to satisfy us of the existence of fixed lines in the spectrum of the light emitted from the comet at present visible, but they were too faint to admit of identification. This experiment should at once be repeated with larger and more powerful apparatus. Many important questions would thus be finally settled. If the comet shone merely by reflected light from the sun, the ordinary solar fixed lines would be the only ones visible. If, however, as is most probable, some of its splendour is due to native light, the spectrum would as readily reveal whether the nucleus or the tail were in the solid or vaporous state;

if the former, it would give a continuous spectrum, whilst if it were a vapour, the spectrum would be disconnected, and the new lines in it would at once disclose the elementary bodies to which they were due. It is, however, impossible to foresee all the valuable information on the obscure subject of stellar chemistry which could in this manner be obtained. We trust that, having pointed out this most promising field of observation, the rich harvest of facts with which science can thereby be enriched will not be allowed to escape for want of able observers.

WATER *versus* FIRE.

THE natural antagonism of fire and water is so deeply rooted a matter of popular belief that were it not for the striking illustration to the contrary exhibited by the recent gigantic conflagration near London-bridge, we should scarcely venture to raise a doubt on this point. A visit which we paid to the burning ruins soon after the outbreak, and some observations which we made on that occasion while accompanying the firemen as near as could be ventured to the blazing mass, have, however, served to confirm our previous impressions in a remarkable manner.

The action of water in quenching fire is principally due to two causes. One is its great capacity for heat, a property which enables it to exercise a considerable cooling influence upon any burning body. The other is its fluidity, enabling it to run over the surface and penetrate the pores of a combustible body, acting the part of a varnish, sealing it up from contact with the atmospheric oxygen, and thereby stopping combustion. In the case of water supplied from a fire engine, the mechanical force with which it impinges upon the burning body may likewise have some beneficial effect, actually beating out the fire as if with a stick. It will however be seen, that unless the volume of water thrown on to a fire be very large indeed in proportion to the bulk of that fire it can do but little good; and if it be very small in comparison to the amount of burning material, it is positively injurious, acting the part of additional fuel to the flames.

Every chemist knows what takes place if a small quantity of water is projected into the midst of a large mass of red hot combustible matter. It is decomposed in presence of the carbon into carbonic oxide and hydrogen, each highly combustible gases, and thereby tending to increase the volume of flame. This we are satisfied was the case with a large proportion of the water which was thrown on during the first few days of this memorable fire. Standing inside the gates of Cotton's Wharf we watched a jet of water from the powerful steam fire-engine, as the firemen directed it into a brightly burning mass of combustible matter. The liquid hissed and vaporised the instant it touched the incandescent matter on which it was directed; but in this and in most other instances where water was

directed into the fiercest parts of the fire, the supply of heat from combustion was so much in excess of the loss of heat required to vaporise the water, that, except at the spot on which the solid jet of liquid fell, there was scarcely any appearance of deadening or quenching, and as soon as the hose was directed towards another part of the blazing mass, the spot on which the water had been previously falling glowed as brightly as ever.

The supply of water, however, had not only a negative action, in some cases it did positive injury; for after falling into the brightly glowing fuel and becoming at once vaporised, it rose through the thick masses of red hot combustible matter, and there underwent decomposition. To a careful observer the result could be seen in the form of an increased vigour in the tongues of flame, which rose and licked the neighbouring walls with renewed violence. We were curious to see if this decomposition and subsequent recombustion of the water could be rendered visible, and on a subsequent occasion examined the flames by means of a small pocket spectroscope. A portion of the ruins was chosen which still blazed with almost its original fierceness, and on which a stream or two of water was being pumped. The spectroscope was at first turned towards a portion of the flames where there was no water; as might be expected the spectrum was quite continuous; a faint glimmer only of the yellow soda line being occasionally visible; but upon the men directing their hose upon this portion, the yellow line instantly shot across the field of view with great splendour, and remained there during the whole time the water was being poured into the burning mass. We subsequently found that the presence of this line offered an easy means of telling whether water was being poured into any portions of the burning ruins. Where water was absent the line was only occasionally visible, whilst whenever water was added to the flames it appeared with considerable brightness, projected across the continuous spectrum.

Now, we submit that this is a good (though indirect) proof that the water had suffered decomposition, and that some portion of the flames which were towering towards the sky consisted of the burning elements of this identical water, which thus acted the part of fuel to the fire. The great difficulty of preparing "water gas," even in the laboratory, so as not to burn with a yellow flame, is well known, and it is therefore unlikely that when formed under circumstances eminently suitable for the production of the yellow soda flame, this should not be present in considerable quantity.

Another point that struck us most forcibly when watching the firemen with their hose, was the utter absurdity of the attempt to produce any effect upon so stupendous a conflagration with such an insignificant jet of water. Three acres of combustible matter were blazing and roaring like a blast furnace, and just at the outskirts of the burning heap a few thin streams of water were being sprinkled on, producing about as beneficial an effect as if a child had taken a penny squirt to it, not even wetting it so much as a heavy shower of rain would have done. It is scarcely part of our province to suggest an improvement on this state of things. To place a floating fire-engine on the Thames capable of raising any number of tons of water per minute, and deluging an incipient fire in so perfect a manner that further outbreak would be impossible, seems a not very difficult problem, although it is one for mechanicians rather than chemists. Like all other difficulties now-a-days, the only question requiring answer is, "How

much will it cost?" and whatever that might be, it could not possibly come up to what the Insurance Offices will have to pay for this one fire.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Colour-tests for Strychnia and the Diagnosis of the Alkaloids, by WILLIAM A. GUY, M.B. Cantab., Fellow of the College, and Professor of Forensic Medicine, King's College, London.

(Continued from vol. III. p. 303.)

I NOW proceed to discuss the third of the four questions propounded in my first communication, namely, Are the colour-tests, or is the selected test, open to any serious objection?

Of chemical tests in general, it may be said that they are more or less liable to the following objections or disparagements:—1. They may be wanting in delicacy; that is to say, they may not be able to detect minute quantities of the substances to which they are applied as tests. 2. They may be wanting in certainty; in other words, they may fail from slight causes difficult to appreciate and define, such as the quantity or proportion in which they are applied, or the strength of their solutions if used in the form of liquid. 3. They may give rise to transient effects, or to appearances difficult to describe or define. 4. They may be unusually liable to failure from impurities in themselves, or in the matters to which they are applied as tests. And, lastly, they may produce of themselves appearances nearly allied to those which they occasion when added to the matters under examination.

From the first of these objections all the colour-tests for strychnia are certainly free. They are able to detect very minute quantities of the poison. From the fourth objection, too, as it will presently be shown, they are at least as free as the majority of chemical tests. To the second, third, and last objections they are certainly exposed, but under what circumstances and in what degree, it will be necessary to inquire.

The occasional failure of the colour-tests for strychnia from slight and obscure causes is not to be denied. It is proved by the experiments already detailed, and is admitted by all experienced chemists. But it should be understood that this objection has only a limited application. It goes only to the extent of impairing the value of the evidence derived from a negative result. If the colour-tests fail when applied to a spot acted on by sulphuric acid, their failure is not conclusive evidence of the absence of the poison. It affords a strong probability of such absence, but nothing more.

The reactions of the colour-tests for strychnia are certainly transient. They are not like the permanent white, crystalline, or coloured precipitates afforded by most chemical tests. But the objection on this ground is also of limited application. In experiments with the colour-tests made in the manner described in a former communication, we obtain two classes of results. We may get a mere trace or "flash" of blue, or we may obtain a deep rich blue colour, lasting for several seconds, for half-a-minute, or even for forty-five seconds, and succeeded by the other tints already described. Now those who maintain the value of the colour-tests for

¹ Substance of part of the Croonian Lectures for 1861, delivered at the Royal College of Physicians; from the *Pharmaceutical Journal*.

strychnia do not pretend that a mere flash of colour affords such evidence of the presence of strychnia as could be used with safety in a court of law; but they would probably be found to assert with equal confidence that a deep rich blue colour, developed by the colour-producing tests for strychnia, and continuing for several seconds in a spot previously treated with sulphuric acid without change of colour, and then succeeded by other colours, affords good evidence of the presence of strychnia. Though the blue colour soon passes away, there is ample time afforded for its complete recognition.

In answer to the objection that the succession of colours is difficult to describe or define, it will suffice to state that though it may not be possible by any words that we can use to describe the successive colours developed by the colour-tests, it is not necessary to do so. For the first deep rich blue colour, characterised by the property of want of permanence, is in itself so remarkable as to constitute a most valuable and trustworthy test for strychnia.

The colour-tests for strychnia are certainly open to the objection that the colour-developing substances are themselves coloured. The peroxide of lead is puce coloured, the peroxide of manganese black, the bichromate of potash orange coloured, the ferricyanide of potassium yellow or orange, and the permanganate of potash lilac. The peroxide of lead and the peroxide of manganese also colour sulphuric acid by admixture, and the bichromate of potash, ferricyanide of potassium, and permanganate of potash change the colour of the acid by solution. But it has been shown that the colours thus produced cannot possibly be confounded with the strychnia blue, and consequently form no valid ground of objection.

The fourth of the five objections applicable to chemical tests in general, certainly does not apply with any force to the colour-tests for strychnia; for I can state as the result of experiments with a great variety of substances, vegetable and animal, including all the matters that are likely to remain mixed with strychnia after its extraction from the contents of the stomach, or from the fluids and tissues of the body, and further with many mineral substances, including common salt, arsenious acid, tartar emetic, and corrosive sublimate—all these animal, vegetable, and mineral substances having been added in quantities far exceeding those in which they could possibly exist as impurities in strychnia extracted from the contents of the stomach, or from the fluids and tissues of the body,—that the colour-tests are little, if at all, affected by such admixtures. This observation applies to the permanganate of potash equally with the bichromate of potash.

For practical purposes, the reaction which demonstrates the presence of strychnia may be described as a clear, rich blue colour, developed by the colour-tests in a spot previously treated by strong sulphuric acid without change of colour, the blue colour in question lasting long enough for perfect recognition, and then passing into other colours. In the absence of the blue colour we can affirm the probable absence of strychnia, but a mere flash of colour does not afford a degree of evidence upon which we ought to rely in a medico-legal inquiry.

Assuming the description just given to be correct, I now proceed to inquire whether there are any known substances which give rise to a similar reaction. The answer must be, in a qualified sense, in the affirmative; but it must be understood the reaction is only *similar*, not the same.

Such a substance is to be found in cod-liver oil, which,

when treated with strong sulphuric acid, assumes a purple tint, mixed with streaks of blue, changing after a time to mulberry, and from that to a red-brown. In these changes of colour there is such a general resemblance to the strychnia tints as would justify their being put forward as an objection to the colour-tests, but for the very obvious consideration that with sulphuric acid alone strychnia undergoes no change of colour. The very fact, too, that cod-liver oil is an oil, while strychnia is tested either as an alkaloid or the salt of an alkaloid, as a crystal or as a white powder, or as a dry deposit from a liquid holding it in solution, deprives this objection to the colour-tests for strychnia of all practical value.

Again, aniline is mentioned as a source of fallacy. But aniline is a coloured liquid, which yields with sulphuric acid a dense white precipitate, the sulphate of aniline. Strychnia, on the other hand, is a crystal or white powder, or a dry deposit from a liquid holding it in solution.

But the sulphate of aniline, which is formed by adding the strong sulphuric acid to the liquid aniline, is sold as a white powder, as are some specimens of strychnia. Hence the sulphate of aniline might possibly prove a source of fallacy, inasmuch as it gives no change of colour with sulphuric acid, and yet when treated with the colour-producing substances yields a colour similar to the blue of strychnia. But the sulphate of aniline, in place of a rich, deep blue, passing speedily into mulberry, and at length into bright red, assumes at first a pale green, which deepens by slow degrees, then passes into a beautiful blue, continuing unchanged for a long period, but at length turning to a black. So that the first characteristic colour with strychnia is the second with aniline, and the transient colour with strychnia is persistent with aniline.

Again, there is a red crystalline substance, pyroxanthine, which produces with sulphuric acid alone, without addition of the colour-producing tests, a blue similar to that produced by strychnia. But there is here no real fallacy, because strychnia is not coloured, and does not yield any colour with sulphuric acid alone.

Nor do papaverine and narceine, which are tinged purple by sulphuric acid alone, create any difficulty, or occasion any fallacy. And the same may be said of the whole body of alkaloids and active principles, whether coloured or colourless, which yield colours when treated with sulphuric acid alone. Of those which resemble strychnia in giving no colour with sulphuric acid, it may be stated with equal confidence that not one of them yields with the colour-developing substances the true strychnia colours, or any colours resembling them.

The foregoing are given as examples of the sort of objections that are urged against the colour-tests for strychnia. They belong to the class of objections which no one out of a court of law would bring forward as worthy of serious attention. They are based, not so much on a misapprehension of the facts, as on an illogical comparison of dissimilar phenomena. But there is one objection founded on a misapprehension and quite unintentional misrepresentation, which ought not to be passed over in silence. It is stated by Dr. Taylor, in his work on Poisons, that the alkaloid curarina obtained from the South American poison curara, "resembles strychnia in the action of the colour-tests, brucia in the action of nitric acid, and both in its intense bitterness." These are the words used by Dr. Taylor at p. 786 of the second edition of his work; and on referring to p. 772, he says of the same alkaloid, that "strong nitric acid

produces with it a blood-red colour; sulphuric acid gives with it a rich carmine tint, in which characters it resembles brucia," and, further, that Pelikan found pure curarina to possess the chemical properties of strychnia, so far as the colour-tests are concerned, and with more constant results. Thus, sulphuric acid when conjoined with the action of chromate of potash, ferricyanide of potassium, or peroxide of lead, "produced a beautiful play of colours," and "that the galvanic test acted similarly." Now, on referring to the passage cited from Pelikan, I find that this beautiful play of colours is a translation of the words, "des colorations rouges très belles," and "la coloration rouge," the one expression being applied to the chemical tests, the other to the galvanic test. So that the alkaloid curarina is set up as a fallacy on the strength of its yielding with the colour-tests (sulphuric acid mixed with chromate of potash, ferricyanide of potassium, or peroxide of lead) a brilliant red colour, or colours, instead of the beautiful play of colours, blue, mulberry, and red, which jointly, and not separately, characterise strychnia. If any one colour is to be deemed the strychnia colour *par excellence*, it is the rich deep blue, and not the brilliant red. Let it also be remembered that sulphuric acid gives with curarina, according to Bernard, a rich carmine tint, while strychnia undergoes no change of colour when treated with sulphuric acid. The statement made by Dr. Taylor that in giving a blood-red colour with strong nitric acid and a rich carmine tint with sulphuric acid, curarina resembles brucia, is evidently founded in error. The reaction of brucia with nitric acid is correctly stated, but brucia certainly does not yield a carmine tint with sulphuric acid. It is one of the list of substances grouped together in my tables as characterised by a negative action with sulphuric acid; and Mr. Jenkins, in the table which I sent round in my last lecture, attributes to it a negative reaction with sulphuric acid (see CHEMICAL NEWS, October 6, 1860); and here I may observe that it is one of the obvious advantages of tables such as these that they place before us the common and familiar reactions of the substances which they comprise, in such an orderly and methodical way as greatly to assist the memory, and to guard against such inadvertent statements as this of Dr. Taylor's respecting brucia.*

On referring to the tests for brucia itself, as given at page 801 of Dr. Taylor's work on Poisons, from which I have been quoting, I find it stated that "strong sulphuric acid colours brucia of a rich rose-pink colour." This colour differs widely from the rich carmine (*belle teinte carminée*) of Bernard, said by Dr. Taylor to be characteristic of the same reaction, and I think that I may venture to state of it that it is not the reaction of pure brucia with pure sulphuric acid. Indeed, it is not the reaction of specimens of brucia of ordinary purity with specimens of sulphuric acid of similar purity. To obtain a rich rose pink with brucia, we must use a specimen of sulphuric acid containing an unusual quantity of nitric acid, and I have no doubt that the unintentional use of such an acid gave rise to this misapprehension. I may add, that to specimens of brucia of fair average quality sulphuric acid will impart a very slight tint (rose colour or buff), and a still slighter tint to the solution of the alkaloid in the acid. But a rich rose-pink colour is an evidence of the presence of nitric acid, and is readily developed in the solution of the

alkaloid in sulphuric acid by a short exposure to the vapour of nitric acid.

I could not have accomplished the object I have in view of vindicating the colour-tests for strychnia from unfounded objections, without tracing this particular objection to its true source. It was necessary to show that the reactions of the rare alkaloid curarina, differ essentially from those of strychnia, and of brucia, which is associated with strychnine in more than one of the plants that yield it; and I could not do this without showing that an esteemed medico-legal authority had made a mistake in reference to a reaction, and an oversight in rendering the words of a French author.

I may add, that on comparing the extract of nuxvomica with a specimen of curara poison, for which I am indebted to the kindness of Mr. Morson, and with the brown powder scraped from an arrow tipped with the same poison, and forming one of a collection kindly presented to me by Mr. Stone, of the College of Surgeons, I find no resemblance between the one extract and the other, except in the property of intense bitterness. The woorara poison imparts to strong sulphuric acid a greenish-brown tint, and the nuxvomica a pink colour; while strong nitric acid, which turns the first to a pale, dirty yellow, changes the second to a very distinct orange colour. The addition to the solution in sulphuric acid of bichromate of potash produces less effect on the colour of the curara than takes place with the extract of nuxvomica. If the woorara really contained any alkaloid capable of developing with sulphuric and nitric acids the deep rich colours attributed to it, I think that some approach to those colours might be expected in the extract itself.

I may conclude this part of my subject by expressing my firm conviction that to the colour-tests applied in the manner I have recommended, there is no valid logical objection; and that though a negative result is not conclusive of the absence of strychnia, a positive result (by which I must be understood to mean the development of a rich blue colour on stirring the colour-developing substances into a spot first treated with sulphuric acid without change of colour, the rich blue colour in question passing, after a few seconds, into other colours) affords undeniable proof of its presence.

In my next and last communication I shall endeavour to discriminate the principal alkaloids from each other, and to represent the differences between them by convenient tabular forms.

(To be continued.)

On the Oleaginous Matter formed on Dissolving different kinds of Iron in Dilute Acids, by EMERSON J. REYNOLDS.

PROUST, I believe, was the first chemist who noticed that the hydrogen prepared by dissolving black cast iron in dilute sulphuric acids possessed a most offensive odour, and that on being passed through glass tubes it caused them to assume a greasy appearance, apparently due to the deposition of an oily matter, which he appears to have considered as a combination of hydrogen and iron, the non-production of which body under the circumstances I will demonstrate further on.

Berzelius afterwards noticed the formation of this oleaginous body, and gave it as his opinion that it was a compound of carbon and hydrogen. More recently, Professor Cameron (in 1855) made some experiments on this oil, the results of which he communicated to the

* These tables will be referred to more particularly in my next paper.

Chemical Society of Dublin. He found it to be very volatile, and destitute of nitrogen, and also that certain metallic salts removed the offensive odour produced by its presence.

But, after all, two very important points have been left undetermined, namely, the precise conditions under which this body is formed, and its chemical and physical characters and relations.

Now, it is the first of these propositions that I intend dealing with in the present paper, reserving the second for a future communication. I had contemplated waiting until my investigations were completed before publishing the results; but in consequence of some observations made by M. Chevreul, before the Academy of Sciences of Paris, I thought it better to bring forward what I have determined up to the present time. Chevreul stated that the combination of the iron with the hydrogen seemed difficult to admit, and that this consideration had led him to conjecture that the water might assist in the production of the oily matter. He, moreover, considered that the researches of M. Fremy¹ on cast iron and steel, seemed to throw some light on the subject, by indicating that it is not carbon, as was represented, which yields the oily body. Now, we can clearly gather from this that M. Chevreul points to nitrogen as being, possibly, the principal constituent of the oil, and not carbon.

In order to clear away all doubts as to its origin, it was essentially necessary to answer the three following propositions:—

1. Is the oleaginous body produced during the solution of pure iron in dilute acids?
2. Does iron, free from every other impurity but carbon, yield the oil when treated with acids?
3. Will iron, containing nothing but nitrogen, furnish the oily matter on similar treatment?

It will be seen at a glance, that the careful solution of these propositions would be certain to make us acquainted with the circumstances which influence its production.

The following are the methods which I adopted for answering each question:—

1. My first object was to prepare absolutely pure iron. This I procured by the process generally employed for preparing "*fer réduit*," care being taken to preclude any possible chance of impurity. The peroxide being precipitated from the pure perchloride by ammonia, after being well washed, it was then submitted to a low red heat. Moreover, the hydrogen gas used to decompose the peroxide was evolved from distilled zinc and pure sulphuric acid. The metallic iron so procured, when dissolved in dilute acid, did not yield the slightest trace of the oil. I did not come to this conclusion after one or two experiments alone, but after repeated trials. This overthrows at once the supposition that the iron combines directly with the nascent hydrogen forming the oil, and, indeed, does more, for its non-formation tends to confirm, if such were necessary, the opinion of Dr. Cameron formerly expressed in this Journal, that such a compound as ferruretted hydrogen does not exist.

2. In answering the second question, it was essential to obtain iron containing some carbon in combination; this I prepared as follows:—Carburetted hydrogen in a pure state was passed over the iron of Experiment 1, heated to redness; the action was allowed to go on for half-an-hour. In this way I obtained iron containing carbon—in fact, cast iron. This compound, when dis-

solved in acids, gave the oil in such quantity as to allow of its being identified with the greatest ease. This experiment proves with certainty that carbon is essential to the existence of the oil, though, of course, it does not furnish us with any information as to whether the elements of water enter into its constitution or not. This can only be found by its ultimate analysis.

3. The third point to be considered in connection with this subject is, whether or not nitrogen gives rise to any oil. To determine this, another portion of the iron of Experiment 1 was submitted to the action of dry ammoniacal gas for nearly two hours and a-half, by which means it became highly nitrogenised (as was proved by the abundant evolution of ammonia on heating in an atmosphere of hydrogen). This body, when treated with dilute sulphuric acid, which readily attacks it, does not, when so acted on, give the least trace of oleaginous matter. Of this I have satisfied myself; but, instead, a solution of ammoniacal sulphate is produced.

To sum up, then, my experiments show the following:—

1. Pure iron does not yield the oil.
2. Iron containing carbon does give the oil.
3. Iron containing nitrogen does not furnish the oleaginous matter.

So far for the origin of this curious body: it now remains for me to determine by analysis its composition, &c. On this subject I am actively engaged at present, and hope to turn to some account the experimental results which I have arrived at in this paper, so as to furnish me with a means of preparing this oil in larger quantity than I have heretofore obtained it.

TECHNICAL CHEMISTRY.

On Electro-Chemical Colouring and the Deposition of Peroxide of Iron on Plates of Steel and Iron, by M. BECQUEREL.

PRIESTLY was the first to obtain coloured rings by means of electricity,¹ by receiving strong charges from a battery, with surface of about two square metres, on metal plates, by means of metallic points directed perpendicularly to their surface.

Nobili, in 1827,² afterwards produced coloured rings on platinum, gold, silver, and brass plates, by putting them in communication with one of the two poles of a voltaic pile, plunging them into metallic and non-metallic solutions, and then by directing perpendicularly to their surface a platinum point connected with the other pole. With positive silver, for instance, and solution of sea salt he obtained a series of concentric circles surrounded with varied irises, the tints being slightly dimmed by contact with the air. On heating the plates all the rings took a red tint.

I began to study the electro-chemical colouration of metals in 1843,³ my chief object being, not to produce coloured rings, but to deposit on plates of gold, platinum, copper, and silver, thin and uniform layers of peroxide of lead presenting successively, according to the duration of the operation, which was generally very short, the rich colours of the spectrum. The operation consists in plunging into an alkaline solution of protoxide of lead

¹ *Philosophical Transactions*, vol. lviii.

² *Annales de Physique et de Chimie*, vol. xxxiv., second series.

³ *Comptes-Rendus*, vol. xviii.

the piece to be coloured, put in connection with the positive pole of a voltaic pile charged with nitric acid and composed of many layers of plates, and closing the circle with a platinum wire in communication with the negative pole, and of which but the point, which alone touches the alkaline solution, is continually in motion. In contact with the object to be coloured, the protoxide of lead, which forms the positive electrode, super-oxidises, becomes insoluble in the alkali, and deposits itself on the surface in slight adherent layers, producing the colour of the thin plates. Air and light gradually fade these colours—a disadvantage I have already mentioned, and which may in great measure be avoided by covering the coloured surface with alcohol varnish, which acts but very slightly on the peroxide. With a little practice all the tints desired may be given to a large object with hollows and projections, and each part painted with the appropriate colour. These colours are now made fast by the processes to be described further on.

If the solution of protoxide of lead in potash is replaced by a solution of protoxide of iron in ammonia, and the gold, copper, or platinum plate by one of polished iron, there is deposited on the latter a layer of peroxide of iron with red or brown tints, which deepen more and more according to the thickness of this stratum, which never exceeds a certain limit on account of the imperfect conductivity of the peroxide.

In a paper on the precipitation of metals from their solutions by other more oxidisable metals, I stated that on plunging a copper plate into a solution of double chloride of potassium and platinum, heated to 60°, the platinum is deposited and adheres to the copper, producing a platinization, which is quickly acted on by the air, taking first a slightly brownish tint, which becomes darker and darker.

This alteration is partly due to the presence of protochloride of copper, which is deposited simultaneously with the platinum towards the end of the operation. The protochloride may be removed by washing the platinised copper with water and acetic acid, or by rubbing its surface with cotton and rouge. The alteration is then avoided, or at least does not appear till long afterwards, probably on account of the air traversing the interstices of the platinum, which with copper forms a voltaic pair. The brownish colour of the platinum surface is like that which protochloride of copper ordinarily takes under exposure to air and light.

If we make use of platinised copper directly it is obtained from a solution of double chloride as a positive electrode to decompose water with a pile composed of several elements, it produces, under the influence of the oxygen disengaged from the positive pole, peculiar colouring effects, seeing that the tints pass immediately to blue and deep crimson, and the protochloride of copper is not modified by light. While in contact with platinum the latter metal doubtless contributes to the colouration. No similar result is obtained by operating with platinised plates previously treated with acidulated water or rouge. Furthermore, air has no effect on the colours produced—not an unimportant fact, as it will make it possible to obtain fast colours also with peroxide of lead.

Owing to the strata of oxide, the result is the same if heat is gradually applied to non-preserved platinised pieces, but the colours are less brilliant.

If a copper plate with a layer of peroxide of lead giving one of the beautiful colours of the spectrum is employed as a positive electrode to decompose the water,

in a few moments it will be found that the coloration is preserved—a result similar to that obtained with platinised copper. By continuing the electro-chemical action for a quarter or half-an-hour, according to the force of the pile, the blue-violet tints fade, turning to green and yellow, as peroxide of lead. The basis of the coloration undergoes no change at the positive pole. It is thought that the acid secondary products formed at the positive pole react on the peroxide and decompose it.

The coloured plates thus preserved seem to be in the same condition as iron when it has been plunged in nitric acid, or when it has been used as a positive electrode to decompose this same acid. It is then in an abnormal state, being unattackable by nitric acid.

When a very slight layer of platinum is electro-chemically deposited on a gold or platinum plate by means of a solution of double chloride of potassium and platinum containing no copper, this layer undergoes no change either by the action of the air or when the plate is employed as a positive electrode to decompose water; but it is otherwise when the solution contains copper. The colouring effects before described are then produced, when the proportion of copper is small and the coloration of the platinum is not destroyed by weak nitric acid—an important advantage practically. In this memoir we have indicated a great improvement in the process of coloration, for by its means more uniform and adherent layers of metals are deposited.

Magnificent colouring effects are produced by a solution of double chloride of potassium and platinum in hyposulphite of soda.

Lastly, deposits of peroxide of iron on iron and steel, which are already almost unalterable by air, will become entirely so when the pieces have been employed as positive electrodes to decompose water.—*Comptes-Rendus*.

PHARMACY, TOXICOLOGY, &c.

Plasmata or Glycerine Ointments.

THE use of a simple plasma or glycerine ointment in eczema and other skin diseases has been common for some years, both in Germany and England. Of late, however, Graefe, of Berlin, has employed it as a vehicle for the application of various active remedies in diseases of the eye.

Simple plasma is made by heating one part of starch with seven parts of glycerine. The heat of a water-bath is not quite sufficient to effect the solution of the starch; and great care must be taken in regulating the heat and continually stirring the mixture to prevent burning. When atropia or belladonna are to be combined with this simple plasma, it will be very important to avoid the burning, which, besides giving a disagreeable appearance and smell, will in these cases seriously affect the remedial value of the compound.

The advantages which this compound possesses over lard, or mixtures of oil and wax, as a vehicle for active remedies, are, that it is soluble in water, retains its consistence at all ordinary temperatures, and, lastly, does not become rancid. It does, however, after a time (usually several months), become mouldy on the surface, but this can be easily removed, leaving the remainder quite good.

With this simple glycerine ointment various substances are now combined, such as atropia, extract of belladonna, extract of opium, sulphate of copper, acetate

* *Comptes-Rendus*, vol. xlvii.

of lead, the ammonio-chloride, nitric oxide, and sub-nitrate of mercury and nitrate of silver. The last is said to be used in Germany, but appears to us a most ineligible compound, for the nitrate commences to decompose the moment it is brought in contact with the starch, and in a very short time is completely reduced.

The plan adopted in the preparation of these ointments may be varied, according as the active agent to be introduced is soluble in glycerine or not. In the former case, as with the extracts before mentioned, atropia and sulphate of copper, they should first be dissolved in the glycerine, and the solutions of the extracts, if necessary, strained; the starch should then be mixed with the solution in a mortar, and the mixture transferred to an evaporating dish to be heated. If the active agents are insoluble in glycerine (as the mercurial salts), they should first be thoroughly mixed with the starch before the glycerine is added. In each case it is advisable to stir the mixture industriously while the heat is applied, as well to ensure the proper distribution of the ingredients as to prevent the burning.

We now proceed to give the formulæ for the compounds which are in use and about to be introduced into the Pharmacopœia of the Royal London Ophthalmic Hospital. It must be premised that the solubility of the menstruum permits the use of a smaller quantity of the active ingredient, and it will be seen that when there is a corresponding ointment in the London Pharmacopœia, the plasma has half the amount of the active ingredient.

Plasma with Sulphate of Copper.

℞ Cupri sulphatis . . .	gr. xxiv.
Pulveris amyli . . .	3jss.
Glycerinæ . . .	3xss.

First dissolve the sulphate of copper in the glycerine, then mix the starch, and heat until the plasma acquires a proper consistence. The sulphate of copper being soluble in the glycerine, this forms a very elegant preparation, of a delicate, transparent, greenish-blue colour.

Plasma with Extract of Belladonna.

℞ Extracti belladonnæ . .	gr. xlv.
Pulveris amyli . . .	3jss.
Glycerinæ . . .	3xss.

First dissolve the extract of belladonna in the glycerine, and, if necessary, strain; then mix the starch with the solution and heat carefully, stirring continually until the plasma acquires a proper consistence.

Plasma with Ammonio-chloride of Mercury.

℞ Hydrargyri ammonio-chloridi .	3ss.
Pulveris amyli . . .	3jss.
Glycerinæ . . .	3xss.

Plasma with Nitric Oxide of Mercury.

℞ Hydrargyri nitrico-oxidi .	gr. xlv.
Pulveris amyli . . .	3jss.
Glycerinæ . . .	3xss.

Plasma with Subnitrate of Mercury.

℞ Hydrargyri subnitratis .	gr. xl.
Pulveris amyli . . .	3jss.
Glycerinæ . . .	3xss.

These three preparations may all be made in the same way. The mercurial salt should first be intimately mixed with the starch and then the glycerine added. It is, of course, important in each case to stir well while the heat is applied. The plasma with the subnitrate is intended to be used in place of the unguentum hydrargyri nitratis,—a preparation to which it does not, however, fully correspond. But there are objections to boiling an acid solution of mercury with glycerine, and if the cold

acid solution be mixed with the plasma it soon becomes almost liquid. The liquid preparation, however, might be useful under some circumstances. The preparation is intended to represent an ointment half the strength of the unguentum hydrargyri nitratis of the London Pharmacopœia.

Plasmata with extract of opium and atropia and acetate of lead may also be easily made. Extract of opium dissolves in glycerine, and this plasma may be prepared in the same way as that with extract of belladonna. When atropia is used great care must be taken in heating the mixture, as the atropia is destroyed at a temperature a little above that of boiling water. Two grains of atropia to one drachm of the plasma is a convenient strength for dilating the pupil of the eye.

PHYSICAL SCIENCE.

On the Nature of the Spark of Ruhmkorff's Induction Apparatus, by M. ADOLPHE PERROT.

1. THE induction spark, which explodes between the extremities of the induction wire in Ruhmkorff's apparatus, when the inductor current is interrupted, possesses at the same time the property of tension, more especially characteristic of the static state, and the properties of quantity characterising the dynamic state.

2. This spark may be divided into two parts, one possessing only the qualities of tension, the other those distinguishing the dynamic state.

PROCEEDINGS OF SOCIETIES.

THE SOUTH KENSINGTON MUSEUM.

Monday, January 7, 1861.

On the Importance of the Study of Chemistry, by A. W. HOFMANN, LL.D., F.R.S.

IN compliance with an invitation from the Committee of Council on Education, I will endeavour to bring under your notice some considerations on the Study of Chemistry as part in a well arranged system of mental cultivation. In an attempt of this kind, owing to the endless variety of relations which chemistry bears to other branches of knowledge, so many starting points present themselves, that it seems important to define at the very outset the aspects under which I propose to submit the subject to your attention. I may, therefore, state at once that I shall examine the question more especially in two directions, aiming, in this first Lecture, at giving you a survey of the conditions which render the study of chemistry invaluable as advancing the material well-being and the intellectual progress of mankind, and pointing out in a second Lecture, the path recognised by experience as the shortest and safest towards the perfect mastery of chemistry as a science; adding at the same time, some remarks on the chemical instruction in schools, where chemistry is taught as a branch of general education, and as a means of mental training.

The importance of the study of the natural sciences, and particularly of chemistry, becomes at once obvious when we compare our modern civilization with that of antiquity. Losing sight of the revolution brought about in the human mind by the reception of new religious views, as not immediately connected with the subject of this discourse, it

¹ Abstract of a paper in the *Annales de Chimie et de Physique*, sect. 3, vol. lxi. pp. 200—223.

cannot be denied that all the other steps of progress which modern times have made, are exclusively derived from the ardent cultivation of the study of nature.

In poetry and sculpture the ancients are still unsurpassed. What epic poem, as to originality, vigour, and freshness, can be compared to those of Homer? We admire the genius of a Shakespeare, the searching eye with which he traces human nature to her innermost recesses; we are carried away by his life-warm creations. Whether he reveal the tenderest emotions of the human breast, or roll before our view the stormy waves of passion, we unhesitatingly accord to him the palm as the first dramatic poet of modern times. Let those, however, who have read the Greek tragedians say, whether Sophocles has been surpassed by Shakespeare!—Modern sculpture ever turns to the antique as to a model of unattained perfection. In all times, from all countries, the sculptor has made his pilgrimage to Rome and Greece to study in the school of the ancients.

He who has seen the frescoes on the walls of Pompeii will bear witness to the high attainments of the painters of those ages, and, granting a great development to the art of painting in modern times, it would be difficult to say how far this is owing to our different mode of viewing the world, which has altogether changed the scope of artistic aspiration, or to the technical improvements, more especially in the colours, which are at present at the disposal of the painter. It is next to impossible to form a clear notion of the music of the ancients; but if we consider the simplicity of the instruments at their command, there can be no doubt that the advance, which modern music has made, is mainly referable to our perfected construction of their instruments, and to the invention of new instruments, for which again we are indebted to an active study of the laws of nature.

Our writers of history, too, in what do they surpass their predecessors except in the endless abundance of material which century after century has heaped up in addition to what they possessed? The writings of the ancient historians are still the first which we place in the hands of our youth, and experience has proved what an inexhaustible source of mental and moral culture successive generations have found in these works. No nation of modern times has produced more brilliant orators than England; as none possesses freer institutions, the native soil of eloquence. But have modern orators outstripped the Greek and Romans (except, perhaps, in the formidable length of their speeches)? To him who would ascertain what progress the schools of mental philosophy in later times have made, compared with those of antiquity, I recommend a plunge into the works with which a certain school deluged the literature of Germany at the commencement of this century. Again, has not modern jurisprudence its very root in the Roman law? Is not the Roman law taught at this very day in all the continental universities? and is not the civil law of the Romans still in force in many states at the present time? I may be reminded of the enormous strides which the mathematical sciences have made in modern times, but we still learn from Euclid the rudiments of geometry: and are we quite sure that the methods, which, like so much new apparatus, has been supplied to mathematical operations, would have unfolded themselves, had it not been for the wonderful development of the natural sciences, more especially of physics, which continually offered to mathematical treatment new material to be mastered, and thus in a measure involved the creation of these very methods?

I might easily pursue this parallel still farther, but I have already said enough for my purpose.¹ Modern

civilization most undoubtedly has its footing in the zealous investigation which the present age has devoted to the forces of nature, and in the wonderful results to which this investigation has led, and, day by day, is leading. The study of the forces of nature has subjected them to our will. Knowledge is power; under no conditions is this axiom truer than when applied to the knowledge of natural phenomena. Time and trouble spent on the study of nature is amply repaid, for in every well examined physical force, man educates for himself an obedient servant, ready at all times to fulfil his behests.

What giant strides in all directions have been made in consequence of the development of the various branches of physical knowledge? The power of steam has gradually become subservient to such a variety of uses that we can scarcely imagine our existence without it; and yet when we see our steamships cleaving the ocean from hemisphere to hemisphere, or the locomotive engine rushing with the speed of the storm over the country, how rarely does it occur to us that we are indebted for this rich inheritance to the patient study of the laws of heat? Were I to pursue, into its various directions, the influence which the substitution of steam power for the uncertain forces of water and wind, or for animal force, has exerted on the several arts and manufactures, I should exceed the limits of this sketch; but I am tempted to glance for a moment at the wonderful change which the introduction of steam has produced in the civilization of nations. We are still far distant from the reign of uninterrupted peace; the clang of arms no sooner ceases in one country, than the flames of war are seen raging in another, but how different in form and fashion are our wars from those of antiquity! We still wage war, but no longer for the purpose of enslaving entire nations. We need no slaves, steam engines are our slaves, our helots. The constructions of our architects, and of our engineers, are not less grand than the pyramids of ancient Egypt, whilst they are immeasurably more useful; but no longer are the inhabitants of whole provinces made to groan for years like beasts of burden, no longer is the towering pile cemented by the sweat of an enslaved people.

A harvest of results, not less unexpected, has rewarded the inquiries in electricity, a science of which antiquity had not even an anticipation. A century has not passed since Franklin made his experiments with the electric kite; a shorter time has elapsed since Galvani saw the frog's leg quiver on the balcony at Bologna, and scarcely have the inquiries been completed which place in its true light the connexion between the experiments of both observers; but what incalculable advantage has not the human race already reaped from these observations! Everyone knows that Franklin was led by the prosecution of his experiments to the invention of the lightning-conductor, and that the elaboration of Galvani's observations gave rise to the construction, in the hands of Volta, of his battery, that grand lever of our own times. A faithful handmaid, the voltaic battery multiplies for us, in durable metal, the noblest productions of artistic genius, formerly accessible only to the chosen few, and this with an accuracy of which no other mode is capable. Not satisfied with giving an exact copy of the form, it clothes the copper with a layer of silver or gold, almost imponderable, yet sufficiently close and uniform to protect the delicate surface against the tooth of time. Beaming from the lofty pharos, the electric ray warns the seaman from the treacherous rock, and guides him safely into port. As a winged messenger, lastly, annihilating time and space, the electric current bears our thoughts with the rapidity of their conception over land and under sea. Great as these acquisitions are, the list of benefits which our generation is destined to reap from the study of electricity is not yet concluded. It was by the help of the voltaic battery that Davy, at the commencement of this century, was enabled to decompose the alkalis into

¹ The subject has been very fully treated by Professor Pettenkofer in one of his popular lectures. In the same lecture the author lucidly explains the causes which, in antiquity, prevented the free development of the study of nature.

their elements. The isolation of the alkali-metals has at all times been considered one of the finest achievements of chemistry. During the half century that has since elapsed, it has afforded incalculable aid to the progress of science; but it was reserved for our time to hail on the market-place of life this glorious discovery of Davy. By the help of one of the metals discovered by Davy,—by the help of sodium,—chemists had succeeded in separating from common clay a metal, equally distinguished for its peculiar white lustre, its lightness, and its resistance to the atmosphere. The industrial production of aluminium, which is of unlimited occurrence in Nature, had been impeded hitherto by the high price of the sodium necessary in its extraction. By the unremitting efforts, however, of H. Ste. Claire Deville, the preparation of sodium has lately become so simplified and so cheapened, that the manufacture of aluminium on a large scale has been actively carried on for some time; and thus we owe to the combined inquiries of chemistry and physics, an industry, the future of which it would be difficult at this moment to calculate.

Scarcely less rich and interesting are the acquisitions which modern times have derived from the assiduous inquiries into the nature of light. To the comprehension of its laws, we are indebted for the perfection of the instruments, which, embracing as it were the infinite in both directions, have gathered the vastness of the heavens within the sphere of our perception, and expanded to our view the living world in the water drop. The phenomena of polarization, but a few years ago the exclusive property of abstract science, have taken their place among the adaptations of optics to practical life. Aided by the polarizing prism, the navigator may search the seas for the shoals threatening his destruction, and the fisherman follow the movements of his prey through depths impenetrable to his unaided eye. In the hands of a Biot, a ray of polarized light became, to the sugar manufacturer, an index of the amount of saccharine matter contained in his liquors; whilst directed by the genius of a Wheatstone, we have seen it performing the work of a chronometer. (Playfair.²)

The method of fixing the picture of the camera obscura, that marvel in the history of science, placed, as it were, on the boundary line between optics and chemistry, directs our consideration more especially to the fruits reaped from chemical inquiry.³ The study of the solar ray led the way to the contrivance of an instrument, which, imitating the structure of the human eye, presents within a narrow compass a faithful copy of the grandest scenes and of the most fragmentary miniatures of nature and art. To this picture, the fugitive offspring of a passing ray, chemical science has given permanent life. But before this discovery, which is one of the most graceful achievements of our own times, could be accomplished, what thought, what toil was necessary; what a series of researches were required, perfectly unconnected with the final result, which nobody could have anticipated! The ley of the soap-boiler had to give up its iodine: the properties of this new element, and its action on various other substances had to be carefully studied; the investigation of sulphur and its oxides had to teach us the existence and the properties of hyposulphurous acid; gun-cotton, lastly, of ephemeral celebrity whilst threatening the world with destruction, had to be tamed down to the permanent usefulness which it possesses in the form of collodion.

Photography is not the only fruit which has arisen from the union of optics and chemistry. The last few months have given evidence of the treasures which are still to be raised on the boundary domains of the two sciences.

It is well known that the spectrum of a flame, of a gas-flame, for instance, in which certain substances are volatilized, exhibits peculiar bright lines, the colour and position of which varies according to the nature of the substance. These phenomena, investigated by several observers, and more especially by Plücker, have lately been further pursued by Kirchhoff and Bunsen. Their experiments have led to truly astonishing results. Not only have these philosophers been enabled to endow science with perfectly new methods of research, in comparison with which the ordinary modes of analysis are coarse and clumsy; but the delicacy of their process has permitted them to recognise the existence of new elementary substances, which, like satellites, accompany the commoner elements. Within the last few weeks they have announced the discovery of a new metal, very similar to potassium, for which they have proposed the name of *cæsium*, from the Latin word *cæsius*, expressing the peculiar greyish blue colour of the spectrum line, the observation of which gave rise to this remarkable discovery.

I have in the first place directed your attention to the achievements which Chemistry has accomplished in conjunction with her older sister, Physics. Many of the phenomena which constitute the domain of chemistry, have been known for centuries; but it is only in modern times that this knowledge has been raised to the rank of an independent science. Little more than half a century has elapsed since chemical knowledge became chemical science, yet how manifold are the services which this new science has rendered in so short a period! It was in the latter half of the last century that the combined activity of some of the rarest minds working in various countries began to spread light over this field of knowledge. The genius and laborious researches of a Black, a Priestley, a Watt, a Cavendish in this country, of a Lavoisier in France, and a Scheele in Sweden laid the foundations of the grand edifice which has since been raised by the united labours of scientific chemists throughout the world; they were the workers out of the principles of that enlightened method of interrogating Nature, the introduction of which into the arts and manufactures has effected more in the brief space of fifty years, than the unmethodical experiments of many previous centuries had achieved. The "transmutation of metals," and the "*elixir vita*," which had eluded the ardour and the perseverance of the alchemist, became the legacy of the scientific chemist, who searched for the philosopher's stone, not in sowing the metals, not in the fragrance of flowers, or in the brilliancy of the morning dew, but in the faithful study of Nature's phenomena, and in the investigation of her laws throughout creation. Modern chemistry has not attempted to convert lead into gold, but what immense sources of individual and national wealth have been opened since the adoption of its methods has infused new life into every branch of industry! On comparing the acquirements of the present age, our comforts, our sanitary regulations, our means of communication, &c., with the state of things in the last century, it may with truth be said that human life has actually gained in duration, while its enjoyments have been increased a thousand fold. It would be both interesting and useful, did this brief sketch admit of it, to notice the effects of chemical knowledge on the various branches of industry in every department of life. It would be found, that not only every art and every trade, but that every individual, had profited by the labours of the chemist.

In taking a retrospect of the rapid progress which some of the arts and manufactures, depending upon chemistry, have made in modern times, we are at a loss to know

² The Study of Abstract Science Essential to the Progress of Industry. "Records of the School of Mines," Part I. 1852.

³ Some years ago I delivered a lecture on the Importance of the Study of Experimental Science, in a national point of view, at one of the evening meetings of the Royal College of Chemistry. Several of the illustrations given in the text are quoted from this lecture, which was never published, although a small number of copies were circulated among the Members of the College.

where to begin our survey, every branch of industry could furnish the most brilliant illustrations.

Were I to trace with you the history of sulphuric acid, from the moment when the Erfurt monastery first witnessed oil of vitriol passing over, drop by drop, from the alembic of Basilus Valentinus, up to the present time, when hundreds of thousands of tons are sent forth from the colossal lead-chambers of Great Britain alone; were I to enlarge on the influence which chemistry, by raising the production of sulphuric acid to its present degree of perfection, has exerted on the development of chemical manufactures; were I to describe the action of sulphuric acid in the preparation of the other acids, of nitric, hydrochloric, of acetic acid; its application in the manufacture of phosphorus, its employment in the improved process of candle-making, in the isolation of stearin from crude tallow, its use by the dyer for preparing his colours, by the skin-dresser for opening the pores of the skin previous to the operation of tanning; by the metallurgist in the new process of refining silver; by the agriculturist in the preparation of most efficient manures,—I should have to unfold a picture for which it would be difficult to find the appropriate frame. Nor could I omit to show you Leblanc struggling with prejudice for the adoption of his great discovery: the transformation by the agency of sulphuric acid of sea salt into carbonate of soda; or to lead you into the busy factories of Lancashire, which now, after the lapse of less than half a century, provide not only this country, but half the world, with soda. Nor would the picture be complete without the glassworks and the soap manufactories in the distance, the operations of which have been forced into unprecedented activity by the production of soda, without the bleaching yards, which owe their very existence to Leblanc's great discovery!

It would be equally instructive to trace, in brief outlines, the history of coal gas, the manufacture of which has become, during the last thirty years, of truly national importance.

The evolution in different localities of inflammable gases from the ground has been noticed from time immemorial, and the connexion of these gases with deposits of coal had been recognised as early as the middle of the seventeenth century.⁴ Years, however, elapsed before correct views of the nature of these gases were established by experiment. In 1739, coal was for the first time subjected to distillation by Dr. Clayton, Dean of Kildare, who thus succeeded in imitating artificially the formation of native gas. From this moment the subject was never lost sight of, but it was not till the end of the last century that the intellectual impulse of that memorable period overcame the difficulties opposed to the practical application of this new source of light. These obstacles we know have been at last entirely removed, gas light is now universal. Our streets, our public buildings, our private dwellings, are brilliant with its radiance. In our daily enjoyments of its benefits, we have almost ceased to be conscious of the comfort and even personal security, which we owe to this, as yet, the best, cheapest, and safest of lights.

But, to the attainment of this end, how many were the scientific problems to be solved, how often had the gas-maker to seek the assistance of the scientific chemist, and to avail himself of the results of his researches, instituted originally perhaps for very different purposes! What a difference between the gas with which Mr. Murdoch lighted up the Soho Foundry in Birmingham, and that now circulating in the arteries of our nightless towns!

The gas produced by the distillation of coal is not an uniform substance. Together with the principal illuminating constituents, light carbonated hydrogen and olefant gas, many other gases are evolved during the distillation, some of which, *e.g.* hydrogen, tend rather to

diminish the illuminating qualities of the gas, whilst others, as ammonia and sulphuretted hydrogen are injurious to health. This formed a serious obstacle to the adoption of coal-gas as a domestic light. By the aid of chemistry, the composition of coal-gas was ascertained, and the nature of its constituents, useful and deleterious, determined. Founded on the knowledge thus obtained, means were devised for the separation of the latter; the temperature was indicated, at which the purest gas comes over during the process of distillation, whilst for the improvement of gas less rich in illuminating constituents, the beautiful and economical process of naphthalisation was suggested. And, finally, simple and elegant burners were contrived for the safe and economical combustion of the product.

The extent to which the use of gas has affected the arts and manufactures in this country can only be conceived by those who are aware of its innumerable applications in the double capacity of giving heat and light. The benefits afforded by gas to our experimental chemists cannot be overrated, more especially in England, where the price of spirits of wine was at one time so exorbitant. But for the use of gas in the laboratory, the progress of chemistry in this country must have been greatly retarded.

In speaking of the general influence of the manufacture of coal gas, it is impossible to leave unnoticed the number of hands daily engaged in raising whole strata of coal, and in loading and navigating the fleets employed in conveying it, not only to the different parts of this kingdom, but to foreign countries, which consume a much larger quantity of English coal for the production of gas than is generally known. The extension of the gas enterprise produced a sensible effect on the iron works by the vast number of retorts, the stupendous gas-holders, and the endless pipes, required for generating, storing, and conveying the gas. Several other branches of trade were also forced into increased activity, and even new trades sprang up in consequence of the extended use of gas. The substances produced in the purification of gas naturally attracted the attention of the gas manufacturer; and chemistry soon pointed out valuable purposes to which they might be applied. The oily matter, which is separated as a secondary product in the distillation of coal, has been the subject of various chemical researches, amongst which must be singled out the elaborate investigations of Charles Mansfield, a young chemist of extraordinary promise, whom a cruel fate tore away to early from science and his friends. His researches showed the more volatile portions of this oil to consist chiefly of benzol, an interesting compound discovered some years previously by Faraday. The history of benzol alone and its uses would fill a little volume. Capable of undergoing in the hands of the chemist an endless number of Proteus-like transformations, this substance has not only assisted the progress of science itself, but given rise to new and important branches of industry. Let me remind you that benzol is the most convenient solvent for caoutchouc; that, as an agent for removing oil and grease, it has become an ordinary household article; that, in perfumery and confectionery, we use it as a substitute for the essential oil of bitter almonds; let me remind you, lastly, that, converted by a succession of chemical processes into aniline, it has given rise to the production of those beautiful mauves, purples, and crimsons in which the lovelier portion of mankind is now indulging to the exclusion of almost every other colour. Nor have the other products of the distillation of coal, which accompany the benzol, proved less useful; the liquid distilling at a very high temperature was found to

⁴ Mr. Shirlky's experiments on the burning well of Wigan, in the *Transactions of the Royal Society for 1659*.

⁵ Charles Blachford Mansfield died (February, 1855) a victim of his scientific zeal, in consequence of an accident which happened to him whilst he was engaged in experimenting with benzol. The memory of this young man, in whom intellectual powers of the highest order were wonderfully blended with the rarest gifts of the heart, will be cherished by all those who had the good fortune of meeting him during his short but useful career.—A. W. H.

be an efficient preservative of timber, and the pitchy residue formed the chief ingredient of an excellent substitute for the flag-stones of our pavement, while the ammoniacal liquors were found useful in improving the fertility of land. Thus, after the lapse of countless ages, was the nitrogen of petrified fern forests resuscitated in the ammoniacal liquors of the gas-works, to vegetate once more, and increase the produce of our corn-fields.

In giving you a brief outline of the influence which the study of chemistry, by the development of the manufacture of sulphuric acid and of coal gas, has exerted on the progress of civilization of the present time, I have selected as illustrations two branches of industry, which are essentially of modern growth. Let us now glance at the benefits conferred by the development of chemistry on some of the arts, which formed the legitimate pride of antiquity.

Take architecture as an example. In Rome this art had reached the highest degree of perfection. The temples, the theatres, the aqueducts of the Roman architects, half ruined as they have been handed down to us, are justly admired as glorious monuments of their taste, of the vastness of their conception—of their constructive powers. It is generally believed that the Romans have been the inventors of the ordinary mortars, some of their earlier buildings being constructed without the intervention of mortar. They certainly discovered the hydraulic mortars, which they subsequently used in all their marine buildings. The material originally used by the Romans in the preparation of hydraulic mortars, was the volcanic rock which abounds in the neighbourhood of Vesuvius, more especially near the town of Puteoli, the Puzzuoli of modern times. Ground to a fine powder and mixed with slacked lime, the dust of Puteoli, *pulvis Puteolanus*, has the remarkable property of hardening under water. When the Romans penetrated into Germany, they found the same material in the valley of the Rhine and in the volcanic districts of the Eifel. For nearly 2000 years the rocks of Puzzuoli and the trass-quarries of the Rhine were the only sources from which this valuable article could be derived. It was reserved to the science of our own age to unravel the mystery of hydraulic mortars. The attention of scientific inquirers was forcibly drawn towards this subject, when, at the close of the last century, Parker and Wyatt made the important discovery that the calcareous nodules, which are found in the clay of the London basin may be converted by simple burning into an excellent hydraulic mortar, which had, moreover, the advantage of requiring no addition of lime. Patented in 1796, this discovery led to an important branch of industry—to the manufacture of "Roman cement." The question of hydraulic mortars had entered upon a new phase by becoming the subject of scientific investigation. Geology and chemistry were appealed to, and it was not long before a satisfactory answer was obtained. Several observers have participated in this examination, but it was not until a prize, proposed by the Society of Harlem, elicited in 1828 the classical inquiry of Fuchs, and based the production of artificial hydraulic cements upon a scientific foundation. It would transgress the limits of this fugitive sketch were I to trace, step by step, the gradual development of the chemical theory of hydraulic cements; the question has been very ably discussed by Professor Pettenkofer,¹ who in our own days has materially assisted in the final elucidation of this subject. Suffice it to say that the conditions involved in the solidification of water-mortars are now known, and that every country possesses the materials for the artificial production of what was once the monopoly of the Bay of Naples and the valley of the Rhine.

(To be continued.)

NOTICES OF PATENTS.

Manufacture of Alloys of Titanium and Iron. Improvements in the Manufacture of the same Alloys. ROBERT MUSHET.

THESE specifications refer to modes of treatment for the reduction of an alloy of iron and titanium from the magnetic iron-sand, which occurs so abundantly in the neighbourhood of Taranaki, New Zealand. Mr. Mushet proposes to overcome the difficulties which stand in the way of successfully reducing this ore in the ordinary blast furnace by incorporating the titaniferous iron-sand either with pitch, tar, resin, or bitumen, previously to its introduction with the fuel (coke or charcoal) into the blast furnace or cupola. Mr. Mushet claims, also, the production of an alloy of titanium and iron from the compound ores of titanium, termed "ilmenites" by mineralogists. The advantages anticipated in the above mode of treatment will consist, firstly, in the more appropriate state of aggregation of the powdery iron ore prior to its introduction into the furnace, thus preventing the choking of the air-passages in the fire, which has always been the consequence of attempting to employ the iron-sand in its natural condition; secondly, the intimate incorporation of the finely divided ore with carbonaceous materials would be calculated to facilitate the reduction of the iron, and with it some of the titanium also present in the compound ore.

Improvements in the Manufacture of "Iron and Steel" and of "Cast Steel." ROBERT MUSHET.

THESE patents have reference to the alloying of titanium-iron with either cast steel, puddled steel, or with sundry products of the iron refinery furnace. The intention of the patentee is to produce from iron in various degrees of carbonisation superior descriptions of steel and other metals having valuable properties.

Improvements in the Construction of Furnaces for Melting Glass. W. PILKINGTON.

THIS invention consists in the adaptation of a steam boiler to the ordinary form of glass furnace. An arrangement of iron pipes is made to deliver jets of steam into the caves beneath the furnaces, where they are caused to impinge upon the lower surfaces of the grate-bars, with the twofold object of keeping them cool, and of supplying steam mingled with air to sustain the combustion of the fuel.

Improvements in the Manufacture of Paper. T. RICHARDSON, Newcastle-on-Tyne.

MR. RICHARDSON proposes to mix with the ordinary pulp for paper-making, certain phosphates, employed either alone, or conjointly with sulphites. The phosphates of lime and magnesia are particularly specified. This admixture is intended to supply mineral matter to the paper, and to be used as a substitute for gypsum and kaolin; the excessive proportion of which in some descriptions of paper has recently been pointed out in the *CHEMICAL NEWS*.² Less gritty than kaolin, the phosphate of lime would be found very similar in character to the sulphate (gypsum), and when employed in moderate amount would probably be considered a legitimate mineral ingredient in paper-making.

Improvements in Treating Bye-Products arising in the Manufacture of Soda and Potash for the Attainment of Anti-chlores, and other useful Products. J. TOWNSEND and J. WALKER, Glasgow.

THE patentees expose the waste oxy-sulphide of calcium (obtained in soda-works) to the action of atmospheric air,

¹ Popular Lecture.

² *CHEMICAL NEWS*, Vol. III. p. 329.

whereby they obtain hyposulphite and sulphite of lime. The peculiar manner of effecting this conversion constitutes their first claim. The sulphite and hyposulphite of lime resulting are used in combination with either of the sulphates of soda, potash, ammonia, or magnesia, for producing paper makers' antichlore. The insoluble portion of the oxidised waste is treated with caustic or carbonate of soda or potash to furnish sulphite of soda or potash. And, finally, the patentees claim the obtaining of sulphur and oxides of manganese; also a bye-product in the alkali manufacture.

Manufacture of Gas. W. CLARK. (A Communication.
Not proceeded with.)

IN this invention it is proposed to introduce hydrogen gas into the retorts used for the distillation of coal, and by this means so to dilute the illuminating gas as to prevent in a great measure its decomposition by contact with the red-hot inner surfaces of the retort. The hydrogen is to be stored in a separate gas-holder and passes thence through an iron pipe set across the furnace in a position where it may receive the waste heat. The hydrogen thus becomes moderately heated before it enters the gas-retort, and there combines with the highly illuminating gaseous products disengaged during the distillation of the coal.

CORRESPONDENCE.

The Sale of Poisons.

To the Editor of the CHEMICAL NEWS.

SIR,—It afforded me great pleasure to read the letter of your correspondent, W. Bennington, on the sale of poisons. It is very deplorable that a subject of so much importance as this should have been dropped, and so easily forgotten. I hope now that it is again agitated, it will not be deserted without some practical benefit having been derived from it. I quite agree with him that all future chemists should pass an examination testing their ability. In answer to the objection raised, that this would be a work of time, and that it would be no guarantee that those who are at present in business are competent to fulfil the duties required of them by the public. I reply, it need not be a work of so long a time as many suppose. Suppose that a Society were formed for the examination of chemists as to their competency to vend drugs and poisons, and a Bill passed with the following clauses:—

1st. That no person should commence the business of chemist and druggist who had not passed the above Society and obtained a certificate of his competency.

2nd. That no young man should be accepted as assistant who had not passed and obtained a certificate.

3rd. That all apprentices should pass, prior to the expiration of their term of apprenticeship.

4th. That all chemists under a certain age (suppose 35 years) should pass an examination within one year after the time of the Bill passing.

5th. That if any one should sell drugs, or call himself a chemist or druggist, without having obtained a certificate, he should pay a certain fine.

6th. That each candidate should be examined in Latin, as far as requisite for a correct translation of prescriptions, &c., and in the nature and medicinal properties and dose of each medicine, &c., &c.

7th. That each candidate should pay a certain sum for each examination.

8th. That each candidate should, if unsuccessful the first time, offer himself three other times, and that three months should expire between each examination.

9th. That each member should pay a small subscription annually to defray the expenses of the Society.

Of course, Sir, other clauses would be requisite, which I leave for some of your readers more competent than myself to add; no doubt it will be a hard struggle, and much must be done before this can be accomplished, but as unity makes strength, let us unite and give a strong pull, and we shall at last come off victorious. I doubt not that such a Society, protected and sanctioned by Government, would be the only means for lessening the many deplorable cases of poisoning which daily present themselves to our view. Many would object to such a Society; but I think no educated chemist would, only those who have never spent one day in studying the properties of those drugs which they daily sell. I am not merely surmising such to be the case. I know many that have walked out of a factory into a "Chemist's" shop, and have taken the title of "Dispensing Chemists," whilst they know nothing about the doses and properties of those drugs which they daily sell. I am personally acquainted with many cases of poisoning, resulting from the fact that such ignorant persons are allowed to sell such dangerous medicines, without knowing or giving the proper caution; but have there not been sufficient already to act as a strong and powerful stimulant to induce us to do something, and to cry to each slumberer, Rise, come to help us? I hope that others of your readers will take the subject up, and that soon something may be done to lessen this great evil. I do not wish, Sir, that anyone should suppose that I mean the beforenamed clauses to be arbitrary, far from it. I only offer them as rough timber which requires to be planed and formed into a noble edifice by some clever builders, into which we may run for protection. Having no apology to make, save the importance of the subject,—I am, &c.

R. J. RUTTER.

Uttoxeter.

[Our correspondent seems to be unaware that the Pharmaceutical Society has been labouring for years to do nearly all he suggests, and has so far obtained an Act of Parliament, which gives an exclusive right to the title "Pharmaceutical Chemist" to gentlemen passing the examination of the Society. Further power than this it is not likely, nor perhaps advisable, that the Society should obtain; but we may add that a movement is on foot which will place the question before Pharmacutists and the Medical Profession in a new light, and it is to be hoped will result in some legislation on the matter. The subject will receive full development in our pages in a short time.—Ed. C. N.]

Sulphuric Acid.

To the Editor of the CHEMICAL NEWS.

SIR,—Some years ago I required a large quantity of pure sulphuric acid, and after some considerable trouble devised a method of preparing it, which afforded me six Winchester quarts daily. Had I required ten times the amount it might easily have been prepared, and if your Subscriber will communicate with me, we may come to an arrangement on the subject.—I am, &c.

JOHN HENRY PEPPER.

Laboratory, Marylebone Institution,
17, Edward Street, Portman Square.

Chemical Notices from Foreign Sources.

TECHNICAL CHEMISTRY.

Green Colours.—Some green colours free from the objections which apply to the arsenical greens are described by Elsner (*Elsner's Chem.-Tech. Mittheil. für 1859-60. Polytech. Centralb. 1861, s. 494*). The first called "Elsner"

green," is made by adding to a solution of sulphate of copper a decoction of fustic, previously clarified by a solution of gelatine; to this mixture is then added 10 or 11 per cent. of protochloride of tin, and lastly an excess of caustic potash or soda. The precipitate is then washed and dried, whereupon it assumes a green colour with a tint of blue.

The "Tin-copper Green" is a stannate of copper, and possesses a colour which Gentele states, is not inferior to any of the greens free from arsenic. The cheapest way of making this, is to heat fifty-nine parts of tin in a Hessian crucible, with 100 parts nitrate of soda, and dissolve the mass when cold in a caustic alkali. When clear, this solution is diluted with water, and a cold solution of sulphate of copper is added. A reddish-yellow precipitate falls, which on being washed and dried, becomes a beautiful green.

Titanium Green was first prepared by Elsner in 1846. It is made in the following way:—Iserin (titaniferous iron) is fused in a Hessian crucible with twelve times its weight of sulphate of potash. When cold, the fused mass is treated with hydrochloric acid, heated to 50° C. and filtered hot; the filtrate is then evaporated until a drop placed on a glass plate solidifies. It is then allowed to cool, and when cold, a concentrated solution of sal-ammoniac is poured over the mass, which is well stirred and then filtered. The titanic acid which remains behind, is digested at 50° or 60° with dilute hydrochloric acid, and the acid solution, after the addition of some solution of prussiate of potash, quickly heated to boiling. A green precipitate falls, which must be washed with water acidulated with hydrochloric acid, and then dried under 100° C. Titanium green then forms a beautiful dark green powder.

Platinum Coating for Porcelain Crucibles.—Elsner gives the following (*Elsner's Chem. Tech. Mittheilungen*, 1859-60, s. 124) as a method by which a strong covering of metallic platinum may be given to porcelain vessels. Platinum-black is rubbed up with oil of turpentine, and the mixture is painted over the object to be coated, made red-hot. The vessel coated is then enclosed in a capsule, and well burnt in a furnace, after which it will be found perfectly covered with a strongly fixed covering of metallic platinum.

Platinum black was found to be fusible in the strongest heat of the furnace oven in the Royal Porcelain Manufactory at Berlin.

Manufacture of Pure Phosphoric Acid.—Dr. J. Neustadt publishes (*Dingler's Polytech. Journal*, Bd. clix. s. 441) an "improved method of making pure phosphoric acid on the large scale." The process involves the preparation of phosphate of soda from bone ash, the formation of phosphate of baryta from this, and the subsequent decomposition of the last by sulphuric acid. In the first place, one part of finely powdered bone ash is mixed with four parts of water and one part hydrochloric acid. After standing six hours, the clear liquid is poured off, and to it is added a boiling solution of sulphate of soda. The sulphate of lime is allowed to deposit, the liquor is poured off, and the precipitate washed and pressed. The liquor decanted, which contains acid phosphate of soda, chloride of sodium, and some sulphate of lime, is now boiled with carbonate of soda and rendered neutral, and then filtered from the precipitated carbonate of lime. We have now a neutral solution of phosphate of soda and chloride of sodium, to which is added just enough of a hot solution of chloride of barium to precipitate the phosphoric acid in the form of phosphate of baryta. Twice the weight of the bone-ash operated on, the author says, is the quantity of chloride of barium required. After well washing, the phosphate of baryta (prepared from one part of bone-ash) is mixed with one part of sulphuric acid diluted with three parts of water, and stirred occasionally for two or three

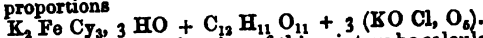
days, until no sulphuric acid is found in the liquor, which is then strong and pure phosphoric acid.

White Gunpowder.—Dr. J. J. Pohl has improved on Augendre's white gunpowder (*Sitzungs. der Akad. der Wissenschaft zu Wien*. Bd. xli. s. 634). He prescribes the following quantities:—

Ferrocyanide of potassium . . .	28 parts
Cane sugar	23 "
Chlorate of potash	49 "

100

which give a well-burning powder, and nearly approaches the proportions



The results of the combustion of this mixture he calculates to be: 2 K Cy + 3 K Cl + Fe C₂ + N + 6 CO + 6 CO₂ + 14 HO: or 100 parts by weight of the powder is resolved into

47.44 gaseous bodies
52.56 solid residue

100.00

The volumes of the gaseous bodies he estimates as follows:—

Nitrogen	1927.0 C. C.
Carbonic oxide	8942.9 "
Carbonic acid	8942.9 "
Steam	20867.6 "

40680.4 C. C.

Compared with ordinary gunpowder, considered as unity, the results are said to be:—

	Ordinary Powder.	White Powder.
Volume of gas set free.	1	2.107
Temperature of flame	1	0.641
Residue	1	0.77

It should seem, then, that the new powder has over the old the advantages of greater power, igniting at a lower temperature, and leaving less residue. The author points out several other advantages: the ease with which the white powder is manufactured, there being no necessity for granulating and glazing, and the less danger of accidents. The higher price of the materials he considers is more than compensated for by the smaller quantity required.

A political and literary contemporary who dabbles a little in science, and who describes the above composition as being *equally white and cleanly with common gunpowder*, dismally prophesies innumerable accidents if white powder should ever come into use, in consequence of the explosive nature of chlorate of potash—a danger to which he, with wonderful prescience, says, the author has never alluded. As it happens, Dr. Pohl is at pains to show that fears like those of our contemporary are groundless. Only, he states, the heaviest stroke of iron upon iron is sufficient to produce an explosion, and that it is impossible to ignite the powder by rubbing it between wood and metal, or between stones.

ANALYTICAL CHEMISTRY.

Estimation of Carbon in Organic Substances.—Dr. W. Knop proposes (*Chem. Centralblatt*, No. 2, 1861, s. 17) the following improvement on Brunner's method of estimating carbon, in the moist way. He found that when organic matter was heated with a solution of chromic acid, the carbonic acid was evolved so rapidly that it was imperfectly dried in the chloride of calcium tube, and not all taken up in the potash bulbs. He, therefore, first collects the gas in an india-rubber bag, and then passes it quietly through the chloride of calcium, and into the potash apparatus. This is done by fitting up a flask with two tubes bent at right angles, the end of one of these tubes is drawn out and closed; the end of the other is fitted to the india-rubber bag, which must be of an elliptical shape,

and open at both ends, that end adapted to the chloride of calcium tube being shut for the time by a clip. The substance to be analysed is then placed in the flask with a cold mixture of diluted sulphuric acid and bichromate of potash. Heat is now applied to the flask, whereupon carbonic acid is given off, and passes into the bag. When the decomposition is ended the clip is removed, and the gas is gently forced through the chloride of calcium tube and into the potash. The closed tube is then connected by means of an india-rubber tube to a gas-holder filled with air (freed from carbonic acid), the end of the glass is broken off within the tube, and air is passed quietly through the apparatus, carrying with it the last traces of the carbonic acid. Dr. Knop says that he has in this way analysed oxalic and tartaric acids, milk, sugar, and other bodies, and obtained the best results.

MISCELLANEOUS.

Royal Institution of Great Britain.—At the General Monthly Meeting, held on Monday, July 1, 1861, William Pole, Esq., M.A., F.R.S., Treasurer and Vice-President, in the Chair, William Beckett, Esq., Alexander John Ellis, Esq., Hardinge Giffard, Esq., and Joseph Neuberg, Esq., were elected Members of the Royal Institution. John Dobie, Esq., W. H. Stone, Esq., and D. J. Wainwright were admitted Members of the Royal Institution. The Secretary announced that the Fullerian Professorship of Physiology was vacant, and that the Managers would appoint a Professor on May 12, 1862. The presents received since the last meeting were laid on the table, and the thanks of the Members returned for the same.

The Boiling-points of Different Liquids.—The laws relating to the boiling-points of different liquids at the ordinary pressure of the atmosphere have lately been investigated by Mr. Tate, and the results of his experiments are published in the *Philosophical Magazine*. He has made experiments with solutions containing the chlorides of sodium, potassium, barium, calcium, and strontium; the nitrates of soda, potassa, lime, and ammonia; and the carbonates of soda and potassa. He has found for all these salts that the augmentation of boiling temperature may be approximately expressed in a certain power of the per-centage of the salt dissolved. The salts enumerated may be divided into four distinct groups; namely, first, the chlorides of sodium, potassium, and barium, and the carbonate of soda; second, the chlorides of calcium and strontium; third, the nitrates of soda, potassa, and ammonia; fourth, the carbonates of potassa and nitrate of lime. In each of these four groups, the augmentations of boiling temperature of the solutions have a constant ratio to one another for an equal weight of salt dissolved. He has also ascertained by experiments that for an equal weight of salts, the boiling temperatures are (approximately) in the inverse ratio of the chemical equivalents of their bases, and in the case of the nitrate of lime and the carbonate of potassa with the equivalents of the entire salts. Although the law thus indicated is not strictly true, it is sufficiently exact to warrant further inquiry, and the cases in which it is found to apply are too numerous to be referred to accidental coincidence. Future researches may extend these laws to other substances, as it is quite consistent with analogy to suppose that the chemical composition of a substance affects the boiling temperature of its solution. It will readily be acknowledged that the prosecution of these experiments may throw additional light upon the generation of steam, the economy of fuel, and kindred questions of great practical importance to engineers.

Artificial Manure from Newfoundland.—Manure, which may now be considered as an article of Newfoundland trade, is manufactured on Massacre Island, at St. Pierre's, in the following manner:—Old herring bait, at a cost of two francs per barrel, is salted with foreign salt, then boiled in a furnace, containing 250 gallons, for three hours; when cold, put into thick round mats, made for the purpose, about two feet in diameter, then placed under a screw, about twelve of them at a time, for twenty hours, by which process the water and oil are pressed out; these run by means of a shallow trough and conducting spouts, to casks outside the building, after which the oil floats, and is taken off, yielding about five per cent.; the mats containing the herring are put out, after pressing, to dry for two days: it is then taken from the mats, put into flour barrels, and closely packed by treading upon it; some is put into boxes containing 224 lbs. each; the barrel contains about the same weight. Caplin are made into manure as above, but do not produce oil. Cods' heads, also in the same manner. Cods' heads are also well dried on a beach, for five days or a week, without any salt. They are then packed into flour barrels, screwed in and sent to France, where they are ground up for manure. All these manures are said to be better than guano, and fifty per cent. higher in price. Mussels, oysters, bones, kelp—I saw specimens of all these manures, ground to a powder, said to be equally as good as any other kinds. In the manufactory, which is about sixty feet long by forty wide, I saw thirty screws fixed in frames, over troughs, say five in each frame. Forty men were here employed last year, but now less, on account of some having been lost coming out the last spring; the establishment could manufacture twenty tons per day with more room. Three hundred tons are sent to France; the island is not large enough to carry it on to the extent desired. There are no mills at present for grinding, but it is intended to import some next year, and fully complete the manufactory at St. Pierre's. If these manures pay and answer at so great a distance as France, why should they not answer the inland agriculture of Newfoundland?

ANSWERS TO CORRESPONDENTS.

"* In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

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Alimentum Salutaris.—We shall always be happy to receive letters from this correspondent, but we have an objection to reprinting letters which have already appeared in contemporary journals.

Chemicus Junius.—You are quite correct in saying that the popular process for detecting alum in bread will show its presence if alum be there. It is however open to the objection that it is equally certain to show the presence of alum when none is in the bread.

Out Flowers.—A little camphor in the water will preserve cut flowers for some days longer than if plain water were used.

THE CHEMICAL NEWS.

VOL. IV. No. 84.—July 13, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Weighing of Moist Precipitates,
by FERDINAND F. MAYER.

MR. CH. MÈNE, of Creusot (*Journ. de Pharm. et de Chimie*, Oct., 1858) gives a mode of weighing which does away to a great extent, with the tediousness and difficulties attending the drying of many precipitates, especially in volumetric analysis. He washes the precipitate thoroughly by decantation, and then introduces it carefully into a bottle, the exact weight of which, when filled with distilled water at a certain temperature, is known. Since the precipitate is heavier than water, the bottle when filled again will weigh more than without the precipitate, and the difference between the two weights furnishes the means of calculating the weight of the precipitate.

In case the precipitate settles but slowly, it may be collected on a filter, and, together with the filter, after washing, be introduced into the bottle, in which case the weight of the filter and its specific gravity, supposing any difference should exist between its own and that of water, is to be taken in account. Precipitates soluble in or affected by water may be weighed in some other liquid.

This method, of which the above are the outlines, is spoken of in the *Jahresbericht der Chemie* for 1858 in rather disparaging terms, and I consider it not more than justice to the method, if not also to Mr. Mène, to prove its correctness, the more so as I have applied the principle on a large scale as far back as 1855.

I engaged in that year in the manufacture of carbonate of lead from refuse sulphate of lead, by treating the latter, in a pulpy condition, with carbonate of soda. The sulphate of lead I used contained very varying proportions of water and soluble impurities, from which latter it had first to be freed by washing. It was then in the state of a thin pulp, and the difficulty was to find the amount of the dry sulphate of lead, as it was a matter of importance to use as little carbonate of soda, and to obtain as pure a carbonate of lead and sulphate of soda as possible. This could only be done by weighing it as whole, or in portions, but as the drying of a tubful of sulphate of lead (from 500 to 1200 lbs.) was impracticable, and sampling not less so, since the upper strata contained a much larger proportion of water than the lead at the bottom; I contrived the following method, which enabled me to leave the management of the process in the hands of a workman:—

I took a strong oaken pail, weighing 8 lbs. when empty, and caused a black mark to be burnt in horizontally around the inside of the pail, two inches below the rim, up to which mark it held 20 lbs. of water. I reasoned as follows:—The specific gravity of sulphate of lead being 6.3, the pail, if filled up to the mark,

would hold 126 lbs. of pure sulphate of lead. The specific gravity of water being 5.3 less than that of sulphate of lead, it followed that if there was 1 lb. of water in the pailful of moist sulphate, the pail would weigh 5.3 lbs. less than 126 (+8, the tare of the pail) = 120.7 (+8); if there were 2 lbs. of water present, the weight would be 115.4 (+8), and so on. This enabled me to calculate a table, giving in one column the actual weight of the pail when filled with moist sulphate, and opposite, in a second column, the amount of dry sulphate corresponding to the gross weight. The weight of dry sulphate was thus found as accurately as could be desired, although the amounts varied in practice from 30 to 105 lbs.

It is nothing but an application of the Archimedean theorem, that when a solid body is immersed in a liquid it loses a portion of its weight, equal to the weight of the fluid which it displaces, or to the weight of its own bulk of the liquid.

This, as I suppose, is precisely the principle applied by Mr. Mène. The precipitate he obtains by a certain chemical manipulation is a substance of known composition and specific gravity. Supposing it to be sulphate of lead, and the bottle, when filled with water at the normal temperature, to weigh 70 grammes = 50 grammes of water, and 20 for tare. After introducing the precipitate and filling again with water it weighed 71.06 grammes. Now, as the specific gravity of sulphate of lead is 6.3, or as the weight of a cubic measure of sulphate of lead is 6.3 times that of a cubic measure of water, and as the space of one part by weight of water is taken up by 6.2 parts by weight of sulphate of lead, it follows that the quantity of sulphate of lead in the bottle, which has taken up the space of one part by weight of water, increases the original weight of the bottle (filled with pure water) by 5.3. To find the amount of water displaced, it is only necessary to divide the overweight (1.06 grammes) by 5.3 = 0.2, which, added to the overweight 1.06 + 0.2 gives 1.26 grammes as the weight of the precipitate.

Hence the rule, which is of great convenience in volumetric analysis, that to find the weight of a moist precipitate, which is a compound of known specific gravity, weigh it in a specific gravity bottle or some other vessel of known weight, when filled with water, or any other liquid, at the normal temperature, again fill it with the water or other liquid, divide the excess of the new weight by the specific gravity of the substance, less that of the water or other liquid (that of water being = 1), and add the quotient to the overweight, which gives the weight of the precipitate.

The Editor of the *Jahresbericht* appears to have overlooked the fact that the precipitates weighed in this manner are definite compounds, the specific gravity of which is well ascertained.

The principle I have exemplified above may not be novel; but as I have never met with it, chemists, as well as manufacturers, especially of colours, will probably

also find it of interest, and certainly highly practicable and easy of execution.—*Am. Journ. Science and Arts.*

Boracic Acid in the Sea-water of the Pacific on the Coast of California, by JOHN A. VEATCH.

THE existence of boracic acid in the sea-water of our coast was brought to my notice in July, 1857. I had, in the month of January of the previous year, discovered borate of soda and other borates in solution in the water of a mineral spring in Tehama County, near the upper end of the Sacramento Valley. Prosecuting the research, I found traces of boracic acid—in the form of borates—in nearly all the mineral springs with which the State of California abounds. This was especially the case in the coast mountains. Borate of soda was so abundant in one particular locality that enormous crystals of that salt were formed at the bottom of a shallow lake, or rather marsh, one or two hundred acres in extent. The crystals were hexahedral with bevelled or replaced edges, and truncated angles; attaining the size, in some cases, of four inches in length by two in diameter, forming splendid and attractive specimens. In the same neighbourhood, a cluster of small thermal springs were observed holding free boracic acid in solution. A few hundred yards from these, a great number of hot springs, of a temperature of 212° F., rose up through the fissures of a silicious rock. These springs held a considerable quantity of borax, as well as free boracic acid. Many other localities furnished similar indications, but in a less extensive form.

In progress of the examination I found that the common salt (chloride of sodium) exposed for sale in the San Francisco market, and which, it was understood, came from certain deposits of that article on the seashore in the southern part of the State, also furnished boracic acid. I was led to attribute it to the fact of mineral springs emptying into the lagoons furnishing the salt. It was, therefore, a matter of no small surprise, when on a visit to the localities, I found no trace of acid in any of the springs in the adjacent district. This led to an examination of the sea-water, and a detection of an appreciable quantity of boracic acid therein. It was at Santa Barbara where I first detected it, and subsequently at various points from San Diego to the Straits of Fuca. It seems to be in the form of borate of soda, and perhaps of lime. The quantity diminishes toward the north. It is barely perceptible in specimens of water brought from beyond Oregon, and seems to reach its maximum near San Diego.

This peculiarity seems to extend no great distance seaward. Water taken thirty or forty miles west of San Francisco gave no trace of acid. In twelve specimens taken at various points between this port and the Sandwich Islands, furnished me by Mr. Gulick, of Honolulu, only that nearest our coast gave boracic acid. In ten specimens kindly furnished me by Dr. W. O. Ayers, taken up by Dr. J. D. B. Stillman, in a trip of one of the Pacific mail steamers from Panama to this place, no acid was discovered south of the Cortez Shoals.

I have not as yet been able to obtain specimens of water south of San Diego, nearer the shore than the usual route of the mail steamers. Neither have I been able to test the breadth of this boracic acid belt any further than the fact above stated, of no acid being found at the distance of thirty or forty miles west from the Golden Gate. I think it probable that it is confined within the submarine ridge running parallel with the

coast, the southern portion of which is indicated by certain shoals and island groups. The source of the acid is undoubtedly volcanic, and the seat of the volcanic action is most likely to exist in this submerged mountain range. It strengthens the probability of the eruptive character of the Cortez Shoal.

I hope in future to be able to make more accurate and extended examinations, unless some one more capable of doing justice to the subject should take it in hand. With this view, I solicited the attention of Dr. J. S. Newberry to these facts while he was in this city, on his way to join Lieutenant Ives' Colorado Exploring Expedition, hoping he might think it worthy of investigation during his stay on this coast.—*Proc. Cal. Acad. Nat. Sci.*

TECHNICAL CHEMISTRY.

On the Origin and Progress of the Phosphorus and Match Manufactures, by G. GORE.

IT has been wisely remarked, that Science "teaches us to be neglectful of nothing; not to despise small beginnings, for they precede, of necessity, all great things in the knowledge of science, either pure or applied." Not only should we not neglect scientific knowledge, but we should nurse as it were the smallest scientific truth; trying and watching the influence of various conditions upon it; increasing its distinctness or importance by modifying the surrounding circumstances; adding here a little and there a little, in order to magnify the extent or variety of its applications to human benefit. It is by such processes of treatment as this, slow and certain, that the little facts of the bygone alchemists, and of the more modern scientific investigators, have been extended and elaborated into the marvellous and magnificent realities of the present day.

Nearly all the great practical results of science of this period have had their origin in apparently useless and out-of-the-way observations. The expansive power of steam was known long before it was applied to any useful purpose. One observed and another observed the influence of various circumstances upon it; one invented and another contrived additional means of controlling it and directing it; one added and another introduced contrivances and pieces of accessory mechanism to increase its power; until from the simple and apparently useless fact of vapour expansion, it has grown to be the giant power of the earth, influencing the actions and habits of nearly all mankind.

The curious facts respecting the action of liquids on metals, observed by Volta and Galvani, and the action of electric currents on magnets, have, by the repeated trials and applications of subsequent discoverers and inventors, been elaborated into the present system of electro-telegraphy, which bids fair to cover the whole earth with its wires and to bring every human being into speaking distance with each other.

No reasonable and intelligent person can compare the commencements of these things with their present developments without being impressed with the insignificance of their origin and the magnificence of their practical results, nor without being convinced of the importance of abstract scientific investigation. It is not my object on the present occasion to dilate upon the relations of abstract science to manufactures and practical life, but to give some account of the origin, progress, and present condition of the phosphorus and match manufacture.

In the year 1669 phosphorus was accidentally discovered in human urine as "a dark, unctuous, daubing mass," by Brandt, a merchant and alchemist, of Hamburg, while searching for a liquid capable of transmuting silver into gold. By some means it appears that at the same time Kunckell, a German chemist, gained some knowledge of the means of obtaining this substance, and communicated it to Kraft, of Dresden, who proceeded to Hamburg and paid 200 dollars for a specimen "for him to make demonstrations to princes." Kunckell prepared some of the substance and exhibited it to the Courts of Saxony and Brandenburg, although it was not a very delicate or agreeable exhibition, "because the unctuous and daubing oyliness was not yet accurately separated from it, and, without doubt, it was very stinking, and therefore unpleasant." He also gave some to his friends, Professors in Germany, and to Mr. Linck, of Leipzig, for sale. An account of it was published in his "Laboratorium Chymicum," Hamburg and Leipzig, 1716, p. 660.

To afford some idea of the effect of Brandt's discovery upon his fellow-townsmen and upon alchemists generally I quote the following:—"When, now, such like noble motives made that good old man Brandt to broil and to sweat much more by his exorcements, one may easily guess how much at that time the whole heaven of Hamburg did hang full of fiddles as he brought forth out of it a lumen which, in the world, is counted the most perfect *agens intrinsicum*. There was then cried nothing but triumph and victory among the chymists. Those good people erected already in their thoughts so many hospitals and poor-houses that no beggar should ever more molest any man in the streets, made great legacies, and pious causes, and what not else." "Besides, the other alchemists did encourage him yet more, and desisted not to make him believe how this was that same fiery ghost of Moses that in the beginning moved upon the water, yea, his splendid shining face: the fiery pillar in the desert, that secret fire of the altar wherewith Moses burned the golden calf before he strewed it upon the fire and made it potable."

About the year 1670 the Honourable Robert Boyle, an Irishman and a philosopher of this country, brought over from Germany a youth named Ambrose Godfrey Hanckwitz to assist him in his chemical experiments, and having obtained information from Saxony of the method of extracting phosphorus, directed his assistant, Mr. Bilger, to instruct Hanckwitz to prepare some of it. Hanckwitz it appears succeeded in preparing an ounce of the substance and presented it to his master even before he received the instructions from Mr. Bilger. He was now sent by Boyle on a tour through "Holland, Italy, Naples, and Germany," to gain further information respecting "the marvellous lumen," but with little success, for he returned without meeting with "even one that could make even that poor daubing stuff as Kunckell's out of the usual microcosmical matters."

In No. 135 of the *Philosophical Transactions* of the Royal Society, dated March 26, 1677, page 87, is a paper by Boyle, entitled, "An Account of Four Sorts of Facitious Shining Substances, communicated to the publisher from very good hands, both in printed papers and in letters not printed." After speaking of three sorts, it says:—"The other is Phosphorus Fulgurans, which is a matter, made both in a liquid and dry form, and not only shineth in the dark and communicates a sudden light to such bodies as 'tis rubbed upon, but being included in a glass vessel, well closed, doth now and then fulgurate, and sometimes also raise itself as 'twere into

waves of light. Differing from the Balduinian stone, which is to be exposed to some shining body, as the day, the sun, the fire, or some lighted candle, to receive light from these, whereas this fulgurating substance carries its light alwaies with it, and when put in a dark place presently shews the same. Of which we have this further assurance given us, that a little portion of it having been kept two whole years hath not yet lost its power of shining. So that 'tis believed if a considerable big piece were prepared of it, it would serve for a perpetual, or, at least, a very long lasting light."

In No. 150 (August 10, 1683), p. 289, is also given "An Account of some Experiments made at several meetings of the Royal Society by the ingenious Frederick Slare, M.D., F.R.S., and one of the Royal Collège of Physicians, with some short applications of them to physical matters." He gives "A Parrallel betwixt Lightning and a Phosphorus," and relates a number of experiments of adding phosphorus to oil of vitriol, water, oil of turpentine, and oil of petroleum, producing combustion, &c.

And in No. 196, p. 579, dated January, 1693, is "A paper of the Honourable Robert Boyle's, deposited with the Secretaries of the Royal Society, October 14, 1680, and opened since his death, being an account of his making phosphorus, &c." "September 30, 1680. There was taken a considerable quantity of man's urine (because the liquor yields but a small proportion of the desired quintessence), and of this a good part at least had been for a pretty while digested before it was used. Then this liquor was distilled with a moderate heat, till the spirituous and saline parts were drawn off, after which the superfluous moisture also was abstracted (or evaporated away), till the remaining substance was brought to the consistence of a somewhat thick syrup, or a thin extract. This done, it was well incorporated with thrice its weight of fine white sand, and the mixture being put into a strong stone retort, to which a large receiver (in good part filled with water) was so joyn'd that the nose of the retort did almost touch the water. Then, the two vessels being carefully luted together, a naked fire was gradually administered for five or six hours, that of that was either phlegmatick or volatile might come over first. When this was done the fire was encreased, and at length for five or six hours was made as strong and intense as the furnace (which was not bad) was capable of giving (which violence of fire is a circumstance not to be omitted in the operation). By this means there came over good store of white fumes, almost like those that appear in the distillation of oyl of vitriol; and when those fumes are passed and the receiver grew clear, they were after a while succeeded by another sort, that seemed in the receiver to give a faint, blewish light, almost like that of little burning matches dipped in sulphur. And, last of all, the fire being very vehement, there passed over another substance that was judg'd more ponderous than the former, because it fell through the water to the bottom of the receiver, whence being taken out (and partly even whilst it stay'd there), it appeared by several effects and other phenomena to be such a kind of substance as we desired and expected."

Hanckwitz, the servant of Boyle, and working under his direction, at the laboratory in Southampton Street, London, succeeded, by his own skill, and perseverance, in considerably improving the process or manufacture, and produced phosphorus in larger quantities than any other person. He confirmed the opinion of his master, that it required an intense heat for its extraction; he

states, "for in truth and fact it is not a work for gentlemen and cabinet chymists, but there is required for it an operator well versed in fires, to whom its mechanick and manipulation is well known." In an advertisement he states:—"For the information of the curious, he is the only one in London who makes inflammable phosphorus which can be preserved in water. Phosphorus of Bolognian stone, flowers of phosphorus, black phosphorus, and that made with acid oil, and other varieties. All unadulterated; every description of good drugs. He sells wholesale and retail. N.B. He sells solid phosphorus, wholesale, fifty shillings an ounce, and retail, three pounds sterling the ounce."

In a "Historical Sketch of the Progress of Pharmacy in Great Britain, by Jacob Bell," it is stated, that "A house and shop, with a laboratory, were built on the Bedford estate, in the year 1706, by Ambrose Godfrey Hanckwitz, who had carried on business as a chymist in the neighbourhood since 1680. He was a maker of phosphorus and other chemicals, which were rare at that period, and which he sold in different parts of the country during his travels. His laboratory was a fashionable resort in the afternoon on certain occasions, when he performed popular experiments for the amusement of his friends. It opened with glass doors into a garden, which extended as far as the Strand, but which is now built upon. Four curious old prints of the laboratory in its former state are in the possession of its present proprietors, Messrs. Godfrey and Cooke, of Southampton Street, Covent Garden, also a portrait of Ambrose Godfrey Hanckwitz, engraved by George Vertue (1718), which he had distributed among his customers as a keepsake."

(To be continued.)

Products from the Combustion of Gunpowder under Different Pressures, by B. F. CRAIG.

THE perusal of the paper of Bunsen and Schiskoff on "The Chemical Theory of Gunpowder," whose publication about two years ago attracted much attention (*American Journal of Science and Arts* [2], xxvi. 106), led me, as it no doubt did others, to make some experiments on the residues of exploded powder.

The investigations of the above-named chemists were made on the products of gunpowder which had been burned in small quantities, and under very slight pressure, and, in concluding their paper, they advance the supposition that, under a difference of circumstances, the products may be essentially different from those which were obtained by themselves.

Such is actually the case, for the solution obtained by washing a gun with well-boiled distilled water, immediately after its discharge, and filtering while hot, will be found to contain sulphuret of potassium, with very small quantities of other substances, while Bunsen and Schiskoff found their residues to consist mainly of sulphate and carbonate of potash. This difference is what was to be expected from our knowledge of chemical laws; and it is also to be observed that the products of combustion in the gun vary somewhat according as the explosion takes place under a greater or less pressure. Thus, in firing with a shot very heavy in proportion to the charge of powder, as in firing a mortar, the washings of the piece are found to be of a bright green colour, while the ordinary washings of a gun are colourless after the suspended carbon has been allowed to settle, or has been removed by filtration.

The colour above spoken of is exactly the tint of a solution of a sesquisalt of chromium; it is not impaired by filtration of the liquid, but disappears in a few hours by exposure to the air, and immediately by the action of acids. The cause of this colour seemed to me at first obscure, but I found that by adding a very minute quantity of freshly precipitated sulphuret of iron to solution of protosulphuret of potassium, the same colour was produced, and a sensible trace of iron may always be found in gunpowder.

A greenish colour is frequently observed when sulphuret of iron is precipitated in the presence of an alkaline sulphuret, but as it disappears on standing, it has been commonly attributed to the presence of particles of sulphuret of iron held in suspension, an opinion which must be erroneous, since I have obtained a solution of iron in sulphuret of potassium of an intensely green colour, perfectly clear and transparent, and permanent for many days in a sealed tube.

Why this green compound should be formed under some circumstances, and not under others, in a mortar, and not in a cannon, is not insusceptible of explanation.

It has been shown that when powder is exploded under slight pressure the alkaline metal remains in large part as a sulphate, but under higher pressure, and corresponding temperature, it is reduced to the condition of a sulphuret. Now, by analogy, it would seem, that with the ordinary charges of a cannon or musket the iron is not reduced to the state of sulphuret, but if, as is often the case in a mortar, the projectile is fifty times the weight of the powder, or upwards, the gases are brought to a state of great tension before the shot moves, and in that state of things the carbon is completely converted into carbonic acid, and the iron, as well as the potassium, brought to the condition of sulphuret.—*American Journal of Science and Arts.*

On the Solidification of Carbonic Acid, by MM. A. LOIR and CH. DRION.

IN a paper read before the Academy, June 2, 1860, we stated that atmospheric pressure liquefies carbonic acid when its temperature is reduced to the point at which liquid ammonia evaporates *in vacuo*. By slightly modifying the conditions of the experiment, we have succeeded in solidifying carbonic acid with the aid of an apparatus as simple as those daily employed in chemical laboratories. This hitherto dangerous and costly operation may in future be easily repeated to a chemical class.

If liquid ammonia is introduced into a glass globe, and the interior of this put in communication with a good air-pump, by the intervention of a vessel containing coke impregnated with sulphuric acid, the temperature of the liquid is rapidly reduced from the first strokes of the piston. The liquid begins to solidify towards -81° ; it soon becomes a mass, and if the air-pump allows the reducing of the pressure to about a millimètre of mercury, the temperature of the solid ammonia becomes lowered some degrees more and reaches -89.5° . This suffices to determine the liquefaction of carbonic acid under atmospheric pressure. We have, in fact, proved that carbonic gas liquefies by passing a current of the dry carbonic acid gas into a small U shaped tube, immersed in ammonia; but as the temperature obtained is only a few degrees below that of saturation, we get only a small quantity liquefied. If, on the contrary, a slight elevation of pressure is employed, the experiment becomes easy, and yields in a short time notable quan-

tities of solid carbonic acid. The following is the manner of operating:—Introduce about 150 cubic centimetres of liquid ammonia into a reversed glass receiver, the sides of which are cemented to a plate with two holes. In the central opening fit a glass tube, closed internally, and reaching the bottom of the receiver, the other opening serving to place the interior of the receiver in communication with the pneumatic machine. Carbonic acid is produced by heating previously-dried bicarbonate of soda in a copper retort, the neck containing fragments of chloride of calcium. One part of this retort communicates by a leaden tube on one hand with the tube which is immersed in liquid ammonia, on the other hand with a small manometer of compressed air. The air being previously expelled from the apparatus, and the temperature of the ammonia lowered to about the point of solidification, the retort is heated, noting meanwhile carefully the pressure. The pressure is thus maintained between three and four atmospheres. Rapidly augmenting transparent crystals soon appear on the sides of the interior tube, so that in about half-an-hour all that portion of the tube which is plunged in ammonia becomes covered with a thick stratum of crystals (about 25 grammes). The experiment may then be concluded and the apparatus dismounted.

Solid carbonic acid, obtained under the above mentioned conditions, appears a colourless mass as transparent as ice. It is easily detached from the sides of the condensing-tube by means of a glass rod; it then divides into large cubic crystals, each side about three to four millimetres. Exposed to the air, these crystals slowly return to their gaseous state; they evaporate, leaving no residue. Placed on the hand, they communicate no immediate sensation of heat or cold; they are with difficulty held in the fingers, and with a slight pressure escape as if covered with an unctuous matter. If one of the crystals is kept between the thumb and forefinger, it soon produces an intolerable burning.

An experiment was performed by placing a certain quantity of solid carbonic acid in a small glass tube communicating with a receiver filled with mercury. After some time the crystals disappeared, leaving no residue, while the receiver was filled with perfectly pure carbonic gas, capable of being completely absorbed by potash. Mixed with ether, in a small porcelain crucible, these carbonic acid crystals yield a freezing mixture of a temperature of -81° .

As a conclusion to these summary indications we will add that the liquid ammonia used in our experiments was prepared by M. Bussy's process,—that is to say, by acting on ammoniacal gas in a globe surrounded with liquid sulphurous acid, the evaporation of which is expedited by an air-pump. By this method nearly two decilitres of liquid ammonia can be easily obtained in less than two hours.

We determined the temperatures here indicated by means of an alcoholic thermometer, on which we marked two fixed points,—that is to say, 0° at melting ice, and -40° at the temperature of melting mercury.—*Comptes Rendus*.

On a Black Varnish for Zinc, by M. BÖTTGER.

M. BÖTTGER describes a process for covering zinc with a chemical, adherent velvet-black varnish. Dissolve 2 parts by weight of nitrate of copper and 3 parts of crystallised chloride in 64 parts of distilled water; add 8 parts of hydrochloric acid of 1.10 density; into this

liquid plunge the zinc, previously scoured with fine sand; then wash the metal with water and dry it rapidly.

This coating constitutes a kind of metallic alloy. It is M. Böttger's opinion, that characters in relief may be executed on a sheet of zinc by using this composition, and by employing dilute nitric acid (1 to 10), as the black coating resists the acid which attacks only the unpreserved metal.—*Journal de Pharmacie et de Chimie*.

Influence of Lithia on Fructification, by M. DE SALM-HOESTMAE.

BARLEY does not develop itself in coarsely-powdered and washed rock crystal; after being watered with water containing carbonate of lime, magnesia and manganese, sulphates, phosphates, and fluoride of potassium, it sent forth a feeble shoot but did not fructify; but by the addition of a trace of nitrate of lithia (a hundredth of a milligramme!) a barley corn flowered, and yielded two perfectly ripe grains.

From this experiment the author insists that lithia is indispensable to the development of grain.—*Annal. der Physik und Chem.*, vol. cxi. p. 644.

PHARMACY, TOXICOLOGY, &c.

On Arsenic-Eating in Styria.

PROFESSOR E. SCHAFER, of Gratz, publishes (*Sitzungsber. der Akad. der Wissenschaft zu Wien*) a few more particulars respecting the arsenic eaters of Styria. The paper seems to be the same as that quoted by Dr. Roscoe (*CHEMICAL NEWS*, vol. ii. p. 261), but contains some particulars which were omitted from our report. At the new moon, it should seem, the use of arsenic is for a time suspended, and then begun again with the smallest dose, which is increased from day to day until the moon reaches the full. The dose is then gradually diminished again, and a dose of aloes is taken occasionally, until strong purging is produced.

Most arsenic eaters observe no peculiarity in their diet, but some avoid fatty food, and the greater number are given to the free use of spirituous liquors. There are some differences of statement as to the form in which the arsenic is taken. In some cases powdered white arsenic is said to be swallowed "in a warm liquid, such as coffee" (*CHEMICAL NEWS*, vol. i. p. 281); but in the case observed by Dr. Knappe, and quoted by Dr. Roscoe, the man took a piece of solid arsenic, crunched it with his teeth, and swallowed it. The urine of this man contained but a trace of the poison, and therefore it is reasonable to suppose that when arsenic is taken in a solid form, on account of its insolubility, the greater part is carried away in the feces.

In the case of a horse, experimented upon by Dr. Koch, arsenic was found in the saliva, blood, urine, and excrement, the largest proportion necessarily in the last. The experiments of Dr. Koch show that the elimination of arsenic by the urine takes place very slowly, and that the poison accumulates in the blood.

On an Improved Method of Preparing Soda Carbonas Siccata, by HARRY NAPIER DRAPER, F.C.S.L.

The directions of the three British Pharmacopœias for the preparation of dried carbonate of soda are in almost every respect alike. Crystals of carbonate of soda are

exposed to the action of heat until the whole of their water is expelled. This process has two disadvantages : 1. The carbonate of soda of commerce, which is always used in practice to the exclusion of the pure product of the *Pharmacopœias*, invariably contains sulphates and chlorides. These are, of course, found also in the dry salt. 2. The hard cake which results from the desiccation of the crystals is extremely difficult to powder, and never makes a really elegant preparation.

Both these inconveniences may be remedied by the substitution of bicarbonate of soda for the carbonate. When this salt is heated to redness it, as is well known, loses one of its two equivalents of carbonic acid and all its water, a pure mono-carbonate remaining. Bicarbonate of soda of commerce is nearly always almost pure, never, as far as my experience goes, containing more than slight traces of chlorides and sulphates. When not heated beyond redness, the resulting carbonate is not hard, but, on the contrary, is a loosely cohering perfectly white powder. The slight difference in the cost of carbonate and bicarbonate is of small moment when compared with the saving of time and trouble, and the superiority of the dried carbonate made from the latter salt.—*Dublin Medical Press*.

PHYSICAL SCIENCE.

On the Boiling-points of Liquids, by M. L. DUFOUR, of Lausanne.

It is an established fact that the temperature of boiling water, instead of being always the same, or varying only with the atmospheric pressure, differs according to the vessel in which the liquid is heated. For instance, water boils sooner in a metal than in a glass vessel, and M. F. Marcet's numerous experiments (*Bibliothèque Universelle*, vol. xxxviii. p. 381) have shown, amongst other things, that the treatment a glass vessel has undergone (as washing in sulphuric acid, &c.) causes several degrees of variation in the boiling point. Water deprived of the air dissolved in it can be re-heated considerably above 100° C. before becoming gaseous, but then it boils violently. Donny, in his interesting experiments (*Annales de Chimie, et de Physique*, third series, vol. xvi. p. 167), carefully heated water completely freed from air, to 135° before a change of condition took place. This retardation of ebullition is manifested also by other liquids, and the production of vapours by starts is a frequent sign of it in glass vessels.

In the actual state of things, boiling produced at a higher temperature than that at which the elastic force of the vapour of the liquid is equal to the external pressure is generally considered as an anomaly due to two causes,—firstly, the adhesion of the liquid to the substance of the vessel; secondly, the absence of air in solution. There are, however, some curious facts, which show that the adhesion of a solid and the absence of air in solution are inadequate to explain the retardation in boiling; but, on the contrary, contact with a solid produces an immediate and decided formation of vapours. If a few drops of water are dropped into linseed oil, heated to 105° or 110° in a porcelain capsule, they fall slowly to the bottom of the vessel. The instant they reach it vapour is formed suddenly; the slightly-diminished drop of water rebounds several millimètres from the bottom, then falls again, causing another disengagement of vapour; again it rises, and so on. Now, it must

be remarked that the drops of water, while floating in the oil, before touching the bottom of the vessel, undergo no perceptible evaporation, and that it is only on their contact with a solid that there is a sudden production of a bubble of vapour.

It will, then, be asked, What would happen were the water while being heated, kept away from the sides of the vessel, floating freely in a medium of a density equal to its own? The proper medium to be employed in these experiments ought to be able to bear temperatures above 100° without boiling, to be about the same density as water, and not to mix with water. Oils will not do, but certain essences sufficiently fulfil these conditions.

Essence of cloves, with a little oil added, forms a liquid in which water maintains its equilibrium in perfectly rounded drops, and moving about freely in the interior. Carefully heated, the mixture always passes 100° C, and sometimes goes much higher before the water boils. It is easily and constantly raised to 120° , 130° , and even higher. I have many times raised the temperature of aqueous globules of ten millimètres in diameter, to 140° and 150° . Minute globules, from one to two millimètres in diameter, have several times reached 170° , and even 175° ,—that is to say, to temperatures at which the elastic force of aqueous vapour is more than eight atmospheres. I am speaking here of water which has undergone no preparation, which has been neither distilled nor freed from air. At these high temperatures the globules do not undergo, as might be imagined, slow and continuous ebullition; they are as quiet and limpid at 150° as at 10° ; it is rather the liquid state continued much beyond the limits corresponding to the pressure under which the operation is performed.

Ebullition is produced when the globules come in contact with a solid. If, drawn by the currents, which heating inevitably occasions, they strike against the sides of the vessel or the bulb of the thermometer, there is suddenly formed a bubble of vapour; the globule, become rather smaller, is projected violently from the point at which it produced this kind of explosion, and then continues floating in the medium. The result is the same if, when the temperature is above 115° or 120° , a globule is touched with a glass or metal rod; an explosion is produced at the point of contact, a bubble of vapour is disengaged and passes through the essence, and the globule touched rebounds as though the solid point exercised a sudden repulsion over it. However, all solids are not equally efficacious in producing this change of state; glass or metal rods sometimes fail, but a slender wooden or charcoal stick always incites an immediate and tumultuous ebullition in the middle of the overheated globules. Contact with saline crystals is generally very efficacious.

It is difficult to preserve large globules from contact with the sides; hence they are the seat of the formation of vapour on one point of their surface, which has the effect generally of breaking them up into smaller globules. In my experiments, the vessels employed were small glass globes, and I have already easily obtained a globule of eighteen millimètres, at 130° C., and others of six to ten millimètres at 150° C., &c. The smallest globules most readily escape contact with the sides, and can be submitted to a higher temperature.

It may well be supposed that the preceding facts may be realised with other liquids if heated under proper conditions. This supposition is confirmed by the experiments I am now making. For instance, chloroform, heated in a concentrated solution of chloride of zinc,

easily reaches 90° and 100° . The chloroform globules float lightly in this liquid, like water globules in essence of clove. Beyond 70° contact with a solid rod produces decided and violent evaporation.

It is difficult not to connect these facts with those in which contact with a solid causes the crystallisation of super-saturated saline solutions, and also the sudden solidification of water, sulphur, &c., when brought below their ordinary temperature for solidifying. It is equally difficult to disconnect them from the facts which I have lately had the honour to bring before the Academy,—facts which showed that liquids resist solidification when immersed in a fluid medium. It appears that contact with a solid determines a change of state in liquids, and it may be that the limits of temperature assigned to the various conditions of bodies are not so absolute as they seem. Our experiments on liquids always made in vessels in contact with solid bodies may perhaps have led us wrongly to consider, as inherent properties of the liquids themselves, the phenomena resulting, at least in part, from the presence of solids. Thus, when water floats freely in a fluid it rarely freezes at 0° , and it is only changed into vapour at a point of the thermometric scale always exceeding 100° .—*Comptes Rendus*.

Researches on the Chemical Action of the Spark from Ruhmkorff's Induction Apparatus, by M. ADOLPHE PERROT.

THE following results have been obtained by the author in a lengthened examination of the chemical action of the spark of Ruhmkorff's induction apparatus. The complete paper appears in the *Annales de Chimie et de Physique*, series 3, vol. lxi. pp. 161—199.

1. Discharges by the direct current from Ruhmkorff's apparatus decompose aqueous vapour.

2. Induction sparks combine or decompose gases or vapours more energetically than sparks from an electrical machine.

3. The intervention of a condenser in the induced circuit increases the intensity of the chemical action of the spark; but as the condenser diminishes at the same time the length and number of the discharges, it should be employed only when the ordinary spark is insufficient to produce the desired combination or decomposition.

4. A portion of the aqueous vapour decomposed by the discharge of the direct induction current undergoes electrical decomposition.

5. The quantity of gas or vapour combined or decomposed by the induction spark increases with the length of this spark, the intensity of the current remaining unchanged. In other words, the longer the spark with the same direct current, the more gas or vapour it combines or decomposes.

6. With a given induction apparatus, as well as induction current, there exists a length of spark corresponding to the maximum of chemical action.

The use of currents of gas or vapour offers great advantages in all processes relating to the chemical action of sparks. In this manner he has obtained as much as three litres of gas hourly by decomposing alcohol, ether, or acetic acid vapour. A current of carbonic acid, submitted to the action of an induction spark, gives a mixture of oxide of carbon and oxygen which in less than an hour increased in volume to 200 cubic centimetres. The decomposition of ammoniacal gas is still more energetic.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, June 6, 1861.

Dr. HOFMANN, President, in the Chair.

MR. A. W. LENNOX read a paper *On Bromide of Carbon*. This body, C_2Br_4 , heretofore obtained by the action of bromine upon alcohol or ether, was prepared by the author from olefant gas by a succession of alternate treatments with bromine and alcoholic solution of potash, precisely as chloride of carbon is usually prepared. It was deposited from its alcoholic solution in beautiful crystalline plates, having an aromatic odour. It fused at 50°C ., and sublimed at a higher temperature.

Dr. DAUBENY read a paper *On the Power Ascribed to the Roots of Plants of Rejecting Poisons or other Abnormal Substances Presented to them*. The author considered that the composition of the ash of any plant varied only within certain limits when grown in very different soils; although, indeed, a small variation in the relative proportions of potash and soda, and of lime and magnesia, might be found according as these ingredients were more or less abundantly supplied. But he contended that these variations had reference, not to the mineral matters associated essentially with the different tissues of the plants, but to the mineral matter of the fluids circulating through the vegetable organism. He inferred that the spongioles of the roots had residing in them a specific power of excluding those constituents of the soil which were abnormal and therefore inimical to the plant, but that they took up those which were normal in any proportion in which they chanced to present themselves, the redundant portions, however, left after the necessities of the organism had been provided for, being again excreted, so that a vegetable will only vary in mineral composition, in proportion to the relative amounts of the several proximate principles produced under the peculiar circumstances of its cultivation. He doubted whether poisons were ever taken up by the living roots of plants, although, when the vitality of the roots was destroyed by the action of a poison, some portion of it might enter the plant by physical imbibition. His experiments were made principally with barium and strontium salts, and with white arsenic.

THE PRESIDENT: I am sure we must all feel extremely obliged to Dr. Daubeny for the very elaborate paper in which he so lucidly treats a subject equally interesting on account of its purely scientific bearings, and on account of its importance for the purposes of agriculture. I see several gentlemen here who have paid particular attention to this question, and hope they will favour the meeting with any remarks which their experiments upon this subject may suggest.

Dr. GILBERT: There are one or two points to which I should like to refer. I quite agree with Dr. Daubeny that there is not, so far as facts would show, anything like a substitution of potash by soda in the way originally supposed. We find that there is a great difference in the composition of the ashes of vegetable substances, so long as they are green and unripe; and even within certain limits—but the limits are very distinct—we find a variation according to the condition of the soil; but the nearer we approach to the maturity of any one organ, the more distinctly is a definite composition visible. And this is even observable in comparing one professedly ripened product with another. For instance, we take wheat grain ripened in good seasons and in bad seasons, and we find in the ashes of that ripened in the bad season, a very much larger proportion of soda than in those of the grain ripened in the good season; and you will find in one season a very much larger quantity of starch, and in

another a very much larger proportion of nitrogenous substances, sometimes not in a perfectly ripe condition; and according to those circumstances we do find a certain variation. But the farther the organ has advanced towards finality, and the more matured it is, the more exact and permanent is the composition. I perfectly agree with Dr. Daubeny in the explanation which he gives of the occurrence of what may be called abnormal matters, at least, ex'aneous matters in professedly ripened substances; that is to say, it is to be accounted for by the fact that the substances are not really ripened and we have a large proportion of the proximate principles. This subject Mr. Lawes and myself went into considerably some time ago,—I think in 1847,—and we have also advocated this view in a paper before this Society. With regard to the question of the condition in which plants take up their food, I am disposed to think we really do not know very much about it, but at any rate it is not, I think, essential from the facts to conclude that they do not take it up absolutely in solution, but plants undoubtedly give off water during their growth. In relation to the substances which they fix we account for their taking up of almost insoluble substances; and what is more, I have seen plants grow to perfect ripeness in perfectly clear solutions, without ever having been in connection with any solid matter; so that at any rate they can grow and mature from solutions. With regard to the question of the taking up of poisons, I have observed that some of the evidence which Dr. Daubeny has quoted is already published in regard to that subject; but there are also some results of a gentleman, who is I believe a Fellow of this Society, also published in answer to those of Dr. Davy, in which he could not get any plants to grow which he manured with arsenical solutions. In the year 1843 we manured a large quantity of wheat with arsenical solutions, and a very large proportion of the plants died; but what did grow we very carefully took care of in the field, but unfortunately it was lost by other means; otherwise it was our intention to feed poultry with the grain, and observe the effect upon them, and also examine whether we got anything in the bones of the animal.

Dr. MARCET: Perhaps, Mr. President, you will allow me an observation upon this interesting paper, which I have listened to with very great attention and interest. From the many experiments which Dr. Daubeny has stated to-night, and among them those of Lindley, may it not be that the arsenic, used in Dr. Daubeny's inquiry, mixed with soil containing manure, and this soil being in a fit state for growing plants, enters into some peculiar chemical combination which neutralizes more or less its poisonous properties, at the same time enabling its rapid elimination from the plants growing on the soil in question; and that, in fact, the poison is not rejected by the roots, but absorbed and then excreted? If we consider the great analogy between vegetable and animal life, this elective property of roots will be difficult to conceive; for in animals every substance which the stomach and intestines contain in a state of solution, is absorbed within certain limits, regulated by the laws of endosmosis. On the other hand, we are acquainted with poisons which under certain chemical conditions lose much of their power; as, for example, mercury, a substance tolerated much better under some forms than under others; indeed, the virulence of a poison appears to be diminished if exhibited in a state which will effect its rapid elimination from the body. Such is probably, for instance, the iodide of mercury. These observations have only just occurred to me, and are offered with due regard to Dr. Daubeny's interesting views.

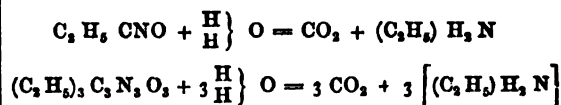
Dr. DAUBENY: I would remark, Mr. President, that Dr. Marcet's view would require that we should be able to detect poison in the plant. If we could find arsenic in the plant, then I admit the fact; but it appears to be very doubtful from my experiments whether the arsenic does

find its way into the interior of the plants. The few instances in which I have detected it, seem to be very well explained by the adherence of some small particles of the substance to the exterior; and therefore the inference I have drawn is, that it is not absorbed by the roots. The same remark applies to strontium, which is not poisonous to plants.

Dr. ODLING: A year or two ago, I was called upon to examine into the causes of the death of some cattle at Swansea. On examination, I found arsenic was the cause of death. The soil of the place on which they were fed was very rich in arsenic, and the animals had taken some of this arsenic into their bodies. The grass was also found to contain arsenic. I then sent for some turnips grown in the soil, but I could not detect any arsenic whatever in the tissues of the turnips. Then I had some of the soil which contained several grains of arsenic in the pound, and we grew some radishes in it, but we could not find any arsenic in them. The arsenic was in a very insoluble state. There seemed to be a great amount of copper slag; but I suppose the iron in a soil is quite sufficient to render any ordinary amount of arsenic insoluble in water. These results, then, seem to corroborate the results obtained by Dr. Daubeny. I read Dr. Davy's experiments at the time they appeared, and I thought them fallacious. There was no doubt about arsenic being present in this case, because there is a large quantity of pyrites in the shale. The shales which are in the coal mines in the district of Swansea, are very well adapted for growing potatoes. It is a well known fact that in Merthyr and Swansea they grow excellent potatoes.

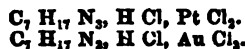
The PRESIDENT: In the interest of the Chemical Society I hope that Dr. Daubeny will favour us with the result of the new researches which he proposes to undertake upon this important subject, as soon as they are completed.

The PRESIDENT then said, that there being no other paper before the Society that evening, he would beg leave to communicate verbally to the meeting some results he had lately obtained in studying the *Decomposition of Cyanurate of Ethyl*. Cyanate and cyanurate of ethyl, as is well known, when distilled with the hydrated alkalies are decomposed, carbonic acid and ethylamine being produced, thus,—

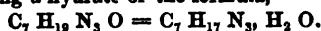


but if, instead of submitting the cyanate or cyanurate of ethyl suddenly to the action of the alkali, the two bodies be allowed to act gradually upon one another, the production of ethylamine is preceded by the formation of several intermediate bodies.

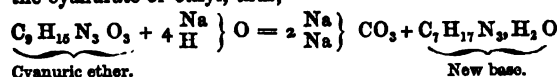
Dr. HOFMANN stated that when cyanate of ethyl is allowed to flow drop by drop into a retort containing ethylate of sodium, a powerful evolution of heat takes place, and a portion of the cyanate distils over; but the larger portion is converted into the cyanurate, which may be obtained by treating the mass left in the retort with hot water and crystallizing. If the mixture of ethylate of sodium and cyanurate of ethyl be heated, the temperature being gradually raised, a large quantity of ethylene gas is evolved, and an alkaline liquid distils over containing alcohol, an oily compound soluble in alcohol, difficultly soluble in water, ethylamine and a basic oil with a very high boiling point, which is partially decomposed by distillation. The basic oil forms with acids a series of salts, of which the iodide, the platinum- and gold-salts are the finest. The analysis of the two latter compounds gave the following formulæ:—



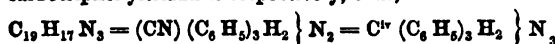
The free base, when liberated from its compounds, retains one equivalent of water, which it holds with considerable force, forming a hydrate of the formula,—



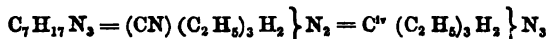
The action which takes place on distilling ethylate of sodium with cyanuric ether is very simple; the change is produced by the gradual action of the hydrate of sodium, which is formed by the decomposition of the ethylate, on the cyanurate of ethyl, thus,—



Dr. Hofmann pointed out that this base is the term in the ethyl-series corresponding to a body in the phenyl-series which he had produced some time ago by the action of tetrachloride of carbon on aniline. The phenylic compound may be referred either to two or to three molecules of ammonia, when it becomes cyantriphenylamine or carbtriphenyltriamine respectively, thus,—

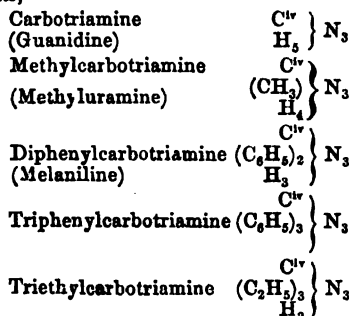


In a similar manner the ethylated body also may be referred to two or to three molecules of ammonia.



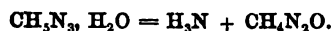
The formulæ representing these bodies as triamines, indicate in some measure the manner in which the three molecules of ammonia are linked together in these compounds. They are also in accordance with the production of the phenyl term by the action of tetrachloride of carbon on aniline.

The base methyluramine, discovered by Dessaignes among the products of the oxidation of creatine, and guanidine, the substance lately obtained by Strecker by the action of oxidizing agents on guanidine,—belong to the same group of bodies. If these substances are referred to three molecules of ammonia, they can be expressed by the formulæ,—

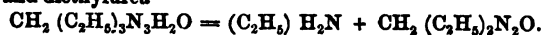


The most perfect parallelism existing between the products of decomposition of these bodies clearly indicates their great analogy to one another.

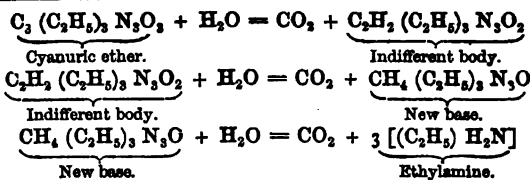
Guanidine treated with nitric acid gives ammonia and urea



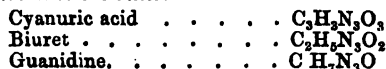
The ethylated base by distillation forms ethylamine and diethylurea



Habich and Limpricht have discovered that an indifferent body is produced when cyanurate of ethyl is treated with hydrate of potassium; this body obviously stands midway between the new base and cyanuric ether; the latter therefore passes through two stages before being transformed into ethylamine, the action consisting in the successive assimilation of water, the carbon of the cyanuric acid being gradually eliminated as carbonic anhydride.



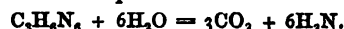
Cyanuric acid, when transformed into carbonic anhydride and ammonia, must evidently undergo a precisely analogous series of changes, intermediate bodies being produced. These bodies are known, but have not as yet been obtained from cyanuric acid. A substance described by Wiedemann as *biuret* is one of these intermediate bodies, and *guanidine* is the other.



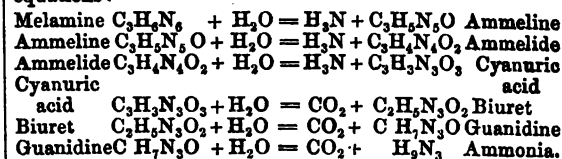
and by proper methods we may probably succeed in arresting the decomposition of cyanuric acid at the intermediate stages.

In conclusion, Dr. Hofmann pointed out that even now the cyanogen group is rich in examples of these gradual transformations. Liebig's researches on melamine have shown that this substance is converted by assimilation of water and separation of ammonia into ammeline, ammelide, and cyanuric acid, and as before stated, cyanuric acid may be transformed by assimilation of water and separation of carbonic anhydride, instead of ammonia, into biuret, guanidine, and ammonia.

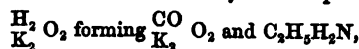
The following equation shows the ultimate product of the action of alkalis upon melamine:—



But this assimilation of water takes place molecule after molecule, not less than five intermediate compounds being produced as is illustrated by the following symmetrical equations:—

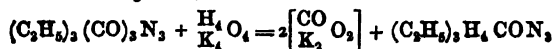


Dr. WILLIAMSON: I may, perhaps, be permitted to draw attention to one circumstance connected with the starting point of Dr. Hofmann's remarks, I mean the reaction of hydrate of potash upon cyanic ether, which I think is not unimportant in this consideration of the transformation—I mean the question of what really happens—of whether it is a replacement of carbon for hydrogen or not. I must say that, in my own opinion the reaction, if you take in the whole process, cannot be represented as being an interchange of carbon and hydrogen. I think it is of necessity carbonic oxide, or the radicle that is combined in the carbonate, that interchanges with the hydrogen. Perhaps I may be permitted to show on the board what I mean. We will take as the starting point the formula of cyanic ether, which I will write as derived from the type of ammonia, in which one hydrogen is replaced by carbonic oxide, $C_2H_5CO N$. When we bring this in contact with hydrate of potash, we must take, in order to represent the reaction, at least two atoms of hydrate of potash,



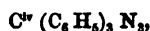
and we have then an interchange of carbon and oxygen from the one group with hydrogen from the other. I will write it out in the form of an equation so that there may be no doubt. In this reaction there can be no possible doubt that what takes place is an interchange of hydrogen and carbonic oxide. Now, a partial decompo-

sition of the same kind takes place in the formation of the remarkable body mentioned by the President, $(C_2H_5)_3H_4CON_3$. I say the same interchange takes place, and that precisely the same formula must be used, if you take into account the two products formed. I will write the formula of cyanuric ether. Twice this quantity of hydrate of potash, in four atoms, and replace the H_4 in them by two atoms of carbonic oxide, and get two atoms of carbonate of potash,

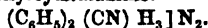


and the H_4 by taking the place of $(CO)_2$ in the cyanuric ether give rise to the body $(C_2H_5)_3COH_4N_3(C_2H_5)_3N_3CO$.

The President: I am much obliged to Dr. Williamson for his remarks, and may state at once that I have no objection whatever to his views regarding the action of the caustic alkalies upon the cyanic ethers. I have only one observation to make. The new bases which I have described contain their oxygen in the form of water, which is separated when they are converted into saline compounds. These saline compounds contain no oxygen. Moreover, several of the bases, e.g., the carbotriphenyl-triamine,



are known in the anhydrous condition. Whatever may be the reaction by which the formation of these bodies is accomplished, it is impossible to assume carbonic oxide either in the anhydrous bases or in their saline compounds. Probably the rapid sketch which I have endeavoured to delineate of these new bases did not set forth, in a sufficiently forcible manner, the anhydrous condition of their saline compounds. One more remark regarding the substitution of carbon for hydrogen. I do not wish in any way to press this view in an undue manner. I have already pointed out that these substances may be represented as cyanogen compounds. There are, in fact, some points which favour this mode of viewing them. Melaniline is formed by the action of chloride of cyanogen upon aniline, and may, therefore, with perfect propriety, be considered as diphenylcyanidamine.



There are at present no data at our disposal by which this question could be settled either way.

Dr. WILLIAMSON: My remark was only with reference to the body you described yourself, Sir, as containing oxygen. Of course, I did not allude to the product of decomposition from which oxygen is removed as water. I know no other view of the formation of the interesting body you have mentioned to us than that which represents it as effected by the interchange of carbonic oxide for hydrogen.

The President: I repeat, I have no objection to the view which you take regarding the form of the decomposition, but cannot accept your view of the constitution of the bases themselves.

Dr. ODLING: I understand you to say, Dr. Hofmann, that by simply acting upon the body which Dr. Williamson has written, the hydrochloric acid turns out the water, and that by the addition of a hydrate you would there get back the original body. They thus differ from one another, as chloride of potassium differs from hydrate of potash.

The President: They do.

NOTICES OF BOOKS.

Report on the Presence of Bisulphide of Carbon in Gas, addressed to Robert Jones, Esq., Engineer to the Commercial Gas Company. By FRED. VERSMANN, F.C.S. London: 1861.

In the early part of last year the battle of the Metro-

politan Gas Regulation Bill was being fought in a committee-room of the House of Commons. Among the points then hotly contested—so hotly, indeed, that the barristers engaged for the Companies withdrew from the field, and left the agents to fight out the case by themselves—was the quantity of sulphur which was usually present, or which might be allowed to be present in gas, without detriment to the consumers or disadvantage to the Companies.

It was about the same time that the Commercial Gas Company employed Mr. Versmann to make some determination of the amount of sulphur in their gas. That gentleman accordingly made a series of experiments at the *Works of the Company*, and the results which he obtained, together with the inferences he drew and the speculations he indulged in, are contained in the Report now under review. But before we refer more particularly to this Report, it will be as well to notice briefly what had previously been said on the subject.

Sulphur is admitted on all hands to be a most pernicious ingredient in gas. It is also conceded that it is possible to make gas which shall contain a very small amount in the form of bisulphide of carbon, while it has been found perfectly easy to remove every trace of that which existed as sulphuretted hydrogen.

The amount of sulphur in gas has been variously estimated by different experimenters. Mr. Lewis Thompson, who was considered a great authority on gas chemistry, has said that the amount of vapour of bisulphide of carbon in gas is seldom less than $\frac{1}{1000}$ th of its bulk, which will correspond to about 119 grains of sulphur in 100 cubic feet. No other chemist has ever, that we are aware of, found so large an amount as this; but it seems probable that other estimates may have been as much below as Mr. Thompson's might have been above the actual amount.

On the part of the companies it has been sometimes said that sulphur in gas is a "myth," and the damage done by it a delusion. Of the mischievous results, however, there can be no doubt. One of the first things which drew public attention to the matter was the damage done to the books in the library of the Athenæum Club. Many of our readers will, doubtless, remember the discussion on the subject, in which the most celebrated English chemists of the day were engaged. Their investigations left no doubt that the injury done to the binding of the books was owing to sulphuric acid formed by the oxidation of the sulphurous acid generated in the combustion of the gas. Other libraries have since suffered to an equal degree, as our readers may see by referring to Dr. Letheby's Report, "On the Coal-Gas Supplied to the City of London, 1854," and beyond this there is not a silversmith, hardware-dealer, or draper, whose shop is lighted with gas, but has his own tale of the damage done to his goods by sulphurous vapours. Mr. J. E. Clift, the proprietor of the Redditch Gas Works, indeed, told the Committee of the House of Commons, that it was found impossible to use gas containing sulphur in some departments of the needle factories, so injurious was it found to be. It was natural then that an Act of Parliament, professedly designed to regulate the quality of gas, should fix on some limit which the quantity of sulphur was not to exceed. Dr. Letheby in his evidence, we believe, said, that the largest quantity he had ever found was 20 grains in a 100 cubic feet, and this was the limit fixed upon. But at one of the last sittings of the Committee (July 18) Mr. Baxter on behalf of the Companies, of whom, we presume, the Commercial was one, said that gas engineers admitted that if the "twenty grains were made forty it would do." The Directors of the Commercial Company, we may suppose, had Mr. Versmann's Report in their hands at this time, and we may now quote his results:—

Series of Experiments made at the Works of the Commercial Gas Company.

Commencement of Experiment.	Cubic feet of gas burnt.	Grains of BaO ₈ O ₃ obtained.	Grains of sulphur in 100 cub. ft. of gas.	Mean of grs. of S. in 100 cub. ft. of gas.
1860, Feb. 7	13	3'08	3'23	2'99
" 28	30	6'80	3'12	
March 5	75	14'41	2'64	
" 14	118	25'42	2'96	6'35
May, 17	3'5	1'53	6'00	
" 21	2'0	0'98	6'70	

Now, we do not intend to question for one moment the perfect accuracy of the above results; but, coupling them with the fact above mentioned, they could not but excite astonishment in the mind of the most innocent person. Was it possible that the Companies knew much better than chemists how much sulphur their gas contained; or were they, when many complained of the badness of their gas, coolly asking permission to make it a great deal worse? Thinking that an independent examination might help us to a solution of these questions, and having the gas of the Commercial Company as supplied to their customers at command, we decided on making a series of determinations for ourselves. Our readers will see that our experiments were made at a distance of twelve months from those of Mr. Versmann; but as his report was written in June, 1860, and only issued this year, the inference is that the Directors wish it to be supposed that the gas Mr. Versmann examined is the gas they ordinarily supply.

The results of our experiments were as follows:—

Commencement of Experiment.	Cubic feet of gas burnt.	Grains of BaO ₈ O ₃ obtained.	Grains of sulphur in 100 cub. ft. of gas.	Mean of grs. of S. in 100 cub. ft. of gas.
1861, April 5	25	108'7	59'56	57'38
" 7	25	113'8	62'34	
" 9	25	89'5	49'03	
" 12	25	115'1	63'07	
" 22	25	109'4	59'95	
" 25	25	91'9	50'36	

Our readers will probably share the astonishment we ourselves felt at the enormous discrepancy between our own results and those of Mr. Versmann. Well might the companies have asked for forty grains! Sixty, indeed, would hardly have been too much for them! That there might, however, be no mistake about the matter, we instituted another series of experiments, the results of which we condense.

Cubic feet of gas burnt.	Grains of BaO ₈ O ₃ found.	Grains of Sulphur in 100 Cubic Feet.		
		Minimum.	Maximum.	Average.
175	793'3	39'9	59'95	49'37

For reconciling the discrepancy between the results we obtained and those obtained by the Author of this Report, various hypotheses suggest themselves; but as our readers are probably as ingenious in guessing as ourselves, we need not further occupy our space with the matter.

Passing from Mr. Versmann's results to his inferences and speculations, we feel bound to characterise them at once as an elaborate mystification. "In proceeding," he says at starting, "to the practical view of the question, i.e., whether this amount of sulphur" [i.e., the amount which he obtained] "may possibly become in any way injurious or obnoxious to persons inhaling the atmosphere of places where the gas is burnt, we must enter the dry field of calculation." Now, no one of credit—so far as we are aware—ever said that the sulphur was injurious to persons inhaling the atmosphere of places where gas is burnt. What has been said, and what has been proved over and over again, is that the sulphur products are injurious

to metal work, to the delicate colours of various textile manufactures, to the binding of books, and to many other things. On the other hand, it has been said that the healthiness of London partly depends upon the disinfecting agency of the sulphurous acid in the atmosphere, no little of which is derived from the gas.

To show, however, that the sulphurous acid cannot possibly injure people, the Author goes on to calculate from his results what proportion by weight of sulphur is contained in a given weight of gas, and how much by weight and volume of sulphurous acid is formed by the combustion of a given quantity of the gas; and then proceeds to show that the burning of seven lucifer matches would give as much sulphurous acid as the combustion of 100 cubic feet of the worst gas he examined. We may dismiss this part of the subject with the remark, that our experiments show, that to arrive at an ordinarily correct result, Mr. Versmann's sulphur calculations must be multiplied by eight at least, and his lucifer matches be made fifty-six, the combustion of which in a room would indubitably give a suffocating odour to the atmosphere.

We need not follow the Author into his carbonic acid calculations, made with the view of showing the far greater danger to animal life from the amount this body formed during the combustion of the gas than from the sulphurous acid generated from the quantity of sulphur he found. *Cela va sans dire*, as our neighbours say. We may, however, express a qualified agreement with Mr. Versmann when he says: "I am of opinion, that before any person could be inconvenienced by these quantities of sulphurous acid, he must have ceased to live, long previously, from the effect of the carbonic acid." After that event we do not imagine the sulphurous acid in this world would inconvenience him at all; but it is easy to see what Mr. Versmann means if we put *would* for *must* in the foregoing sentence.

We have said enough to show the urgent necessity for the appointment of a Gas Examiner at the East end of the Metropolis, and now return for a moment to the Commercial Company. The Directors have just issued a circular to their customers, announcing that they have made arrangements for improving the quality of their gas to the standard of the Act of Parliament, and consequently intend to raise the price. How perfectly gratuitous! The Act allows 20 grains of sulphur in 100 cubic feet; and they publish a Report to show that the maximum amount contained in their gas is 6'7 grains, and then talk of improving the quality to the standard of the Act. Whether they do improve it or not, however, our readers will shortly learn.

There remains something to be said about the methods of estimating sulphur in gas, and the removal of bisulphide of carbon, to which we intend to recur in our next number.

NOTICES OF PATENTS.

Purification of Gas. F. J. EVANS and G. F. EVANS. (Not proceeded with.)

THE patentees propose to eliminate the sulphur compounds by admitting to the foul gas a small proportion of atmospheric air, or pure oxygen, and then passing the mixed gases over one of the following materials, heated to any temperature between 200 and 400 degrees Fahrenheit:—Coke, charcoal, asbestos, chalk, and asbestos coated with spongy platinum may be used for the purpose, and the heated purifier containing one of these substances is placed intermediate between the condenser and the ordinary purifier. The sulphur compounds become oxidised, and the sulphurous acid produced may be removed either by passing the gas through the crude ammoniacal liquor, or by adopting any of the ordinary processes of purification.

The problem involved in the foregoing specification seems to have been met by the suggestion, recently patented, of applying the hydrate of lime to effect the same purpose. When heated to the temperature indicated slaked lime appears to possess the property of converting all the sulphur compounds present in the impure gas into sulphuretted hydrogen, or such products at least as are capable of being removed subsequently by the ordinary lime purifiers. This method does not, moreover, render necessary the admixture of air, a proceeding which, if possible, it is certainly desirable to avoid.

Electric Telegraph Cables or Ropes. C. S. DUNCAN.

For the purpose of insulating the conducting wire or wires they are bound in a spiral manner with strips of cane which have been well tanned with oak bark, terra Japonica, or any known tanning agent. Raw silk may be used for binding on the strips of cane. The covered wire is then thinly coated with gutta-percha, india-rubber, or marine glue, or any mixture of these, and the interstices between the coils well filled up. This system of proceeding is repeated as often as desirable for the purpose of imparting strength to the wire, or of preserving it from external injury, and the admission of water. The cable thus built up is further covered with hemp, yarn, or wood shavings, and finally bound with strands of copper or other metallic wire, wire-rope, or rope composed simply of hemp.

Preserving Electric Telegraph Cables and Wires prior to their being laid. R. A. GLASS. (Not proceeded with.)

The invention of Mr. Glass consists in packing the cables in air-tight boxes, or cases with double sides throughout, into the outer spaces of which may be introduced water, ice, or any cooling mixture. Or, further, the cables may themselves be immersed in water, and the tanks, which are now to be single cased, closed with tight covers, and the whole packed in a material being a non-conductor of heat.

Protecting and Insulating Subterranean Telegraph Wires. A. F. JALOUEREAU.

This patent refers to a process of insulating metallic wires by enclosing them within bitumenized paper tubes, or similar tubes made of any non-conducting material.

Purifying Oils and Varnishes. N. C. SZRELMLEY.

This specification refers to a mode of treatment for the clarifying of oils and varnishes by processes of filtration through certain porous media. In some cases the viscid character of the oil or varnish requires to be overcome by slightly warming prior to passing through the filter. This last recommendation might, in the case of some oils, sensibly depreciate their quality by rendering them more liable afterwards to become rancid; but this objection cannot possibly apply to varnishes which are themselves prepared at temperatures more elevated than that at which the process of filtration is intended to be conducted.

A New Mode of Obtaining an Article resembling Honey. C. STEVENS. (A Communication.)

This invention consists in the production of a saccharine compound bearing a certain resemblance to honey, by the action of diastase upon any one of the ordinary varieties of starch. Whether derived from wheat, rice, or potato, the starch is mixed with about three times its weight of water, and a very small proportion of sprouted barley or infusion of malt then added. This latter ingredient acts the part of a ferment, and, at temperatures somewhat below 200 degrees Fahrenheit, induces the conversion of

the starch into grape sugar, or more commonly into a mixture of dextrin and sugar. Such a product would undoubtedly possess a sweet flavour, but could scarcely, we think, be mistaken for ordinary honey.

An Improved Fuel. C. L. HANCOCK. (Not proceeded with.)

THE inventor suggests the binding together of small coal or coal-dust with the help of water and lime as a cement. The mass resulting from the incorporation of these materials is to be formed into cakes or blocks, and dried either in the air or by exposure to a moderate degree of artificial heat. No great pressure is required in the formation of these blocks; and, although slaked lime is preferred by the inventor, he claims also the use of "compounds containing lime" (under which term are probably comprehended hydraulic limes, mortars, cements, &c.), for the purpose of agglutinating the coal particles, and of setting them into shaped masses.

As compared with any of the ordinary kinds of "patent fuel," the advantage of the improved mode of manufacture pointed out in the foregoing specification would appear to be very problematical. One of the first considerations taken into account in the selection of a fuel for all ordinary purposes is its comparative freedom from mineral impurities; for not only do these tend to impede the combustion of the fuel, but directly contribute to increase the proportion of ash left on burning. Small coal is often objected to, on account of its containing considerably more mineral matter than larger blocks from the same sample; and when it is proposed still further to add to the proportion of an objectionable ingredient, we might safely venture to predict the result of such a suggestion.

CORRESPONDENCE.

The Monohydrate of Sulphuric Acid.
To the Editor of the CHEMICAL NEWS.

SIR,—You did me the honour to report a verbal notice which I made to the Royal Society of Edinburgh of an experiment on sulphuric acid. It was stated, as the result of four experiments, made some years since, that the true monohydrate could be obtained by keeping it at a temperature about 50° below its boiling point. I have repeated these experiments lately, but cannot obtain the same result, the acid always being at least half a per cent. too weak, and the specific gravity too low. It is better promptly to confess an error than to allow it to occupy another chemist's time in attempts at verification. I am at present engaged in endeavouring to find out how I was deceived in my experiments.—I am, &c.

LYON PLAYFAIR.

Edinburgh, July 8.

The Sale of Poisons.
To the Editor of the CHEMICAL NEWS.

SIR,—I quite agree with your Correspondent, that no person should commence the business of chemist and druggist who has not obtained a certificate of competency. This should be the case in England; but how much greater a necessity there is for such a law in Ireland it is difficult to say. I was once in a small town in the North of Ireland, some distance from home. In the course of the day I had a violent griping pain, and naturally applied for something to allay it to the druggist of the town. I found that he carried on two rather dissimilar trades, for he was a druggist and a publican. In the front room of the house medicine was dispensed, in the back room whisky. On stating the purpose of my visit, the chemico-publican

weighed out some muriate of morphia on a dirty, and, I should imagine, not very accurate pair of scales. He then went to the other part of the establishment, and brought therefrom a tumbler, from which someone had just been drinking porter. In this my medicine was mixed. I ask anyone if it is right that such men should be allowed to dispense medicines.—I am, &c. G. F. R.

Chemical Notices from Foreign Sources.

ORGANIC CHEMISTRY.

Preparation of Caramel and Assamar.—Peligot's way of making pure caramel is to heat the sugar between 215° and 220° C. as long as aqueous vapour is evolved, treat the residue with water, filter, and then precipitate the caramel from the aqueous solution by means of alcohol. It is difficult, however, to wash this precipitate free from the mother liquor, and Dr. J. J. Pohl has found it easier to make caramel in the following way (*Sitzungsber. d. Akad. d. Wissenschaft zu Wien*, Bd. xli. s. 623): He heats cane sugar contained in a metallic vessel which will hold 10 or 15 times the volume of the sugar in an oil bath to 210° or 215° C. as long as aqueous vapours escape, stirring up the mass occasionally with a spatula. The mass is afterwards finely powdered and digested with alcohol for two or three hours, and the digestion is repeated until the fluid no longer tastes bitter. The residue contains, besides the caramel, another body greatly resembling Voelckel's Caramelan. If the heat be continued sufficiently long, the alcoholic solution has an intensely bitter taste, and on evaporation yields a brown mass possessing all the properties of Assamar. This body, it seems, retains its properties in alcoholic solution, but in dilute aqueous solution is slowly changed into sugar again—grape sugar, however, not cane.

ANALYTICAL CHEMISTRY.

Estimation of Oxygen in Organic Matters.—Stromeyer describes (*Annal. der Chem. und Pharm.*, Bd. cxvii. s. 247.) a method of estimating oxygen by a combustion. He employs the oxide of copper which is made by heating gently the carbonate $2\text{CuO}, \text{CO}_2 + \text{HO}$. One part of the substance to be analysed is mixed with four parts of this oxide of copper and two parts carbonate of soda, and placed in a combustion tube with about as much granulated oxide of copper before it. (The oxide of copper is granulated by making it into a dough with one-tenth of carbonate of soda and water, and then pressing it through a sieve.) After the combustion the oxide of copper is dissolved in a solution of persulphate of iron containing free sulphuric acid. The solution is moderately diluted, and then the amount of oxygen given up by the organic matter burnt is determined by a standard solution of mineral chameleon. The author gives a great number of experiments showing the results obtained by his process. The amount of oxygen is in general too small, but the loss never exceeded a quarter of an atom.

TECHNICAL CHEMISTRY.

Herring Pickle.—Messrs. Girardin and Marchand (*Journal de Pharm.*) describe the employment of herring pickle as a manure in agriculture. After describing the process of curing herrings on the north and west coasts of France, they thus describe the liquid in question:—

Herring pickle is a reddish liquid, rendered turbid by various organic matters, as blood, oil, &c., but when filtered it has a deep amber colour. Our authors have, during the past five years, made several analyses of the crude pickle as used for manuring land, and having a density varying from 20° to 25° Beaumé. The following is the mean of several analyses of a litre (1000 gramme measures) of pickle:—

Chloride of sodium	255.11 grammes.
Sulphate of soda	5.73 "
Phosphate of lime	0.98 "
Ammonio-magnesian phosphate, traces.	
Phosphate of ammonia	1.92 "
Phosphate of propylamine	3.53 "
Lactate of ammonia	5.76 "
Lactate of propylamine	10.79 "
Albumen	1.90 "
Soluble organic matter	15.10 "
Insoluble organic matter.	17.36 "

318.18

Propylamine ($\text{C}_3\text{H}_7\text{N}$), or its isomer trimethylamine, exists normally in herring pickle. Its presence was first stated by M. Wertheim, and we have confirmed his researches. In an experiment on a larger scale we find that 100 parts of the dry matter obtained by neutralising with chlorohydric acid, the volatile alkalies which are disengaged from it under the influence of potassa or lime consist of

Chlorohydrate of ammonia	30.23 parts.
Chlorohydrate of propylamine	69.77 "

We find the aqueous solution of propylamine to possess the following properties:—It is very alkaline, and exhales a strong ammoniacal odour, recalling that of herring pickle. It precipitates the salts of alumina, but an excess dissolves the precipitate. It yields with the salts of copper a liquid of sky-blue colour. Neutralized with chlorohydric acid and properly evaporated, it yields very deliquescent crystals, soluble in absolute alcohol when they are perfectly dry. Chlorohydrate of propylamine combines with chloride of platinum and forms a double salt, which can be isolated in transparent octahedral crystals of a beautiful orange red colour, and preserving a persistent odour of herring pickle. Finally, sulphate of propylamine and sulphate of alumina unite to form an alum like ammoniacal alum, but which is deliquescent.

In the recent pickle, the authors found creatin, inositol, and a body analogous to glucose, whilst in the old and fermented pickle butyric acid was a constituent.

MISCELLANEOUS.

Chemistry of the Comet.—Since the article in the last Number was written, I have been able to get a somewhat more satisfactory observation of the spectrum of the light emitted from this body. The light was concentrated by passing through a compound lens, of large aperture and short focus, before receiving it on to the slit of the spectroscope. A coloured spectrum was distinctly though faintly visible, extending from the red to the blue, the latter colour, however, being very faint, and no violet being distinguishable. The general appearance of the spectrum was similar to an excessively weak solar spectrum. The lines D, E, δ , and, I fancy, F, were visible; in addition to which there was a suspicion of a faint band in the upper part of the green, but whether this was due to lines or other cause I am unable to say. No bright lines could be seen. The light is, therefore, principally of solar origin; the proper light of the comet, if it have any, being very faint in comparison. The above observations refer only to the light from the nucleus and that part of the coma immediately surrounding it.—W. CROOKES.

Gold-Producing Machinery.—A Return recently laid before the Parliament at Melbourne, by the Minister of Mines, shows the rapid progress made to the 31st of December last, in applying steam and machinery to the production of gold in that colony. It shows that on

December 31, 1860, there were 107,572 adult miners—viz., 60,874 Europeans and 28,100 Chinese engaged in alluvial workings, and 18,570 Europeans and 28 Chinese engaged in quartz-mining. The number of steam-engines employed in alluvial workings for winding, pumping, puddling, &c., was 294, amounting to 4137 horse-power. Besides steam-engines there were 3957 horse-puddling machines, 354 horse whims, 128 water-wheels, 56 horse-pumps. In addition to these there were engaged in quartz-mining and crushing 420 steam-engines, equal to 6696 horse-power, 158 whims, 6 water-wheels, 26 whips, and 40 horse-crushing machines. The approximate value of all this mining plant is set down at 1,259,660*l.* and there are in the principal towns in the mining districts manufacturing of engines and machinery that compete successfully with those imported from England. Great as is the progress shown by these figures in giving a character of permanency to the supply of gold, it is now probable that the colony is about to enter on a still greater development of this source of wealth, the alluvial diggers having hitherto been kept idle for a large portion of the year by the want of water. Government are forming large reservoirs for collecting and storing rain on the various diggings, and quartz-mining is likely to receive an immense impetus from the many experiments being made and patents claimed for improved methods of extracting the finer particles of gold from the *débris*, or tailings of quartz that have passed through the ordinary processes of crushing, washing, and amalgamating. Among those thus employed is Mr. Porter, of Italian Gully, near Ballarat, who being engaged in quartz-crushing, announces that he has discovered that by placing 76*lb.* of quicksilver in the bottom of a retort, and then putting into it 23*lb.* of tailings, and placing the retort on the furnace until the quicksilver is vaporized, it is by this means brought into contact with all, even the most minute particles, while the loss of quicksilver has been only 6*oz.* out of the 76*lb.* placed in the retort. The result alleged to have been obtained in these comparatively small experiments is, it is remarked, "so great that, if the same can be obtained on tons as is announced on cwt., it will be difficult to give an approximate estimate of the extent to which the production of gold will be carried in Victoria; for the great bulk of the colony, from Ballarat to Twofold Bay, and from the banks of the Murray and Owens Rivers to the parallel of Melbourne, is one great goldfield of more or less richness." The immediate consequence at present is that quicksilver is all bought up at 2*s.* 1*d.* to 2*s.* 6*d.*, and is now held for 3*s.* to 3*s.* 6*d.*

Hot Water as an Agent in Warfare.—Some years ago, the employment of hot water and steam as a means of defence made incidentally and without special appliances, was signally effective. A small British war steamer, cruising in the China seas, was attacked by a whole fleet of the pirates who infest those waters in such great numbers, and, notwithstanding the best defence that could be made with guns and small arms, they were commencing to board in swarms, when the engineer, without orders, brought out a hose from the boiler to the deck, and by a little well directed squirting, in a very few seconds drove them all overboard into their boats or into the sea, and many who escaped scalding were drowned.

Minerals in Siam.—Mr. H. J. Moyle, of Bake, in Cornwall, who has been travelling in South Africa, was attracted to Siam by its novelty and the facility of exploration disclosed by the recent treaty. Having made his way to Nophburi, about sixty miles in a direct line from Bangkok, he found the country so rich in copper and iron ores (of the former of which the natives made no use, being ignorant of their commercial value) that he returned to Bangkok, and obtained, under a deed from the Siamese Government, permission to work the lodes of that district, on payment of a royalty of ten per cent. of the ores extracted. It must, however, be remembered that the Siamese

Government is under no treaty obligation to permit foreigners to work the mines of Siam. The country along the banks of the River Chan Phya (Meinam), as far as Nophburi and its environs, lat. 15 deg. 25 min., is covered with alluvial deposits, or a mixture of these and diluvium. The hilly and mountainous districts, extending north and south in this latitude, and running many hundreds of miles in both directions, contain many valuable and rich mineral productions, though the natives do not avail themselves of them by working mines. In these hills I detected several rich and valuable veins of copper ore, principally in grauwacke; I considered them of so much value and importance, that I applied for and obtained permission from the Siamese Government to work them for five years, on payment of a royalty of one-tenth of the ores to the Crown of Siam. Lead and silver ores I also discovered, as well as enormous beds of magnetic iron ore and specular iron ore. The country to the north is carboniferous limestone with positive indications of the existence of coal. The rivers, creeks, and canals, with which this country abounds, extending in many places to the base of the hills, afford great facilities for transport, &c. I am impressed with the opinion that, should a fair portion of encouragement be given, the country of Siam will rival, if not surpass, any country of the East in the richness of its mineral productions.

Telegraphic Extension.—Steps are being taken for carrying out the new system of telegraphic extension in the metropolis and provinces on the principle of a fixed annual rental to public and private establishments, so as to furnish them with an exclusive wire for their own use and private means, hitherto unattainable by the ordinary telegraphs, for the receipt and transmission of intelligence. The wires will be combined into ropes 230 yards in length, containing 40, 80, or 100 wires, according to the requirements of the district; and in connecting different districts a system of triangulation is to be adopted, dividing the environs and towns into a series of triangles, as nearly equilateral as possible, with sides about a mile in length. The supporting posts will be fixed at intervals of 200 yards on the tops of the houses, and it is anticipated that the net-work of wires will, when extended over the metropolis, act as a "lightning" conductor, and remove all danger to be apprehended from lightning.—*Mechanics' Magazine.*

ANSWERS TO CORRESPONDENTS.

* In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

* All Editorial Communications are to be addressed to the Editor, and Advertisements and Business Communications to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

Carbon.—The process is not yet published: we will give it as soon as possible.

A Constant Reader will find his query answered in the leader of our last Number.

W. P.—We will make inquiries and communicate the result in a subsequent Number.

J. Barclay.—The subject is now under consideration. We hope shortly to be able to give a series of such articles.

Binitronaphthalin.—**W. E. R.**—This compound is prepared by acting on naphthalin with strong nitric acid, at a slightly elevated temperature.

W. Gilkes.—We are unable to explain "why the blood does not coagulate in the body of a person killed by lightning." Is it certain that such is the case?

J. J.—The water thrown on to the recent great fire could not have been decomposed by the heat alone. Water requires a temperature approaching that of melting platinum to decompose it, and in our observations on this fire we could not see any parts, even where it was raging most fiercely, where the temperature was as high as a white heat. In only a few instances it reached a yellow heat.

THE CHEMICAL NEWS.

VOL. IV. No. 85.—July 20, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Analysis of Mineral Waters, and on the Arrangement of the Results, by R. HAINES, M.B., Professor of Materia Medica, Grant Medical College, Bombay.

PERHAPS I may be allowed to make a few general observations on the analysis of mineral waters, the popular notions regarding which I have found to be very undefined, if not incorrect. The resources of chemistry are sometimes overrated and sometimes underrated, and in both cases the effect is to discredit its professors,—in the one because the results fall short of expectation; in the other because facts which can really be proved beyond dispute may seem to be beyond the power of the chemist to demonstrate, and are hence viewed with suspicion as being hypothetical, when they are not so.

In the analysis of by far the greater number of compound substances, certain preliminary operations, partly mechanical and partly chemical, are requisite, in order to bring the materials into solution, in which state only can the several acids, bases, metals, and so on, be removed from their combinations by the re-agents employed. But in the case of water, everything is already in solution, and it might be supposed therefore that the analysis must be a matter of comparative simplicity. Such, however, is not the fact; on the contrary, the very solubility of all the constituents deprives the analyst of one of the most ready and accurate means of separating them from each other. But there is another reason why the analysis is of a complex description, viz., the great variety of acids and bases simultaneously present. Each of the bases is, of course, capable of combining with any one of the acids; and as most waters are neutral to test paper, it follows that all, of both acids and bases, must be in some state of combination. The ultimate object of the inquiry is to discover what compounds—what salts, in fact—are present in the water; and we are led to endeavour to separate them directly from each other. Now, the ordinary method of separating salts which may be in solution together is by evaporation and crystallisation; by taking advantage of the various solubility of different salts at certain temperatures, some are made to crystallise out from the hot solution, some as it cools, and others by gradual and spontaneous evaporation; and in this way, by a continuation of the process, the salts are finally obtained distinct. Such a process is applied, on a manufacturing scale, in the treatment of sea and brine spring-waters, &c.

But this method, besides being inconveniently operose and tedious, is far too rough and uncertain in its results to deserve the name of analysis. A modification, however, of this method was adopted in the earlier attempts at analysis, and promised to lead to more correct results

with less loss of time. The water was first of all evaporated to dryness, and the saline mass weighed. The constituent salts and other ingredients were then separated, one after the other, by the use of various solvents, which had the property each of dissolving some one or two only of these constituents. Thus alcohol dissolved out the alkaline and earthy chlorides; then cold water, or a solution of sulphate of lime, dissolved sulphates of soda, potassa, and magnesia; then weak acid dissolved carbonates of lime and magnesia, hot nitric or muriatic acid dissolved sulphate of lime and oxide of iron, and silica remained to the last undissolved. The bases in each little group could then be separated without difficulty by the usual means. A method of this kind will be found described in some of the earlier editions of Brande's Chemistry, and in similar works.

This method is faulty for two reasons. In the first place, the solvents used are not altogether without action upon the constituents, which they are presumed to leave behind, so that the several solutions are found to contain small quantities of the latter. Accuracy, therefore, is impossible. The other reason is not so self-evident, but it is even more weighty. It is, that there is no certainty that the salts thus obtained really existed in the water. The acids and bases contained in these salts were of course there; but that they were combined each to each, in the manner represented by the salts obtained by evaporation, there is no evidence whatever to show. If we take the same acids and bases in a free state, cause them severally to combine in various ways to neutral salts, and then mix the solutions of these salts together, we shall, on evaporating the solution, always obtain a saline mass of similar composition, however various may have been the forms of combination into which we had at first thrown the acids and bases. Thus, let A, B, C, be three acids, and α , β , γ , three bases; suppose that on evaporating a solution which contains them in certain proportions the following salts have been obtained α A, β B, and γ C. It is clear that this is not the only way in which these constituents may be combined. No less than five other combinations are possible, thus:—

I.	II.	III.	IV.	V.	VI.
α A	α A	α B	α B	α C	α C
β B	β C	β A	β C	β A	β B
γ C	γ B	γ C	γ A	γ B	γ A

And if the three salts formed by any one of those groups be separately dissolved, the solutions mixed, and the liquid evaporated, the residue will in all cases contain only the three salts first mentioned, viz., α A, β B, and γ C. These salts are deposited in succession as the evaporation goes on, and it will be found on investigation that those which are first deposited are invariably the least soluble of all (in the case supposed,

¹ It will be observed that in this table each binary compound occurs twice. The total number of binary compounds possible is consequently nine.

the nine) forms of combination which can be obtained from the acids and bases.

There is thus strong presumptive evidence that the arrangement of acids and bases which prevail in the dry salts is, so to speak, accidental, and that, whatever may be the combinations in the solutions, the salts may be, in fact, only formed at the instant of their deposition, under the influence of cohesive force, which is so well known to modify the mere chemical elective affinity. The phenomenon is, in fact, identical with the ordinary one of precipitation, and depends on the same causes.

The inquiry leads us back to doctrines which lie at the very threshold of chemical science—those of affinity and combination. In the last century, when chemistry was but in its infancy, Berthollet, a most accurate observer, came to the conclusion that union and decomposition were not wholly determined by affinity, but that the relative quantities of the several bodies in presence of each other influenced the nature of the compounds which they might form, thus controverting the older views of Bergmann. But the promulgation by Dalton, in the early part of the present century, of the Atomic theory, and by Berzelius of his electrochemical theory, tended entirely to discredit the doctrines of Berthollet and his school. More recently multiplied research has turned the current slowly, but decidedly, in the opposite direction. The portion of Berthollet's theory which held that the relative proportions of the constituents in a compound might vary, has, of course, been permanently exploded. Compounds when formed must have a certain never-varying composition; but the question of the sufficiency of mere elective affinity to determine what compounds shall, in certain cases, be produced, has met with a different fate. Every day's experience brings proofs that, however true the electrochemical theory may be in a general way, there are a host of phenomena for which it fails by itself to account; temperature, state of aggregation, degree of solubility, and, what we are now chiefly concerned in, quantity—all modify the results in a way different from that which the mere relations of affinity would lead us to anticipate. Still, however, the question before us remains unanswered—What is the actual state of combination of acids and bases, so long as the solution is too dilute to afford a crystalline precipitate under the influence of cohesion, which, at a certain point, subverts the chemical affinities? It is evident that the ordinary methods of chemical inquiry are at fault, and a decision, if arrived at at all, must be founded on other considerations. The following quotation from the last edition of Turner's Chemistry shows one kind of argument employed. After speaking of a supposed mineral water containing sulphuric and hydrochloric acids, and lime and soda, in which case, of course, sulphate of lime and chloride of sodium will be obtained on evaporation, he continues:—"The late Dr. Murray, who treated this question with much sagacity, observes that some mineral waters, which contain the four principles above mentioned, possess higher medicinal virtues than can be ascribed to the presence of sulphate of lime. He advances the opinion that alkaline bases are united in mineral waters with those acids with which they form the most soluble compounds; and that the insoluble salts, obtained by evaporation, are merely products (as distinguished from educts). He therefore proposes to arrange the substances, determined by analysis, according to this supposition. To this practice there is no objection; but it is probable that each acid is rather distributed between several bases than combined exclusively with either."

I may allude, in passing, to a curious observation which has been made by Dr. Struve, the proprietor of an extensive establishment at Brighton, devoted to the manufacture of artificial or imitation mineral waters. He finds that in mixing the saline solutions with water, not merely the proportions of the ingredients, but also a certain order of mixing them, must be observed, otherwise the water will not have in all cases the same taste—a proof, if the observation be correct, that so trivial a circumstance as this may determine the nature of the decomposition.*

During the last year or two, an able English chemist, Dr. Gladstone, has occupied himself with this subject, availing himself, for the purpose, of the variations in the tint of coloured saline solutions when mixed with other salts, or diluted with water; and his results, though not absolutely conclusive, tend, I believe, to establish the truth of the opinion above advanced by Dr. Turner, that the acids and bases in a mixed solution are distributed in a more or less complex manner among each other, instead of being paired off two and two, under the operation of simple affinity.[†] Indeed, in the case I previously put, of three acids and three bases, it is quite possible that every one of the nine possible forms of combination may exist, in various proportions, at one and the same time in the same liquid. It is clear, therefore, that the ordinary mode of exhibiting the constitution of mineral waters, by arranging all the acids and bases as combined to such and such salts, is unscientific; it is conventionally adopted for the sake of distinctness, and because the public, for whose benefit these analyses are generally made, require something easily intelligible. The misfortune is, that as every one is left pretty much to follow his own method of grouping, different experimenters, who may have each made an equally perfect analysis of the same water, may exhibit their results in so very different a form, that no one would be able to divine, from mere inspection, that the water analysed had been even similar, still less identical in the several cases.

To chemists themselves the circumstance does not much signify, as they are able, by a few minutes' calculation, to re-arrange the constituents in any way most agreeable to their views; but the public are liable to be misled by it, and to imagine that all such cases of apparent discrepancy must arise from incorrectness or want of skill on the part of some or all the operators.

But the best way by far to avoid misconceptions of this kind, is to give, first of all, the positive results of the analysis, the actual quantities of each acid, base, or other constituent that have been obtained, and then, in a separate table, to arrange them according to such system as may be preferred. The figures in the first table can then be compared directly with those in another, which gives the analysis either of the same or of any other water. It is also to be regretted that no uniform standard of quantity of the water to which to refer the ingredients has been adopted. The standards used are not even of the same nature, some being of weight, some of measure. The former is the most scientific, and was used by Berzelius and many of the Continental chemists, the quantity of water assumed being usually either 10,000 or 1000 parts. In Germany

* See "Miller's Elements of Chemistry," 1856, part II. p. 1093.

† A summary of the latest experiments on this subject by Dr. Gladstone, and by Malaguti, will be found in the second volume of Dr. Graham's Chemistry; but by far the most complete and exhaustive view of the whole subject is given in the first volume of Gmelin's "Handbuch der Chemie," a translation of the latest edition of which is now under publication by the Cavendish Society.

the Prussian Civil Pound of 7680 grains, equal to 16½ oz. avoirdupois, is made use of. But a standard of measure is now most commonly used on the Continent; this is generally the litre of about 35 fluid-ounces. In England the variety is most perplexing; in the older analyses the wine pint is used, containing 16 fluid-ounces of 455·6 grain measures each; in the later analyses the imperial gallon of 70,000 grain measures is preferred, but sometimes the imperial pint of 20 fluid-ounces of 437·5 grain measures each, or 8750 grain measures in all, is used, and occasionally a bulk corresponding to 10,000 grain measures of water, or one-seventh of a gallon; the last will not, in the majority of cases, differ by more than a very few thousandths parts from 10,000 grains weight.

TECHNICAL CHEMISTRY.

On the Origin and Progress of the Phosphorus and Match Manufactures, by G. GORE.

(Continued from page 18.)

WHEN we consider that 1000 parts of urine contain scarcely one part of phosphorus, and of this probably only a portion was obtainable by the processes first in use, we shall not be surprised at the statement of Boyle that "the liquor yields but a small proportion of the desired quintessence," or at the price of 50s. an ounce charged by Hanckwitz for his product. Even an improved process, purchased and published by the French Government in 1737, yielded, under the direction of Hellot, Geoffroi, Dufay, and Duhamel only four ounces of phosphorus for every five hogsheads of the liquid.

After the death of Hanckwitz, in 1741, some experiments were made by Margraaf, Fourcroy, Vauquelin, and others; but no great improvement in the production of phosphorus appears to have been effected until 1769, when Gahn made the important discovery of phosphoric acid in bones. Margraaf had already demonstrated the individuality of this acid as early as the year 1740, and it only now remained to devise a process for extracting it. Scheele immediately did this, and various eminent chemists quickly succeeded in making various improvements in the method of working, and bequeathed unto us substantially the same process of manufacture as that now in operation.

The present sources of phosphorus are the bones of buffaloes and other animals, slaughtered in the great hunting-grounds of South America (the Pampas bordering the La Plata), where bones are used as fuel; exhausted "bone-black," or "animal charcoal" of sugar refineries; calcareous deposits of phosphate of lime, or "mineral guano" from the coast of Yucatan; and similar deposits from the Island of Sombremos; but the chief of these sources is the burnt bones from Monte Video, Rio Janeiro, Rio Grande, &c., and the animal charcoal of the sugar refineries. These various substances contain from 60 to 90 per cent. of their weight of phosphate of lime, or from 12 to 18 per cent. of phosphorus.

The phosphorus-yielding material, of whatever kind, having been suitably ground, a weighed quantity of the powder is placed in a large circular tub, lined with lead, with a mixture of oil of vitriol and water, and stirred by means of a revolving wooden stirrer, driven by a steam-engine, steam being admitted by a pipe into the mixture to facilitate the action of the sulphuric acid upon the powder.

The changes which bone-ash or other varieties of

phosphate of lime undergo in the above operation are these: the oil of vitriol or sulphuric acid gradually unites with the lime and forms sulphate of lime, and sets the phosphoric acid free; so that after the process there remains a semi-fluid mixture of a solid substance, sulphate of lime (gypsum or plaster of Paris), with a fluid body, phosphoric acid, separable by filtration.

The creamy mixture is now transferred, by means of ladles, to a filter or drainer, from which, with the aid of occasional stirring, the phosphoric acid filters into a vessel beneath. Water is added to the drained contents of the filter until the drainings cease to taste acid. The sulphate of lime or gypsum is then removed, in its damp state, to a furnace or other source of heat and dried, and constitutes a residuary product suitable for the manufacture of artificial manures.

The dilute filtered solution of phosphoric acid, containing some phosphate of lime and a small quantity of sulphate of lime dissolved in it, is transferred to leaden vessels and slowly evaporated over a gentle fire; the small quantity of gypsum then deposits itself upon the bottoms of these vessels and is removed by scraping. The liquid is deprived of as much more of its water as possible by further evaporation in similar vessels, and on cooling acquires the consistence of cocoa-nut butter.

The butter-like paste is then well mixed with a due proportion of powdered charcoal, and the mixture heated in furnaces of brick or iron until it is brought into as dry a powder as can be attained. The powder consists of charcoal, phosphoric acid (a compound of phosphorus and oxygen), a little phosphate of lime, and a little water, which is chemically combined with the phosphoric acid, and not capable of removal by the means yet applied.

We have already alluded to the intense heat employed by Boyle and Hanckwitz in their distillation of phosphorus: the same is also necessary in the present mode of manufacture. The vessels in which the phosphorus is separated consist of a number (about ten) of small retorts, of a convenient shape, carefully constructed of the most refractory fire-clay, narrowed at their mouths, and arranged nearly close together in a horizontal row in a furnace, somewhat similar to the retorts in a gas-works. They are placed in a nearly horizontal position, with their open ends slightly raised, and in a furnace so constructed as to subject them to nearly a white heat.

The black powder is introduced into the retorts when the latter are comparatively cool, and the retorts are about half filled with the mixture. Bent pipes, open at both ends, are inserted, air-tight by means of clay, into the mouths of the retorts, and their outer ends dip into warm water, in cast-iron basins, placed to receive the distilled phosphorus. I may now incidentally remark that all the manipulations with ordinary phosphorus in its simple and separated state are performed under the surface of water, otherwise it would quickly inflame and be reconverted into phosphoric acid.

The heat of the furnace is gradually raised, and in the course of some hours phosphorus begins to distil over, and accumulates in a melted state in the basins of water. The fire is still further raised, by gradual means, until a most intense heat is obtained; the phosphorus then distils rapidly. The heat, after being thus continued as long as any more phosphorus appears, is gradually decreased, and the basins containing the crude phosphorus are removed. The time occupied in the distillation is, in some cases, protracted from forty-eight to seventy-two hours.

In this operation the charcoal, at a high temperature, combines with the oxygen of the phosphoric acid, forming therewith carbonic oxide and carbonic acid gases, which escape through the nose-pipe, and the phosphorus thus set free is converted into vapour by the heat and distils over into the receiving basins. At the same time, the portion of water not extracted by the preceding process is also decomposed, and its hydrogen set free, combines with some of the phosphorus and forms phosphuretted hydrogen gas. The inflammable gases, carbonic oxide, and phosphuretted hydrogen, are conducted away from the phosphorus basins by means of a pipe and consumed.

When the distillation is at an end the furnace is cooled down and the residuary contents of the retorts extracted. The residue consists chiefly of charcoal (of which there is always an excess) and of undecomposed phosphate of lime, together with a few impurities, and is used in the composition of manures, the phosphorus in it being of too difficult or too unprofitable extraction. From the penetrating and destructive character of phosphorus, the retorts require frequent renewing, some manufacturers, when the best fire-clay is not employed, not using them for more than one operation.

The appearance of a phosphorus distillery containing between 200 and 300 retorts, which we have frequently visited when in full operation, and the furnaces at their maximum heat, is somewhat fearful:—The long, yellow flames of phosphuretted hydrogen and carbonic oxide shooting forth from the escape pipes; bits of burning phosphorus spitting forth in fiery balls from little crevices or leaks at the mouths of the retorts; the incessant bubbling of the vapour of phosphorus and escaping gases in the basins of hot water; the almost unbearable heat from the furnaces on each side, and from the red-hot flues under foot; the intolerable stench of phosphuretted hydrogen and burning phosphorus; together with the acid fumes, and filthy grim aspect of the place, combined to produce an impression on our senses which we cannot fail to remember.

The small cakes of crude phosphorus, each weighing several pounds, are collected from the iron receiving-basins when cold and melted together under water. The impurities which were carried over from the retorts into the basins by the current of gases and phosphorus vapour, now settle to the bottom of the fluid mass, and the supernatant phosphorus is drawn over into shallow copper pans, containing a small quantity of hot water to prevent contact of air, by means of leaden syphons previously filled with hot water, and allowed to solidify.

The large cakes or cheeses of phosphorus thus obtained still contain impurities, and are of a dirty red colour, chiefly arising from the mechanical admixture of a red variety of phosphorus in small particles. They are broken to pieces under cold water, and the fragments placed in hot water contained in a leaden vessel (heated by steam), together with a bleaching agent. The phosphorus is stirred with the heated mixture until it is bleached, or its brownish colour is removed, which generally occupies two or three hours.

The liquid phosphorus is again drawn by means of syphons into shallow copper pans, and allowed to solidify. It is then broken under water, and the pieces are placed in hot water in a double-sided (or steam-jacketed) vertical iron cylinder, lined with lead, with a perforated bottom covered with chamois leather and canvass, and while in the fluid state pressure is applied to the phosphorus, and it passes through the leather, &c., into a vessel of hot water beneath, leaving the residuary impurities, red

phosphorus, &c., upon the cloth in the form of a dirty reddish substance of an earthy appearance.

The bleached and purified phosphorus is now cast into wedge-shaped pieces, or it is moulded into cylindrical sticks, half an inch in diameter, and ten inches long, by the aid of glass tubes immersed into the phosphorus under water, or by means of an apparatus with tubes specially contrived for the purpose. On some occasions the phosphorus is very brittle and difficult to draw into sticks, but if it is in a satisfactorily pure state it is as ductile as lead or soft copper wire. The appearance of the purified phosphorus in the form of wedges is that of very transparent wax or glass of a slightly greenish-yellow colour, but when in the form of sticks it usually appears colourless. It is needful on all occasions to protect the purified substance from strong daylight, otherwise it soon becomes yellow and opaque. For conveyance it is always packed in water, generally in tin cans, the covers of which are soldered air-tight.

The phosphorus we have described is that of the ordinary kind, but there is another variety equally pure discovered in 1848 by Professor Schrötter, of Vienna, which is produced as follows:—The ordinary phosphorus in its purified state is placed under water in a cast-iron boiler over a furnace; then melted, cooled, and the water removed from its surface. The vessel is then immediately and securely closed, air-tight (except a small iron-tube for the escape of vapour), by a cast-iron cover. The lid has a vertical iron tube, closed at its lower end, fixed in it, which projects downwards into the phosphorus, and is open at its upper end for the reception of a thermometer. Heat is now gradually applied by means of the furnace until the phosphorus is at about 450° Fahr., and that temperature is maintained for a greater or less period of time, according to the amount of phosphorus operated upon, and the mass is then allowed to cool. A quantity of about 200 pounds is kept heated about three or four weeks. After this process the vessel is opened, and the phosphorus, which has now become a hard, red brick-like substance, is covered with water, and extracted by means of iron bars, &c. This process of conversion requires to be conducted with great circumspection, otherwise (from overheating of the phosphorus) fearful explosions may ensue; experience has, however, shown that they may be almost wholly avoided.

For commercial purposes the red or amorphous phosphorus, as it is termed, is required to be in a state of fine powder; the stony fragments are, therefore, broken into small pieces under water in a mortar, and ground under water between pieces of mill-stone in a vessel supplied with a small stream of water, which carries off the finer particles in its overflow stream into a large tank, where they gradually subside, or into a filter where they are collected. A process is then resorted to for the separation of any ordinary phosphorus which the powder may contain.

(To be continued.)

PHARMACY, TOXICOLOGY, &c.

On the Detection of Croton Oils in Mixtures,
by JOHN M. MAISCH.

IT may be sometimes of importance to prove in a fixed oil the presence of croton oil. Its peculiar effect upon the skin will show it whenever it occurs in a considerable proportion. There is, generally, little danger of mistaking for it other oils of similar properties; as, for instance, the sulphuretted oils of mustard, garlic, &c.

which manifest themselves by their odour. Fixed oils, however, might contain a sulphuretted oil, with its odour masked by some other perfume, and in so small a proportion as to produce only after several applications a slight inflammation of the skin; croton oil might be present under similar circumstances, and the question would then occur, How are we to prove the presence of croton oil? We can readily prove the absence of sulphur, but this would not prove whether or not croton oil was contained therein, unless we were by distillation to remove all volatile matter from this mixture. Small quantities cannot be well analysed in this way, as the inflammation produced might be too slight; besides, this method would not be one easily executed.

In searching for a convenient and ready mode for proving in oily mixtures the presence of croton oil, I availed myself of the results of Dr. Thomas Schlippe's researches on croton oil, (*Annalen d. Chem. und Pharm.* cv.) who, without being able to discover the cathartic principle, found the rubefacient ingredient, *crotonol*, to be a resinous principle, which, together with other resins and free acids, is removed from the oil, by agitation with an alcoholic solution of caustic soda, and then separated by neutralising the solution with hydrochloric acid. This mixture of various components of croton oil possesses the property of reddening the skin in a high degree.

I have followed strictly these directions, sometimes substituting for the soda, caustic potassa, and sulphuric for hydrochloric acid, with the same good results. Experimenting, first, with mixed oils, containing from $\frac{1}{8}$ to $\frac{1}{10}$ of croton oil, I afterwards operated with olive and almond oil, containing one drop to the ounce. The quantity of caustic potassa is regulated according to the amount of croton oil, or, if unknown, in accordance with the effect of the oil upon the skin. For oils insoluble in alcohol, I have used 85 per cent. alcohol, adding to it a sufficient amount of an alcoholic solution of caustic potassa, and agitating well; after separation had taken place, I removed the alcoholic liquid, and added to it an equal amount of water, and a sufficient quantity of sulphuric or muriatic acid. Soon an oily liquid rises to the surface, which, when applied upon the arm, produces in the course of three or four hours not only inflammation, but also the pustular eruption peculiar to the effects of croton oil. If the quantity was small, the acidulated liquid was shaken with strong ether, the ether evaporated, and the residue applied.

For castor oil, and in the presence of volatile oils, I have employed diluted alcohol, mixed with an alcoholic solution of potassa, and omitted the dilution of the decanted alcoholic liquid. The results were equally satisfactory, and in this manner I have detected croton oil, in the so-called castor-oil capsules.

It appears to me possible that croton oil may in this manner be detected in the stomach of animals poisoned by it; but not having the leisure for further experiments at present, I shall have to defer this subject until some other time.

According to Dr. Schlippe, croton oil contains about 4 per cent. of crotonol, and it is therefore necessary to operate very carefully, and without wasting anything. One accustomed to work with small quantities will not fail to detect it.

In conclusion, I will remark, that in pills and emulsions, croton oil is detected in precisely the same way; these preparations are first exhausted with ether, and the residue from the evaporation of the ether is employed.—*Amer. Journal of Pharmacy.*

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting, Friday, May 10, 1861.

The Duke of NORTHUMBERLAND, K.G. F.R.S., President,
in the Chair.

On the Relation of the Vegetable and Animal to the Inorganic Kingdom, by WILLIAM SOOVELL SAVORY, Esq., F.R.S.

AFTER some introductory remarks to show that plants and animals, when viewed in the relation in which they stand to inorganic substances, may fairly be regarded as together constituting one great kingdom of nature—the organized, the speaker proceeded to consider the relation of organized to inorganic bodies, in structure, in composition, and in function. He challenged the truth of the cell doctrine, and asserted that its leading propositions were not tenable. Not only are some of the simplest tissues of the most complex plants and animals formed without the direct agency of cells, but also that elaborate tissue, striated muscular fibre. He referred to the common *Amœba* or *Proteus*, as an example of a living form, simpler than an unicellular organism inasmuch as it exhibits no distinction of cell wall and cell contents; and to *Actinophrys*, as an illustration of a structure apparently cellular, produced by simple reticulation of a structureless amorphous substance. Vegetable cellular tissue has been generally supposed to be formed by the multiplication of distinct cells, but there is better evidence of its production by a similar process of reticulation of protoplasm. The simplest forms of life, and the simplest tissues of the highest, reveal no difference in structure,—no distinction of parts to the severest scrutiny, and cannot, therefore, as heterogeneous bodies, be distinguished from homogeneous inorganic matter.

Chemistry draws no line of demarcation between the organized and inorganic kingdoms. All questions of composition are of degree only, not of kind.

Life constitutes the grand distinction. The difference is infinitely greater between living and dead organic matter than between dead organic and inorganic substances. Without attempting to describe it, life may be distinguished by its effects. In life, when reduced to its simplest terms, and separated from all those elaborate details which invest it in the more complex forms, there exists a definite relation between destruction and renewal, a regulated adjustment between waste and repair whereby the condition is maintained, notwithstanding constant change.

Life is not a state of resistance. The proofs of this are clear and complete. Waste or destruction is a necessary, an inevitable condition of its manifestation. It is involved in every vital act. And the power of compensating for this waste or change, the repair or reproduction necessary to the continuance of life, involves that of assimilation,—the power of converting foreign matters into the structure of the organism: in other language, the power of appropriating food.

We cannot conceive life without including these two conditions—consumption and supply. Life is not a state of change only as opposed to stability; for this is simply a question of degree everywhere, and dependent on the conditions to which bodies are exposed. Neither dead organic nor inorganic bodies are immune from change. Nor is life peculiar as a process of repair only; for this may occur in inorganic bodies, as crystals, under favourable circumstances. But in life there is the constant and concurrent operation of these two processes, whereby it is distinguished from mere change on the one hand, and from repair on the other.

It does not appear that we can at present safely venture

further than this. If we attempt to define the vital process of nutrition, distinctions fail us.

The investigation of the phenomena of life has not been in any way assisted, our knowledge of the vital processes has not been in any measure advanced, by the assumption of what has been styled a "vital principle;"—an empirical term, which, like some others when employed in physiology, is, even at the best, equivalent to nothing more than the final letters of the alphabet in an algebraical formula: for it is, when used in its least objectionable sense, a mere expression of something unknown. But the assumption of such an agent or principle, however designated, annihilating or suspending the operation of forces acting elsewhere, has not proved altogether harmless in its influence upon the progress of knowledge. By referring all vital actions to this obscure agency, while nothing was thereby explained, inquiry was to a great extent, and for a long while, checked. Many dazzled by the idea that the nature of vital phenomena was exalted by thus associating them with some mysterious and peculiar principle apart from, and opposed to those agencies which act elsewhere, missed the grander conception, that even in the vital functions may be recognised the operation of forces, some of which, at least, are common to both kingdoms of nature; while, between these and others which appear to be peculiar to living tissues, it is probable that a relation may exist like that which prevails between the chemical and physical forces.

Again, it is needful to beware how we create artificial distinctions. Is there not much assumption involved in the confession that we are unable to construct the simplest form of living tissue? Men sometimes talk as if their powers were limited only by life. But can we construct a crystal any more than a nucleated cell? We may fulfil certain conditions, under which, as we have learned from experience, crystals are formed; but what is our share in the act itself? In like manner we may take a seed or an egg, and place them under circumstances in which they will develop. In either case we are acquainted with the necessary conditions, and we fulfil them. We can do no more.

The speaker concluded with some remarks on the fact that the tendency of advancing knowledge is to efface the lines of demarcation which have been hitherto drawn between the Natural Kingdoms.

THE SOUTH KENSINGTON MUSEUM.

Monday, January 7, 1861.

On the Importance of the Study of Chemistry,
by A. W. HOFMANN, LL.D., F.R.S.

(Concluded from page 11.)

I MIGHT go on for hours quoting illustration after illustration, but I must limit myself to pointing out a few of the advantages which the most important branch of human industry, *agriculture*, owes to our science. At an epoch when the resources of the agriculturist seemed almost exhausted, chemistry offered her hand, to lead him up a new ascent of improvements. An accurate study of the composition of plants, taught us the actual nature of their food,—that it was partly obtained from the atmosphere, and partly from the soil. It taught, likewise, that while the nutriment derived from the air (identical for all plants), is continually restored to it by the never-ceasing processes of respiration, combustion, and putrefaction; the mineral aliment (varying with each vegetable species), is drawn from a source which is liable to exhaustion. The knowledge of the importance, to the development of plants, of a due supply of their mineral constituents, implied the recognition of the necessity of faithfully restoring to the soil such inorganic matters as are removed from it in our crops. This principle sealed the pledge of alliance be-

tween agriculture and chemistry. To keep up the fertility of his fields, the cultivator of the soil had to make himself acquainted with its general composition, as well as with the nature both of the particular substances, which he is annually extracting from it, and of those with which he must supply the loss. For each of these purposes the aid of chemistry became indispensable to the farmer. Davy's excellent work on agricultural chemistry first called the attention of the more intelligent British farmers to the value of chemistry in the improvement of their art, but it is only within the last twenty years that the attention of agriculturists has been generally directed to the subject, more particularly by the works of Liebig and Boussingault. The nature of manures once clearly defined, almost every agricultural improvement at which practice had arrived by slow degrees, received a satisfactory explanation, whilst a variety of improved applications necessarily suggested themselves. The principles of fallowing and of the rotation of crops, and the theory of soil-burning, are no longer mysteries; the action of lime, of wood ashes, and of bones is now perfectly intelligible. In this country, too high a value cannot be set upon the discovery of new sources of material convertible by the farmer into human food. Chemistry has put us in possession of the excreted wealth, which centuries had accumulated in the islands of the Pacific; and if the means which it has suggested for preventing the enormous waste of valuable matter perpetually and irrevocably swept away by the Thames and others of our large rivers, have not as yet been perfectly successful, it has enabled us to substitute for natural manure, artificial products, for the components of which the refuse of every trade and manufacture is now carefully sifted. Perhaps there is no more striking illustration of the value of the aid which agriculture has derived from her new ally, than the success which of late has attended her search for mineral manures. This search, directed by the philosophical interpretation of a few isolated facts, has been rewarded by the discovery of considerable quantities of phosphate of lime in various parts of England; thus realising the prophetic anticipation of Liebig, that "in the remains of an extinct animal world, England is to find the means of increasing her wealth in agricultural produce, as she has already found the great support of her manufacturing industry in fossil fuel, the preserved matter of primeval forests, the remains of a vegetal world."

Nor is the increase of his vegetal produce the only object in the pursuit of which the farmer relies upon the counsels of the chemist; in the feeding and fattening of his animal stock, he stands in no less need of his instruction.

And here we may briefly allude to the benefits, which *physiology, pathology, and medicine* in general, have derived, and must still further derive, from the prosecution of chemical inquiry. The investigation of the food of plants was followed by a no less vigorous scrutiny of the food of animals. The materials consumed by animals were shown to fulfil two distinct purposes in their economy, enabling us at once to separate into two distinct classes the substances supplying,—each respectively,—the aliments of nutrition, and those of respiration. In vegetal fibrin and albumen, we became acquainted with a group of nitrogenous compounds, which we find again in the animal organism as the chief constituents of the blood. These materials, prepared by vegetal life, the herbivorous animal receives and assimilates into its blood, to supply the constituents of its different organs and structures.

Again, we have a series of substances, such as fat, sugar, gum, starch, &c., which, though not truly to be called nutriments, as having no part in the construction of the body, are no less essential to the maintenance of life. These substances serve the process of respiration; combining with the oxygen inspired by the animal, they continually compensate the loss of temperature, and thus become the source of animal heat. And here presents

itself one of the most interesting subjects for contemplation,—the animal organism in its relations to the surrounding atmosphere. The full appreciation of the effect of air and temperature on the animal system, was a most important step towards a true understanding of the conditions of health. A vast number of diseases originate in disturbances of the relation between the living system and these influences, the intimate knowledge of which affords to the physician the simplest, safest, and most efficient means of preserving health and of curing disease.

Valuable as have been the fruits of chemical inquiry, still more may be expected from the further prosecution of this study. The notion that the action of most of our medicines is chemical, is daily growing into a general conviction. We admit that with every change wrought by pharmaceutical agents in the state of our organism, there occurs a corresponding change in its composition, resulting from their reaction on one or more of its constituents. But of these transformations, which doubtless could be expressed in numbers as definitely as can our laboratory-processes, how few are we in a condition to explain; in how few instances has the physician even a vague conception of the mode in which any medicine performs its office! Nobody doubts the power, which the principles of the Cinchona bark, of the tea and coffee, exert upon the living body, but we are perfectly in the dark as to the way in which they act upon the animal economy. But if we meet with a series of similar substances in several animal fluids: e.g., *urea* and *creatin*, almost constantly present in urine; *glycogen* generally; and *cystine*, occasionally, excreted in the same liquid, and if we find that all these substances exhibit in their chemical relations a close analogy with *choline* and *lecithine*, we begin to feel a sort of anticipation of the manner in which these agents may act upon the system. Such examples illustrate at once the nature of the aid which the therapeutics may confidently expect from the progress of organic chemistry. Medicine some years ago found itself in a predicament very similar to that of agriculture at the same period; its resources appeared to be in a state of exhaustion, the rich capital of facts accumulated in the department of organic morphology by the industry of the anatomist, and by the acumen of the physiologist, could not yield its full fruits until an equivalent of knowledge had been drawn from the study of bio-chemical phenomena. This state of things, however, is rapidly changing; associated with chemistry, medicine no longer draws the veil of vitality over processes, the mystery of which may be unlocked by the key of analysis; it no longer shrinks from climbing, step by step, the ladder of recognition, because its upper extremity, disappearing among the clouds, seems to rise for ever beyond the grasp of inquiry. The special zeal with which the field of organic chemistry has been cultivated during the last thirty years, the simple and accurate methods which we now possess for determining the composition of organic products, the amount of analysis actually performed, and, more than all, the still untiring energy of the numerous labourers in the same field of investigation, hold out the promise that the connexion between medicine and chemistry, becoming daily more intimate, will be productive of benefits, the importance of which we can scarcely venture to estimate in the present state of our knowledge.

I have thus endeavoured to indicate some of the most remarkable features of the new field, into which the present generation has been conducted by natural sciences and especially of chemistry. I have tried to sketch the prospects, which a continuation of these labours promises to open before us. It remains to me now to consider the cultivation of chemical science under its other equally important aspect, as the unfailing source of the purest and highest intellectual enjoyment, and as a means of mental training, more effectual perhaps than any other discipline.

If poetic antiquity endowed with a new soul him who learnt to speak the language of another people, how shall we find a fit term to express the sharpening of our perception, the enlargement of our views, the opening of a perfectly new sphere of vision, accruing from the acquisition of a language which unfolds to us the comprehension of nature? Phenomena so common that we are accustomed to pass them by almost with indifference, how are they changed,—I might say ennobled,—by the light that science has shed over them! True it is that the guiding hand which, according to immutable laws, rules over the economy of nature may be recognised without the aid of science. It is visible to everyone who, with an open eye, looks into the world; but no one familiar with the mutual relations by which nature's phenomena are linked together, could deny that with each step forward in the field of research, the conviction becomes more deeply rooted, and that each new observation discloses a new source of the most elevated contemplations.

When glancing, in a former portion of this lecture, at the benefits arising to agriculture and medicine from the progress of chemistry, I have already hinted at the inquiries which have for their object the examination of the chemical conditions of vegetal and animal life, and at the light which they have thrown upon the true nature of respiration and nutrition. I am inclined to look again at these inquiries from a different point of view. They have taught us that animal respiration is a true process of combustion; in order to secure the necessary warmth, we have to burn at every moment a part of our body; this source of heat lasts so long as we supply to the seat of combustion continually renewed fuel; so long as the alternation of breathing maintains the supply of the necessary amount of oxygen, and accomplishes the removal of the products of combustion, i.e. of water and carbonic acid. With each respiration the animal robs the atmosphere of a certain quantity of oxygen, and introduces into it a proportionate bulk of carbonic acid. If we consider that the endless processes of combustion, carried out for domestic purposes and for the purposes of industry, produce the same effect; that, lastly, the transformations of organic matter, which we designate as putrefaction and decay, are, in many respects, nothing but processes of slow combustion: it is evident that the unlimited continuance of these conditions must gradually involve a change in the constitution of our atmosphere. Such a change has not yet been observed. It has been justly argued that the immense space of the atmosphere necessarily renders changes of this sort extremely slow, that our analytical methods are not sufficiently accurate, that our experiments do not extend over a sufficient length of time; nevertheless, who could fail to perceive that the compensation is furnished by the difference of conditions which govern the life of the plant in contradistinction to that of the animal? With its leaves, as with so many lungs, the plant withdraws the carbonic acid from the air. Under the influence of sunlight the carbonic acid splits into its constituents—the carbon is employed in building up the plant, whilst the oxygen is returned into the atmosphere. The plant is referred, for its support, to the compound which the animal ejects from its organism as no longer serviceable for the purposes of its life; but it requires only a part of this compound; the rest is set free, again to take part in the development of a new animal generation.

How simple and how wonderful are the means which Nature employs to fulfil her least, as well as her greatest designs!

A mine of interesting considerations is opened by the phenomena to which we have slightly adverted. The carbon of the plant is derived from the air, in which it exists in the form of carbonic acid. The carbonic acid is, as we have seen, partly furnished by the respiration of the animal; but in combustion as in decay a supply of

carbonic acid no less inexhaustible is secured to the atmosphere. But with how new a signification do these processes become endowed when we view them in relation to vegetal life! Combustion, which suggests to the ordinary observer the idea of destruction—decay, which we are in the habit of considering as the symbol of death, in the eye of the chemist are but changes of form bearing the germ of a new unfolding of life. Of all the changes which surround us in nature how different are the views we take, when once the study of chemical phenomena has convinced us of the indestructibility of matter, when once we have accustomed ourselves to consider, as fixed and unalterable, the sum of matter which, under the influence of the forces of nature in the processes of vegetal and animal life, is capable of assuming so endless a variety of form!

Contemplations of even a higher order have been attached to this recognition. No one is likely to deny the danger of attempting to refer the conditions of spiritual life to the laws of the material world, still, this speculation, which appears so consonant to ordinary conception, has been at all times indulged in. If in the gradual development of the cell, of the organ, of the animal,—each phase of which, although fulfilling a definite purpose of the present, nevertheless points to a future end as yet unfulfilled,—physiologists have seen but a special case of a much more general law, which governs likewise the unfolding and ultimate destiny of the human mind;—can we wonder that in the indestructibility of matter (the medium in which all these transformations are accomplished without a particle of it being lost),—chemical philosophers should with delight have imagined they had found a measure applicable to spiritual life, and that indestructibility of matter thus became to them a symbol of the immortality of the soul?

Thus we see emanating from the results of chemical inquiry a rich vein of considerations such as must be acceptable to every thinking mind; but these considerations are by no means limited to objects of general interest. No department of knowledge,—no branch of study,—can be imagined, but what might draw from the stores of chemical experience important solutions and significant suggestions. We find abundant proofs of this assertion in the chemico-agricultural, and chemico-physiological inquiries of Liebig, the general results of which are embodied in his admirable "*Familiar Letters on Chemistry*." The merchant, the political economist, the statesman, the ethnographer, the historian, each of them finds in this book numerous questions, in which he takes a lively interest, elucidated from the chemical point of view. Let us select a few examples from the superabundant material teeming on every page of Liebig's letters.

The ethnographer makes us intimately acquainted with the manners and customs of different nations, but how seldom does he go a step farther and attempt to explain such manners and customs (which are certainly not accidental) by referring them to the physical conditions of life of the respective nations, to the geographical situation or the climate of the countries which they inhabit! We obtain the most circumstantial accounts of the dietetics of different races, we learn with a sort of discomfort the small portions of, as we should think, unsubstantial nourishment which satisfy the inhabitant of a tropical country, while we lay aside with disgust the bill of fare of the Esquimaux, informing us of the quantity of blubber which constitutes the chief ingredient of his meal. How clearly has Liebig proved the natural necessity for this difference in the choice and quantity of food, by showing that it depends on the quantity of carbon, the combustion of which, under given circumstances, is required for the preservation of sufficient warmth! We have as little reason to admire the South for its moderation as to reproach the North for its extraordinary appetite. Every one who has lived some time in a tropical

climate recollects how his appetite diminished; whilst our Arctic travellers, as we see by their journals, become quickly reconciled to Polar diet. It is well known, moreover, that the beasts of prey of the Arctic regions considerably exceed in voracity the animals of the torrid zone.

If the historian inquires into the causes of the decline and gradual disappearance of the North American Indians, he is led to the perfectly correct conclusion, that a combination of circumstances must be involved in producing so striking a result. In any case he will learn with great interest the chemical conditions which are contributing to this catastrophe. Viewed in the light of chemico-physiological experience, the extinction of the red population must be ascribed to the animal food on which the Indian, as hunter, has especially to rely. The same experience has, in fact, taught us that a given surface of land, when serving the purposes of agriculture, is capable of supporting a number of persons from eighty to ninety times greater than when covered by forests inhabited by a nation of hunters. "Thus we see a scanty tribe of Indians dispersed over immense tracts of hunting ground, which must be considerably extended with the slightest increase of their number; and thus we understand the endless warfare which is decimating those red sons of the wilderness. It is no contest of principles, as with a civilized people, which, sparing the conquered, grants to him an honourable peace, but a mutual war of extermination—the desperate struggle for the morsel of bloody flesh which, like the beasts of his forests, the Huron tears with his teeth!" (Schödler.*)

It would be easy to multiply examples of the same kind, but I would rather allude, in conclusion, to the importance of the study of Chemistry, as a means of education apart from the advantages (accruing from the acquisition of such a vast variety of positive knowledge), which I have endeavoured to point out in the earlier parts of this lecture.

No one doubts the influence which mathematical pursuits have in developing the mental powers. No one can deny that the precision of thought, the correct inference from given premises, essential to progress in mathematics, gradually becomes a habit, which is soon transferred to intellectual labour of any other kind. The mathematician starts from certain fundamental truths, which, without external aid, he is capable of elaborating out of his own mind; and on this basis he piles up, step by step, the wonderful edifice of his science. It is this nature-like mode of construction, disdaining to raise another stone before the previous one is thoroughly secured, which constitutes the value of mathematics, as an element of mental training.

The pursuit of Chemical studies involves no less the development of the mental powers; it leads no less to precision of thought; it improves no less the habit of drawing correct conclusions, which, once acquired, is as easily transferred to other fields of mental labour. Chemistry, as a science, differs essentially from mathematics in this one respect,—that the truths which are its foundation cannot be drawn from the human mind itself. These truths are external to our mind, and can only be obtained by careful observation of the outer world; but, once our mental property, these experience-gathered truths afford a foundation equally adapted for the nature-like unfolding of a system of conclusions, which are not less logically linked together than the conclusions of the mathematician. The chemist has, however, this advantage over the latter:—whereas the mathematician has to find the guidance of his steps within himself, the chemist possesses in nature a mirror which, duly questioned, at every moment readily informs him whether he is still advancing in the right direction.

* Discourse on the Study of Chemistry as a means of Education.

NOTICES OF BOOKS.

Lights and Lighthouses: Report of the Commissioners appointed to inquire into the Condition and Management of Lights, Buoys, and Beacons.

THE blue book, detailing the evidence taken in the course of this comprehensive Parliamentary inquiry has recently been published in the forms of two massive volumes. These pages enter upon the discussion of several questions in physics, and include the relation of experimental results in many directions of scientific investigation, important in themselves and possessing great interest from the frequent recurrence of names of first-rate repute in science. Professors Faraday, Potter, Smyth, and Airy, Sir David Brewster, Sir John Herschel, Dr. Gladstone, Mr. Way, and Mr. Holmes have all been either directly called upon to assist in the inquiry, or their published investigations referred to for information. Foreign Governments and scientific authorities have also been consulted.

Her Majesty's Commission bears date December 8, 1858, and is addressed to Rear-Admiral William Alexander Baillie Hamilton, and Captain Alfred Phillips Ryder, of the Royal Navy; John Hall Gladstone, Esq.; Duncan Dunbar, Esq., Chairman of the London Local Marine Board; and Samuel Robert Graves, Esq., Chairman of the Liverpool Local Marine Board. The instructions conveyed to the Commissioners aforesaid charges them to inquire into the number, quality, and position of the lighthouses, floating lights, buoys and beacons on the coasts of the United Kingdom; to institute inquiries relative to the efficiency of the lights on our own coasts and those of our colonies, and the cost of constructing and maintaining these as compared with the expenditure for the like objects in foreign countries. Also to inquire whether the present system of management and control is well adapted for securing the most efficient lighting and buoying of the coasts of the United Kingdom and to suggest, if possible, any improvements calculated to ensure the more efficient conduct of that service.

The Report of the Commissioners is dated March 5, 1861. So great is the mass of evidence accumulated for the purposes of the inquiry that upwards of 900 closely-printed pages are taken up with the details of this laborious investigation. The great bulk of these particulars have no special interest for the readers of the CHEMICAL NEWS, but we purpose to devote a few pages in our Journal to the consideration of the scientific portion of the evidence, and to deduce therefrom the more important conclusions arrived at in the course of this inquiry.

With regard to the mode of taking evidence the Committee state at the outset that they have deviated from the system usually followed in similar cases by relying not only on oral evidence, but they have placed themselves in communication, through the medium of printed questions, with many individuals, both in this country and abroad, known to take interest in these subjects, and whose testimony would be regarded as worthy of confidence. On this point the Report sets forth that "*visd voce* examination is always best for extracting the truth from the unwilling, but written questions are best for obtaining deliberate opinions from men anxious to give them. A professional man could not be called before a Commission without offering to pay him not only for his expenses, but also for his loss of time. A number of answers to the same question admit of ready comparison, and that whenever a great number of skilled opinions are brought to bear on a single fact, if almost unanimous and from independent sources, greater reliance may be placed on the general conclusion than when a small number are consulted separately."

The Commissioners prepared accordingly a series of questions addressed particularly to men of science. These

were distributed among some fifty chemists, physicists, and mathematicians, and their replies were carefully collated and presented as an Appendix to the Report. Special investigations in several departments of experimental science have also been proposed by Dr. Gladstone. These refer to the production of light, its utilization; the space-penetrating power of differently coloured rays; direction and altitude of the light. In reference to fog signals, the study of the most favourable conditions for the transmission of sound. The relative durability of different building materials as applied to the construction of towers and beacons, constantly subject to the wearing action of the waves. Such, in a few words, is an outline of the questions considered in the Report before us. Many of the problems, as that of the economical production of the voltaic and magneto-electric lights, the later modifications of the lime-light have been the subjects of recent experiment, and are likely to meet with renewed discussion in the present day. On this account, therefore, we shall need no apology for introducing them in these columns to the notice of our readers.

In reference to "Circular No. IX." the general purport of the questions addressed to scientific men is as follows:—

I. The evidence of—

Optics.—(Production of Light.)

II. In lighthouses which are *easy of access*, do you think there is any method of producing light better than burning oil to the best advantage?

(Gas is used in some harbour lights. The magneto-electric light has been under trial at the South Foreland, and several other inventions have been proposed.)

III. Taking into consideration the difficulty of transport to lighthouses in remote positions, and the danger which results from an accidental extinction of such light, do you think that the method you have referred to could safely be substituted for oil in lighthouses so situated?

Utilisation of Light.

IV. Revolving Lights, &c.—What optical arrangement do you consider best for showing to an observer placed on the horizon, or at any point within it, for short periods, recurring at intervals, the greatest possible quantity of the light produced by the combustion of a given quantity of oil, or by any other given method?

(Metallic and other reflectors, polyzonal lenses, ground, and the same cast, totally reflecting prisms, and combinations of these, are used, or have been suggested.)

V. Fixed lights. What optical arrangement do you consider best for showing continuously and simultaneously to observers on the horizon, and at any points within it, the greatest possible quantity of the light produced by the method to which you have referred?

VI. Do you consider the methods which you have described applicable to floating lights, with or without modifications?

VII. Please to send any suggestions you may wish to offer relative to—

a. The manufacture of reflectors, including materials and shape.

b. The manufacture of glass.

c. The manufacture of lenses and prisms.

d. The placing of optical apparatus in lighthouses, so as to use the light produced to the best advantage, having due regard to the position and elevation of the light.

Colour.

VIII. Can you suggest any means other than the interposition of coloured glass for producing serviceable coloured lights?

IX. Have the goodness to send any suggestion you may wish to make relative to—

a. The manufacture of coloured glass.

b. The placing of coloured glass with reference to the light.

(Coloured chimnies, glass screens within the lens, the same outside, coloured panes in lanterns, and combinations of these plans, have been tried or proposed.)

X. *a.* What colours can, in your opinion, be seen at the greatest distance in all weathers, and at the same time can be most easily distinguished from each other, and from white light?

b. Is white light, or some coloured light, in your opinion, most serviceable in a fog? If the latter, what colour?

XI. Distance. Do you know any method by which the distance from a light may be determined?

a. When the height of the light above the water is known.

b. When the height is not known.

Mechanics.

XII. Can you suggest any improvements in

a. The construction of lighthouses in exposed or other situations?

b. The construction of beacons in similar positions?

XIII. Can you suggest any improvements in the construction of lamps, revolving apparatus, or other mechanical appliances now used in lighthouses, &c.?

Flotation.

XIV. to XVII. These questions relate to particulars of form, and to systems of mooring floating beacons.

Acoustics.

XVIII. What description or pitch of sound is most capable of penetrating a fog? What method do you consider best for producing such a sound?

XIX. What description of fog-signal would you recommend as the most serviceable?

(Gongs, automaton bells, and guns are used, as also reflectors and other contrivances for directing and condensing sound. Mechanical and steam whistles, &c., have been suggested.)

XX. What method do you think best for concentrating sound, or for throwing it in particular directions? Do you think that any method would enable mariners to distinguish with sufficient certainty the direction from which a sound proceeds?

XXI. At what altitude should fog-signals be placed, so as to be heard at the greatest possible distance?

Meteorology.

XXII. What is the elevation at which it would be desirable that light should generally be placed on the coasts of the United Kingdom, having regard to these facts, viz.: the higher the light the greater the distance to which the rays will reach before they are intercepted by the horizon? The lower the light the less likely it is to be obscured by clouds.

XXIII. Supposing the telegraph to be extended to certain lighthouses and light-vessels at salient points of the coast, what is the meteorological information which it would be most desirable to transmit to passing ships, and how and in what form could it be most readily received and communicated by light-keepers?

XXIV. Tide Signals.—What system would you recommend for general adoption in connection with lighthouses at the entrance of harbours, for indicating the state of the tide by day and by night, to persons in the offing?

(Various systems are now in use, including balls, flags, coloured lights, illuminated figures moved by the tide, &c., and others have been suggested.)

XXV. *a.* Are you aware of, or can you suggest any good system for identifying lighthouses and floating lights by day?

b. Can you suggest any improvement in the systems now used for identifying them by night?

In a future Number we shall proceed to a critical examination of the statements made in reply to the foregoing scientific questions.

Report on the Presence of Bisulphide of Carbon in Gas, addressed to Robert Jones, Esq., Engineer of the Commercial Gas Company. By FREDERICK VERSMANN, F.C.S. London: 1861.

[SECOND NOTICE.]

ALTHOUGH there can be no doubt that some sulphuretted hydrocarbons are generally present in coal-gas, it is quite certain that the principal sulphur-holding ingredient is the bisulphide of carbon. In whatever form, however, sulphur is present, the only practicable way of estimating the amount is by burning the gas in air, and so forming sulphurous acid, which is then converted into sulphuric and weighed as sulphate of baryta. The great difficulty hitherto has been to effect the condensation of the whole of the sulphurous acid. This would not, at first sight, appear to be a difficult matter; but experience has shown that it is not so easily accomplished.

The water produced in the combustion, when condensed, carries down with it a very small proportion of the acid. Some experiments made to ascertain how much would be secured in this way, gave an average of 24 grains of sulphate of baryta, equal to 3.28 grains of sulphur for the 100 cubic feet—a merely fractional part of the amount which we have shown was actually present in the gas. Even when the condenser is artificially cooled, the greater part of the sulphurous acid would appear to escape, and it is only by fixing it as sulphite of ammonia that it is possible to secure the whole. Several contrivances have been invented for effecting this, one of the most successful being that of Mr. Wright, who ensured the perfect combustion of the gas by burning it under a platinum rosette and then collected the products of combustion in a vessel containing ammonia. In this way over 40 grains of sulphur were found in 100 cubic feet of gas.

The plan which Mr. Versmann adopted is described as follows:—

"A small air and gas-burner was placed inside a very spacious glass-retort, open at the bottom, and afterwards closed by dilute ammonia, contained in a suitable glass-vessel. The retort was then connected with two Wolff's bottles, the first of which contained a solution of ammonia, and the second a solution of iodine in iodide of potassium; the gaseous products of combustion were forced through these liquids by means of a strong air-current, produced by a water aspirator, and thus absorbed.

"The solution of iodine was merely used in case some of the sulphurous acid escaping unabsorbed by the ammonia; but it was found in all these experiments that not a trace of it could be detected in the second bottle.

"The sulphurous acid combined with the ammonia to form sulphite of ammonia was then converted into sulphuric acid by a solution of iodine in iodide of potassium, precipitated by chloride of barium, and weighed as sulphate of barytes.

"This apparatus was connected with the neck of the retort; but a similar one was in communication with the body of the retort by means of a glass tube passing through the dilute ammonia into the body of the retort."

This description, it must be confessed, is not too intelligible; but we very well know, and shall presently illustrate, the difficulty of describing a system of apparatus without the aid of a drawing.

We have said before that we do not intend to question the perfect accuracy of Mr. Versmann's results; but we may just hint at the possibility that the rapid transit of a "strong" current of heated air through a bottle of ammonia would probably leave, in a short time, only a vessel of tepid water, whereby the advantage of the ammonia would be entirely lost; and further, that by passing quickly through the iodine solution, the sulphurous acid might perhaps escape oxidation and get away into the atmosphere undetected. This, however, is mere speculation, but the truth of it is rendered somewhat probable by

the fact that, in the experiments, which lasted a short time, Mr. Versmann obtained a much larger per-centage of sulphur than in those which lasted a longer time. For instance, the combustion of 118 feet gave only 2·96 of sulphur for 100 feet of gas, while the combustion of but 2 feet yielded at the rate of 6·7 in the 100 cubic feet.

The plan adopted in our own experiments is the contrivance of Dr. Letheby, and is at once the most simple and effectual means yet devised for estimating sulphur in gas. As no account of it has yet been published, we shall, with the Doctor's permission, describe the arrangement of the apparatus. This would be better understood with the help of a drawing, but for the present a verbal description must suffice. The objects are, to effect the combustion of the gas in a strongly ammoniacal atmosphere, by which means the sulphurous acid is fixed by the ammonia as soon as formed, and to condense the sulphite of ammonia. These objects are accomplished in the following way:—Two glass cylinders similar to those used for electrical machines are connected together by means of a wide glass tube fitted with bungs. To one of these cylinders is adapted the neck of a retort, which is curved downwards, and widened to admit for some distance the short chimney of a Leslie's burner, which is used to burn the gas. In the centre of the burner, and so as to come a little above the points of flame, a tube is fixed leading from a bottle of strong ammonia. From the end of the second cylinder a wide glass tube is carried a foot or two up into the air, to serve as a chimney and assist in creating a draught. With this apparatus it is only necessary to observe two precautions—to burn the gas very slowly,¹ and to keep up the supply of ammonia. The latter is of the utmost importance. It should be supplied so that the air escaping from the chimney connected with the second cylinder may always be alkaline. A large quantity of water condenses in both cylinders; but that one nearest the burner will be found to contain almost the whole of the sulphite of ammonia, some of which will be seen to have condensed in a solid form on the upper surface of the cylinder. After pouring out the solution, and rinsing out the cylinders and tubes, the sulphite is converted into sulphate of ammonia, and the sulphuric acid determined in the usual way.

Here we might leave this part of the subject, but we will just give the average results of three experiments in which the ammonia was imperfectly supplied, to show the great importance of a regular supply of this gas:—

	Cubic feet of gas burnt.	BaOSO ₃ per 100 cub. ft.	S. per 100 cub. ft.	Average S.
I.	17·6	222·6	30·53	27·9
II.	22	175·9	24·12	
III.	12·7	210·23	29·08	

With regard to the removal of bisulphide of carbon from gas, Mr. Versmann quotes with approval the opinion of Dr. Frankland published in the last edition of Ure's Dictionary, that there was little ground for hope that any practical means would be devised for its abstraction. He is a bold, if not a rash man, who, in these times, when the apparent impossibilities of one day are continually becoming the established results of the next, ventures to predict, or even to opine, the impossibility of accomplishing anything; and yet chemists are often indulging in vaticinations of the kind. Perhaps before Dr. Frankland had written the above opinion—certainly before Mr. Versmann had quoted it—the thing had been done, and by the use of the most simple means. On the 24th of May, 1860, Mr. J. E. Clift, from whose evidence we have before quoted, told the Committee of the House of Commons—and Mr. Versmann might have read it in the *Journal of Gas Lighting* for June 5—that Mr. Bowditch's process had been tried with success at Wakefield, and that he (Mr. Clift) was preparing to use it at Redditch.

¹ In all our experiments the gas was burnt at the rate of half a cubic foot an hour.

The London Companies, however, are strangely averse to improvements, when the use of them involves any payment to a patentee, and when it is found impossible to dispute a patent right (for which we are informed they have a common fund) they decline to use the process altogether. It remains, therefore, with the public to insist that the provisions of the Metropolis Gas Regulation Act are thoroughly carried out, and if the Companies do not choose to make use of the means for removing bisulphide of carbon, let them take care to prevent its formation.

We shall probably have something more to say on the same subject before long; but for the present we may conclude with the remark, that this Report is another illustration of the value of analytical certificates. In the gas which Mr. Versmann examined for the Commercial Company he found a maximum of 6·7 grains of sulphur in 100 cubic feet. In the gas which the Company supplied to their customers, after the publication of this Report, we found an average of 57·38 grains of sulphur in 100 cubic feet!

NOTICES OF PATENTS.

Sheet Iron. D. A. MORRIS.

THE intention of this patent is to produce on surfaces of sheet iron a hard, black, enamel-like coating, which is said to have great protective action against corrosion and the formation of rust, and also to give an improved appearance to the metal. The film of enamel is stated by the patentee to consist of proto-carbide of iron, and to be composed of one equivalent each of iron, oxygen, and carbon, or represented in chemical symbols, Fe O C.

The mode of proceeding contemplated in this invention would probably confer on the iron surface additional power of resistance against corrosive influences. But, inasmuch as the nomenclature adopted in the specification is a little ambiguous, we are unable to offer a more precise opinion upon the merits of the proto-carbide of iron containing oxygen as a protective against rust.

Colouring Matter. W. CLARK. (A Communication.)

IN order to obtain pure dye-extracts and brilliant lakes the inventor describes a new mode of treating madder and other tinctorial substances, whereby the more permanent colouring matters are purified from certain associated constituents, which interfere with the brilliancy of the dye. This end he proposes to accomplish by acting on the madder roots or powder with aqueous solution of sulphurous acid, to which a small proportion of sulphuric or other mineral acid has been added.

The principle of this process would appear to be susceptible of wide application, for among those colouring matters which are distinguished by their ephemeral character many are known to be bleached or entirely destroyed by sulphurous acid, while another more permanent class are able altogether to resist the action of this chemical agent.

Separating Copper from its Ores. P. SPENCE. (Not proceeded with.)

THE method of extracting copper described in the specification is applicable to all the sulphuretted ores of that metal, and to the poorer class of residual products obtained in the process of manufacturing sulphur and sulphuric acid from ordinary pyrites. The inventor, in dealing with any of these materials, grinds them to the state of fine powder, which is then subjected to a process

of roasting, at a moderate temperature, in a reverberatory or other suitable furnace. By this treatment the copper is mainly converted into sulphate, which may now be extracted by water, and in this manner separated from iron and the other ingredients of the ore.

The leading features of this invention are precisely identical with the process of treating copper ores patented many years since, and put into operation in this country by the Messrs. Bankart. It has long been remarked that a considerable loss of metal was experienced by the admission of rain to the heaps of roasted ore lying about the copper smelting works; and that iron tools left accidentally exposed to the drainage from these heaps invariably became coated with copper. This old observation appears to have suggested the several modifications of this general system of treatment, which have, from time to time, been proposed.

Obtaining Products from Coal, Gas-tar, Gas-pitch, Coal-tar, Asphalt, Resin, &c. E. PULS.

For the purpose of obtaining products from either of the materials above named, the inventor mixes them with a mineral acid, or with any oxidizing chemical agent, and then subjects the mixture to distillation in any convenient apparatus. Solid materials, such as coal, pitch, or asphalt, should first be broken into small pieces, or even reduced to coarse powder. On account of its low price, sulphuric acid is the oxidising agent generally preferred by the inventor; and in its use this acid requires to be intimately incorporated with the solid substance exposed to its action; but if a fluid, such as coal-tar, be the material employed, it is considered desirable to modify the action of the acid by previously mixing this latter with powdered charcoal, or with any other substance in a fine state of division which will not neutralise its effects.

It would be difficult to define exactly the nature of the chemical products obtained in these processes. Sulphurous acid would in all cases be liberated; but the nature of the fluid products of the distillation must vary not only with the different materials experimented upon, but will depend also on the proportion of acid or oxidising agent employed.

Manufacturing certain Gases applicable in generating Heat and Light, and in Bleaching. C. BINKS. (Not proceeded with.)

This invention has reference to the economical production of chlorine and oxygen gases, and is based upon the powers of affinity exerted at high temperatures between certain oxides and the elements of water and of hydrochloric acid. For the production of oxygen gas two methods are proposed. The first depends upon the decomposition of water, in the form of super-heated steam, by the combined agency of chlorine and of a substance capable of uniting with the hydrochloric acid produced. The second method consists in liberating oxygen from the brown or black peroxides of manganese by the action of chlorine assisted by a moderate heat. In regard to the production of chlorine the new process is based upon the decomposition of hydrochloric acid at a high temperature by the conjoint action of oxygen gas and of a material having an affinity for the elements of water.

In the foregoing specification the interesting alternations of chemical affinity appear to have been correctly defined, but in considering the expediency of the methods proposed for the economical production of oxygen and chlorine, we incline to the opinion that the recent suggestion of Deville relative to the preparation of oxygen, and the older methods of producing chlorine will maintain the advantage.

CORRESPONDENCE.

The Great Fire.—Storing Nitro.

To the Editor of the CHEMICAL NEWS.

SIR,—There are many facts well known, but the practical application of which to the ordinary affairs of life is but too often little heeded till some circumstance happen that fully impresses the necessity of making use of our knowledge. The recent great fire in Tooley Street, and the somewhat similar catastrophe which took place not long ago at Gateshead, will at once point to the fact that nitre must not be stored in the same warehouse with inflammable materials—as at Gateshead with sulphur, and in Tooley Street with oil, tallow, &c. Nitre, or saltpetre, is perfectly harmless by itself, and cannot be set on fire; but if sulphur, oil, tallow, or any other burning substances, be in contact with it, then a compound is produced having all the dangerous qualities of gunpowder, of which nitre is the active ingredient. Knowing these facts, it will be obvious that our warehousemen must in future store nitre in such warehouses as contain only incombustible materials. No amount of fire cement, iron doors, or brick walls, will save from destruction a warehouse on fire if it contain nitre and any other body that is inflammable, under ordinary circumstances. As I have before said, nitre is harmless enough by itself; and thus, by your kind aid, calling attention to this known fact, I leave the officers of the Insurance Companies to compel its being warehoused in separate and appropriate warehouses, away from anything that will burn.—I am, &c.

SEPTIMUS FISHER.

Chilswick.

Pure Sulphuric Acid.

To the Editor of the CHEMICAL NEWS.

SIR,—In reply to the inquiry of your correspondent "A Subscriber," in your Journal of the 29th ult., I shall be happy to supply him with pure sulphuric acid in any quantity and at any time if he will favour me with his address, as I am always making it, and have at this present time upwards of a ton weight in stock.

I am, &c.

PHILIP HARRIS.

Chemical Works, Bull Ring, Birmingham.

To the Editor of the CHEMICAL NEWS.

SIR,—In reply to the letter of "A Subscriber," in your Journal of the 29th of June last, I beg to inform him that I will supply any quantity of pure sulphuric acid, free from traces of any foreign matters that are injurious, at a very moderate cost. The cause of the inferiority of English acid of the present day is owing to the almost universal use of pyrites instead of the best Sicilian sulphur. The latter is only used at my Works.

I am, &c.

F. C. NORTON.

Oil of Vitriol Works, Bow Common, London, E.

The Sale of Poisons.

To the Editor of the CHEMICAL NEWS.

SIR,—The discussion which is taking place in your columns on the above subject cannot but be productive of good; it would be an injustice to the public if such an important matter were allowed to remain quiet. The Correspondents in recent numbers of your Journal very properly insist upon the importance of all persons who commence the business of chemist and druggist having a "Certificate of Competency." You refer (in a note appended to one of the communications) to the Pharmaceutical Society as an authorised body whose organization and

requirements are complete; but are you, or are many of your readers, aware, that a scheme is being discussed by the Council and Members of that Society for the introduction of members who have not passed any examination? the sole object of which departure from the original rules of the Society seems to be to increase their funds, which are found to be flagging.

Now, I submit that, should such an innovation take place, the Pharmaceutical Society will lose ground considerably in public estimation. The desire on the part of the Pharmacutists to raise the educational *status* of chemists was very praiseworthy, and they might eventually, by increasing the numbers of their members, and securing public favour to themselves, have occupied the entire ground now so unequally shared—i.e., in excess—by unqualified pretenders to the trade.

As the case is, however, with regard to that part of the dealings of chemists, which relates to the sale of poisons—it seems high time that the Legislature should enact some restrictive measures as to the kind of persons who may or may not embark in that deadly trade. As well as some compulsory clauses respecting the use of such methods in the dispensing and sale of poisons by authorized chemists that shall seem best calculated to protect the public from that class of accident, which has become but too common of late.

The most effectual plan that has been devised, is the use of the "safety or drop bottles," a description of which appeared in your columns some months ago. I would strongly urge upon the public the necessity of taking this matter into their own hands, and in default of compulsory legislation to that effect, they should, when ordering liquid poison of any description, require it to be sent out in the "narrow-necked bottle;" such a demand would be irresistible, and the object in view would be accomplished.

I ought to apologise for the free manner in which I have expressed my views, with the expectation of a place in your columns; knowing that you do not favour legislation on the subject, but that you advocate "free trade" in drugs; however, you must remember that "free trade" in poisons means free trade in death, for not only does it not afford any protection against accident, but it offers great facilities to the intentional poisoner, and gives a great increase to crime.—I am, &c.

PRO BONO PUBLICO.

Chemical Notices from Foreign Sources.

II. ORGANIC CHEMISTRY.

Oxyacanthine.—Karl Wacker gives (*Wittstein's Viertel-Jahresschrift*, Bd. x. s. 177) a full account of oxyacanthine. This body was discovered by Polex in the bark of the root of the common berberry. The author prepared it from the precipitate produced by carbonate of soda in the mother liquor from which berberine had separated. The precipitate forms a catechu-like mass. This is treated with water, acidulated by hydrochloric acid, the solution is filtered from the resinous matter separated, and then precipitated by ammonia. This precipitate is washed, dried, and then extracted with ether. The ethereal solution is allowed to evaporate to the consistence of an extract, which is dissolved in distilled water, with the addition of hydrochloric acid. This solution is largely diluted, and then saturated with ammonia, whereby a tolerably white precipitate of oxyacanthine is produced. By repeated solutions and precipitations it is obtained quite pure and snow-white.

Pure oxyacanthine, $C_{32}H_{23}NO_{11} + HO$, is a dazzling white powder, which, when kept in a shady place, undergoes no change; but when exposed to direct sunlight becomes of a yellow colour. This, however, as in the case of santonine, appears to be only a molecular change, for the solution of yellow oxyacanthine in an acid is again

precipitated white by an alkali. It can be obtained in the crystalline state by allowing a strong ethereal or alcoholic solution to evaporate spontaneously. The taste is a pure bitter, which does not long remain. It melts to a yellow liquid at $139^{\circ}C$, and at a greater heat gives off empyreumatic vapours, leaving a residue of shining carbon. Chloroform dissolves it easily under all circumstances. Strong hydrochloric acid dissolves it quickly, without occasioning any change of colour. Strong sulphuric acid forms a clear solution, which has a shade of yellow. Concentrated nitric acid gives a brownish-yellow fluid, from which warty crystals soon separate. Chromic acid is reduced to chrome oxide. Chlorine water gives a yellow solution to which ammonia communicates a darker shade, but causes no precipitate. The behaviour of iodic acid is worthy of notice; if a small crystal be placed in contact with either pure oxyacanthine or a salt, and then a few drops of water be added, a smell of iodine is immediately evolved, and starch-paper, touched with the mixture, is coloured blue.

A solution of a neutral salt of oxyacanthine shows the following reactions. Ammonia gives a white precipitation which dissolves in a large excess of the precipitant. The other alkalis and their carbonates give a white precipitate; so does lime-water. Nitro-picric acid gives a lemon yellow precipitate; tannic acid a yellowish cloudiness; iodide of potassium a white, and an ioduretted solution of iodide of potassium a brownish-yellow precipitate. Sulphocyanide of potassium, and bichloride of mercury, give a white, chloride of gold a yellow, chloride of platinum a pale, greyish-yellow, and nitrate of palladium an orange-coloured precipitate. Ferrocyanide of potassium produces a white, the ferricyanide a sulphur yellow. Phosphomolybdic acid gives a yellowish white.

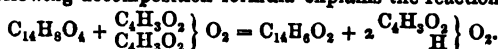
The salts formed by oxyacanthine are all white, and are obtained in warty or acicular crystals, with the exception of the acetate, which will not crystallise.

In conclusion, the author proposes the name of *Vinetin*, from *vinetier*, the French name for the berberry, because the word oxyacanthine calls to mind the *Crataegus oxyacantha* rather than the *Berberis vulgaris*.

Saligenine.—This body, say Beilstein and Seelheim (*Annalen der Chem. und Pharm.*, Bd. cxvii. s. 83), is best obtained pure by crystallization from pure benzole. From a hot solution it separates perfectly on cooling, for one part of saligenine requires 52 parts of benzole at $18^{\circ}C$. to keep it in solution. The sp. gr. of pure saligenine estimated in a cold saturated solution in benzole at $25^{\circ} = 1.1613$. Perchloride of phosphorus acts violently on dry saligenine. By the reaction of two molecules of the former on one of saligenine there are formed saliretine, oxychloride of phosphorus, and hydrochloric acid:—

Saligenine. $C_{14}H_8O_4 + PCl_5 = PO_2Cl_3 + 2HCl + C_{14}H_8O_2$ Saliretine.
Saliretine was attacked with difficulty by perchloride of phosphorus, and a chlorinated body was produced which contained no oxygen, but gave more chlorine than answered to the formula $C_{14}H_8Cl_2$. The analysis of this body gave C 33.12, H 1.11, and Cl 65.81 per cent. Dried at $150^{\circ}C$. Saliretine has at $25^{\circ}C$. the sp. gr. 1.1161.

By the treatment of saligenine with acetic acid anhydride, and some ether, in a sealed tube, saliretine is formed, which in this way is obtained pure with tolerable ease. The following decomposition-formula explains the reaction:—



Saligenine appears to form definite compounds with baryta and lime. It easily dissolves in baryta water, and from the solution evaporated in vacuo crystals resembling wavellite separate, which, dried at 100° , have the composition $C_{14}H_8BaO_6$.

III. TECHNICAL CHEMISTRY.

Preparation of Gun Cotton.—A chemist, who

does not give his name, has published (*Archiv. der Chem.*, Bd. oliv. s. 270) an account of a series of experiments which he has made on the preparation of gun cotton. The results he has arrived at may be summed up as follows:—The smaller the quantity of cotton wool operated upon the more easy it is to prepare a good gun cotton. An ounce of cotton wool he found was as much as could be manipulated with safety. The proportions found to give the best results were one ounce of cotton wool, 16 ounces of nitrate of potash, 12 ounces of English sulphuric acid (sp. gr. 1.84), and 12 ounces Nordhausen acid. Five minutes in this mixture was found to effect the change in the cotton perfectly. A longer exposure was not only useless, but mischievous. The temperature at which the action of the mixture goes on best is from 154° to 160° F.

MISCELLANEOUS.

The Cork Trade of Bordeaux.—There are at present in Bordeaux twenty-five cork factories, employing seventy-five workmen, using annually 3,396,846 lbs. of cork bark, and producing about 10,000,000 corks. 90,000,000 ready-made corks are obtained from the neighbouring places, Nèzac, Bayonne, and Provence. The cork trade of Bordeaux, therefore may be estimated at 100,000,000 corks annually, worth about 156,333*l*. Two-fifths of this quantity are exported to the French colonies, the United States, Chili, India, &c.; three-fifths are used in Bordeaux, at the average price of 1*l*. 4*s*. per thousand. These corks are divided into four classes—namely, the extra fine, used for champagne bottles, and sold for from 2*l*. to 4*l*. 16*s*. the thousand; the very fine used for bottles of wine of the first quality, and sold for from 1*l*. 4*s*. to 2*l*. the thousand; the fine, used for bottles of wine of the second quality, and sold for from 16*s*. to 1*l*. 4*s*. the thousand; and the common corks, used for bottles of common wine, and sold for from 2*s*. 4*d*. to 16*s*. the thousand. Nearly all of them are made by hand. There are so many difficulties and disadvantages connected with their manufacture by machinery that the use of machines is unprofitable. One class of workmen cut them into lengths, another turns them, and women are employed to separate them into the different classes named above. One cutter can prepare work for five turners, and one turner can make on an average 1200 a-day. The cutters are paid from 3*d*. to 4*d*. per thousand, and the turners from 1*s*. 2*d*. to 1*s*. 6*d*. per thousand; that is about 1*s*. 6*d*. a-day.—*Mechanics' Magazine*.

Glass.—Glass has become with us an article so singularly cheap and common, that we are apt to lose sight of its immensely diversified qualities; but if only considered from a philosophical point of view, we shall find that few of the substances which we have in daily use, either in a simple or compound state, can be compared to glass in point of importance and usefulness. Firstly, unlike any mineral, it is inodorous and clean to the fingers, and does not lose any of its weight by usage or wear; it is always transparent, whether in a cold or a red-hot state; it can take any shape whatever while in a state of fusion, and it retains it absolutely after it has cooled. It is capable of receiving the highest polish, and of taking any coloured tint, either on its surface or in its body; and it also has this peculiar and invaluable advantage, that it does not retain any taste of any liquid or acid it may have contained; it is the most flexible of substances while in fusion, and becomes harder than any pure metal when once it has become cold; lastly it is not liable to rust or to be consumed by fire. The innumerable applications of glass are such now, that it is difficult to imagine any one branch of industry or of manufacture which could be carried on a single day without the use of glass in one shape or another. To some of the

most important amongst the sciences, such as chemistry, physics, astronomy, the use of glass is a matter of absolute necessity; and in proportion to the gradual and increasing requirement of those last-named sciences, especially astronomy, it will be found that the glass manufacturer has been obliged to perfect his mode of manipulation, and, by the aid of chemistry, has of late years obtained such magnificent results that the field for astronomical observations has thereby been considerably enlarged.—*F. Joubert*.

Substitute for Silver.—Messrs. De Ruolz and De Fontenay have lately obtained, after several years' experiments, a new alloy, which may be very useful for small coin and for many industrial uses. It is composed of one-third silver, 25 to 30 per cent. of nickel, and from 37 to 52 per cent. of copper. Its inventors propose to call it *tiers-argent*, or tri-silver. Its preparation is said to be a triumph of metallurgical science. The three metals, when simply melted together, form a compound which is not homogeneous; and to make the compound perfect, its inventors have been compelled to use phosphorus and certain solvents which they have not yet specified. The alloy thus obtained is at first very brittle; it cannot be hammered or drawn, and lacks those properties which are essential in malleable metals. But after the phosphorus is eliminated, the alloy perfectly resembles a simple metal, and possesses in a very high degree the qualities to which the precious metals own their superiority. In colour it resembles platinum, and is susceptible of a very high polish. It possesses extreme hardness and tenacity. It is ductile, malleable, very easily fused, emits when struck a beautiful sound, is not affected by exposure to the atmosphere, or to any but the most powerful reagents. It is without odour. Its specific gravity is a little less than that of silver. An alloy possessing these properties must be very useful to gold and silversmiths. It can be supplied at a price 40 per cent. less than silver, and its greater hardness will give it a marked superiority. It may also serve as a substitute for gold-plated or silver-plated articles, which are now so common on account of their cheapness, but which will not bear re-plating more than a few times, and which are, in the long run sometimes more expensive than the pure metal. The new alloy, however, will be most useful for small coin. Its preparation and coining are so difficult that the coin made of it cannot easily be counterfeited. Its hardness would render it more durable than silver; and thus the expense of re-coining, and the heavy loss arising from the wearing of our silver coinage, would be greatly diminished.

ANSWERS TO CORRESPONDENTS.

C. Bades.—The tube in which the platinum wires are sealed, should have the air exhausted from it. The points are too close together, they should be eight or ten inches apart at least, and must be connected with the poles of a powerful induction apparatus. The price of a good apparatus of this sort (called a Ruhmkorff's coil) is very high. Probably, with the necessary batteries, one to answer your purpose would not cost less than 20*l*. You will obtain a better light by using charcoal points. The induced current does not give so strong a light. You can get American books of Samson Low, Ludgate Hill.

A Lady.—The subject of the crystallization of quartz is one of great interest to the chemist. We shall be glad to hear further particulars.

Dyeing Mixed Fabrics.—A correspondent is anxious to know how to dye a mixture of silk, or wool and cotton, with Magenta.

Solid Chloride of Manganese.—A correspondent writes:—"Can any of your readers inform me where to obtain a quantity of solid chloride of manganese? I am aware it is made in large quantity as a waste product, but cannot ascertain where, or at what cost it may be procured."

A Subscriber.—The *Journal of the Royal Institution* is not now in existence. The *Journal of the Chemical Society* appears every quarter, and the *Philosophical Transactions* once or twice a year, according to their bulk. The index to our first Volume is now out of print, and cannot be obtained except by purchasing a bound volume, of which only a few copies remain.

Books Received.—"A Guide to Croft Spa." By Miss D. Wilkinson. Dartington: 1861. "A Manual of Qualitative Analysis." By R. Galloway. Third Edition. London: Churchill. "Proceedings of the American Pharmaceutical Association for 1859." Boston.

THE CHEMICAL NEWS.

VOL. IV. No. 86.—July 27, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Normal Variability of the Properties of Atmospheric Air,
by M. HOUZEAU.

THE ravages of certain epidemics, and the diseases which seem to be confined to certain localities have long caused it to be supposed that the air may contain injurious principles, and thus temporarily and at intervals acquire unhealthy properties which it does not possess in its ordinary state. The facts I am now about to communicate to the Academy show that this variability in the properties of the air breathed by human beings is much more frequent than is usually supposed, and that, instead of being exceptional, it may be said to represent the normal condition of the atmosphere.

If two blue litmus test-papers, of the same size, sheltered from rain and sun, are exposed to the air at the same day and hour, it will be observed that in country air they become completely decolorized in three or four days, while the paper retains almost all its colour if exposed, under the same circumstances, to the air of a town. (Comparative observations made at Paris and Montmorency, August 8, 1856, and at Paris and Nanteau, near Nemours, September 3, 1857.) Similar effects are produced if the operation is performed in two places on the same horizontal line, and distant from each other one or two kilometres only.

This curious chemical reaction is, so to speak, constant both in summer and winter, but its intensity is augmented when the atmosphere is violently agitated, as during tempests and hailstorms, and at the approach of a storm. I think that the attention of agriculturists and physiologists should be directed to this fact, as it shows that plants and animals are differently impressed by the air according to their situation.

The variability of the chemical properties of the air is singularly confirmed by other characters. If the decolorising action of the air is generally less intense in towns than in the open country, its property of permanently reddening blue litmus seems to follow an inverse order. Blue papers, exposed to the air, but sheltered from rain and sun, receive a fast red tint sooner in a town than in the country. In Paris, in 1856, as d'Arcet had remarked in London long ago, this phenomenon was normal in certain streets abutting on the Conservatoire des Arts et Metiers, and it was observed at Rouen in quarters furthest removed from manufactories,—in those parts nearest the hills covered with verdure in summer. It is true that the prevailing winds blow in that direction, and may thus bring with them the smoke of the town. The papers thus reddened preserve their tints when submitted to the action of an air-pump, or when heated in a tube to 100°. The action of the atmospheric acid on the coloured re-agent usually manifests itself forty-eight hours after exposure to the open air. The edges

of the paper first become red, and at the end of the third, fourth, or fifth day, the change from blue to red is complete. Generally, when the decolorising phenomenon above mentioned takes place in a town, it is preceded by the reddening of the re-agent; this seems to me very rare in experiments made in the country.

These differences in the properties of atmospheric air, examined at the same moment, and in a somewhat restricted radius, are confirmed, and, at the same time, made more obvious, by another re-agent, litmus paper of a wine-red colour, half of it being iodised. A paper of this kind will often turn very blue in twelve or twenty-four hours, and sometimes even in six in the portion impregnated with neutral iodide of potassium, and exposed in the country, and yet will undergo no alteration, even in a much longer time, if exposed to such air as circulates freely in a town about one kilometre from the country station. (Rouen and its environs, July 11, 1858; June 6, 1860.) For many reasons, the phenomenon is less surprising when it occurs in localities further apart and situated at different heights, as Paris and the villages of Laqueus, in Brie, and Nanteau, near Nemours (September 3, 1857), and as Rouen and Florence, in Italy, according to the observations which MM. Carina and Silvestri have been kind enough to make with my papers (December 20, 21, and 29, 1860). Furthermore, it has frequently been proved that this difference in the action of the air on the same re-agents is perceptible, even when the papers are only six metres apart on the same horizontal line, but separated by a house built in a treeless field. Thus, it sometimes happens that demi-iodised litmus placed to the north of a building the blue colour is far more intense than that placed to the south of the same building and at the same height. An exceptional case is very rare, at all events, within my field of observation. A similar result is obtained if the experiment is made at the two extremities of a vertical line. At Rouen, blue litmus became more thoroughly decolorised, and demi-iodised litmus became bluer at the summit of the cathedral than at six inches from its base, &c.—*Comptes-Rendus*.

On White Lead, by A. ADRIANI, M.D., Ph.D., F.R.N.I.F., &c., &c., Analytical Chemist and Lecturer at Edinburgh.

SOME time back I had occasion to analyse a sample of white-lead paint supplied by a most respectable firm, which sample I found to contain 21.9 per cent. of sulphate of baryta. Doubting whether the sample given me was really made by the firm it was said to have been supplied from, I obtained a small keg filled with the paint, and marked by the firm as being white-lead paint of first quality, which paint I found on examination to be identical in composition with the previous sample, containing fully 22 per cent. of sulphate of baryta. It therefore appears, that with the practice of mixing the

white lead with linseed oil, that of adulteration is keeping pace, it being a well-known fact, that the detection is rendered somewhat more difficult when, instead of being sold as a white dry powder, white lead is sold in the shape of a thick paste.

I take leave to quote here the composition of twenty-seven samples of white lead, as made in different manufactories of the kingdom of the Netherlands, and analysed within the last six years by Dr. Vlaanderen,¹ assistant to Professor Mulder at Utrecht. The hygroscopic water was not separately estimated.

	I.	II.		Calculated.	Equivalent.
CO ₂	11'4	11'4	CO ₂	11'4	= 2
HO	2'3	2'4	HO	2'3	= 1
PbO	86'4	86'5	PbO	86'3	= 3

or PbO, HO + 2 CO₂, PbO.

	III.	IV.	V.	VI.	VII.	VIII.
CO ₂	12'4	12'2	12'0	12'4	12'0	12'3
HO	2'0	2'1	2'0	2'1	2'0	2'1
PbO	85'0	85'6	85'9	85'7	86'1	85'4

	IX.	X.	XI.	XII.
CO ₂	12'3	12'0	12'3	12'2
HO	2'3	1'8	2'1	2'0
PbO	85'5	85'6	85'5	85'6

Calculated. Equivalent.

CO ₂	12'2	=	5
HO	2'0	=	2
PbO	85'8	=	7

or 2 PbO, HO + 5 CO₂, PbO.

	XIII.	XIV.	XV.	XVI.	XVII.
CO ₂	12'7	12'7	12'5	12'7	12'5
HO	2'3	1'7	2'1	1'9	1'9
PbO	85'2	85'5	85'8	85'4	85'7

	XVIII.	XIX.
CO ₂	12'9	12'9
HO	2'2	2'1
PbO	85'3	85'4

Calculated. Equivalent.

CO ₂	12'7	=	3
HO	1'7	=	1
PbO	85'6	=	4

or PbO, HO + 3 CO₂, PbO.

	XX.	XXI.	XXII.	XXIII.	XXIV.
CO ₂	13'1	13'5	13'5	13'2	13'0
HO	2'0	1'5	1'6	1'7	1'8
PbO	85'4	84'7	84'9	85'2	85'2

	XXV.	XXVI.	XXVII.
CO ₂	13'3	13'2	13'1
HO	1'8	1'9	2'0
PbO	85'1	85'0	85'1

Calculated. Equivalent.

CO ₂	13'4	=	4
HO	1'4	=	1
PbO	85'2	=	5

The larger quantity of water is no doubt due to hygroscopic water. Two only of the samples of white lead examined have the following composition:—

PbO, HO + 2 CO₂, PbO.

Ten gave this:—2 PbO, HO + 5 CO₂, PbO.

Seven gave this:—PbO, HO + 3 CO₂, PbO.

Eight gave this:—PbO, HO + 4 CO₂, PbO.

Dutch white lead, therefore, consists of 1 equivalent of hydrated oxide of lead, with 2, 2½, 3, and 4 equivalents of neutral carbonate of oxide of lead; 2 and 3 occur more rarely, but 2½ and 4 are usual.

The results here quoted are published by Professor Mulder in a Dutch work, containing the results of

¹ Now, and for some time since, in the island of Java, as Chemist to the Colonial Government.

researches made in his laboratory. In order that it should not be supposed that the samples analysed were select samples, Professor Mulder distinctly states, that he was sorry he could not find from what manufactories the samples had been obtained, so that he could not make sure of another interesting point, viz., whether in the same manufactory the white lead was always obtained of the same composition. The quantity of hydrated oxide of lead which occurs along with the carbonate of lead is by no means unimportant. At the desire of the party for whom I made the analysis above referred to, I do not publish the name of the firm who supplied the white lead directly. I made three analyses of the sample, and found that the carbonate of oxide of lead and the hydrate of the oxide of lead only amounted to about 60·5 per cent., the remainder being the sulphate of baryta and the oil. Neither chalk, nor sulphates of lime or baryta are found in the Netherlands, unless it be in small quantity in the soils: these articles, as far as wanted, are imported from abroad.

Rubidium and Cæsium.

ACCORDING to a communication made by MM. Bunsen and Kirchhoff to Professor Redtenbacher, both rubidium and cæsium, the two alkali metals recently discovered by means of spectrum analysis, have a great chemical similarity to potassium, differing from this metal and from each other in the degree of solubility of their double chlorides of platinum and potassium. At the boiling point, 1000 parts of water dissolve 49 parts of chloride of platinum and potassium, 6 parts of that of rubidium, and 3½ parts of that of cæsium. Rubidium is found in largest quantities in the lepidolite from Rosna (in Moravia), and with traces of cæsium in certain saline mother-liquors. Cæsium seems essentially confined to mineral waters, 39,600 litres of that of Dürkheim yielding 6 grammes of chloride of cæsium.

TECHNICAL CHEMISTRY.

Destructive Action of Minium on the Hulls of Iron Ships. Letter from M. JOUVIN to M. DUMAS.

THE observations I made last March on the iron hull of the imperial packet *Guienne*, are confirmed in every point by those furnished by the hull of the packet *Le Bearn*. At first sight there was such strong resemblance between the state of the two hulls, that they seemed identical; but by going into details the following differences were observed:—

The whole of the hull of *Le Bearn*, which has received a coating of minium, has become dull red; the vivid tint of oxide of lead is extinguished in a vague colour as if washed out; or as if the painting had been covered with a sort of grey glazing. On this ground scales of hydrated oxide of iron stand out in relief, and are just as numerous and large as in the case of the other vessel. On this occasion I found that the layer of oxide of iron was much thicker at the corners and edges of the sheet iron plates than elsewhere, although the friction must be greater on these prominent parts. This fact escaped my notice with the hull of the *Guienne*.

I think the bubbles are as numerous on the rest of the coat, if not so large as on the *Guienne*. These bubbles may be always divided into two kinds:—

1. Bubbles containing some drops of liquid.

2. Bubbles without liquid, and containing merely air or a gas.

The latter are remarkable for being covered at their base with minute crystals of lead, overlaid with a thin coating of hydrated oxide of iron or yellow ochre. The internal surface of the pellicle of which they are composed, examined with a lens, shows needles of chloride of lead in the midst of slight concretions, which have the appearance of "horn lead" of mineralogists. (*Kérassine*.)

The concretions of chloride of lead, which I found afterwards at the base of both kinds of bubbles, on the iron hull, imbedded in magnetic oxide of iron, the dark colour of which they relieve by their slightly amber tint.

The liquid contained in the other bubbles I have subjected to a more careful examination. Tested *in situ* it slightly reddens litmus paper. To the taste at first it is freely styptic and rough, a taste due to the ferrous chloride, as I said in my first paper; to this soon succeeds a sweetish taste, resembling a weak solution of acetate of lead. In my observations on the *Guienne* I recognised, by this taste, the presence of a dissolved salt of lead in the liquid contained in the bubbles. I did not mention it at the time, because I had not determined its nature.

In order to find out, I collected a certain quantity of the liquid by absorbing it with a piece of blotting-paper, when it became easy, by means of distilled water and re-agents, to determine that the liquid consisted of a solution of ferrous and plumbic chlorides. Do not these two chlorides form a true soluble saline combination? I am strongly inclined to this opinion. When occasion serves, I propose to revert hereafter to this study. One fact which my experience has taught me is, that solutions of ferrous chloride become, by contact with minium, charged with chloride of lead, as may have been easily foreseen.

The liquid of the bubbles, when exposed to air, quickly becomes covered with a thin pellicle of sesquioxide of iron, without precipitating the slightest trace of chloride of lead. This salt does not appear until only a trace of liquid remains at the bottom of the capsule. By means of the microscope, and even with a lens, it is easy to detect in the midst of the sesquioxide of iron the play of colours of the micaceous spangles of this salt, the deposit of which in the capsule seems to me to be effected in the same manner as if the bubbles were dried.

There was a degree of regularity in the disposition of the concretions on the hull of the *Guienne* which was not to be found on that of *La Bearn*. In other respects it presents exactly the same foliated structure; it has always externally and at the moment of immersion the same gray colour of the iron; then, under this thin envelope, yellow ochre; and at the base the dark green of hydrated ferrous oxide and ferroso-ferric oxides. In all these points the concretions on the two vessels were almost identical. We shall likewise soon establish their chemical identity.

To conclude. Two chemical experiments on a very large scale have been originated in the workshops of *La Ciotat*. 1200 square metres of sheet iron, covered with linseed oil, and mixed with minium for the first layer, and the same preparation, with the addition of mercuric sulphate (7.5 per cent.), for the second layer, have been launched on the sea; that is to say, that, endowed with a positive electrical polarity (for the hold itself is also painted with minium), this gigantic electrical pair, forming at the same time a kind of condenser, has been plunged into a solution of alkaline chlorides

saturated with air. It has then made two or three voyages to Brazil, touching successively at Lisbon, St. Vincent, Pernambuco, and Bahia, to go into port according to circumstances, either at Rio or Bordeaux. During these long voyages this hull, besides the friction of the water and the shock of the waves, has undergone great inequalities of temperature, which must have caused prodigious dilatations in its metallic sides.

We have now before us the results of these two experiments:—

1. A considerable quantity of oxides of iron (ferroso-ferric oxide predominating) either in concretions or pulverulent.

2. Ferrous chloride.

3. Plumbic chloride.

4. Metallic lead.

Chemical analysis has not hitherto enabled me to discover more than this, either in the concretions or bubbles, besides, of course, minium and the oily menstruum.

The following is the mean of ten analyses made by the same process and under the same conditions:—

	Concre- tions of the <i>Guienne</i> .	Concretions of <i>La Bearn</i> .
Sesquioxide of iron	72.45	70.54
Ferrous chloride	2.85	2.86
Plumbic chloride dissolved by ferrous chloride	2.80	2.52
Oxide of lead mixed with chloride . .	7.30	4.95
Alkaline chlorides	0.87	1.42
Organic matter	3.73	4.99
Water	10.00	12.72
	100.00	100.00

What has become of the mercuric sulphate? Hitherto I have failed to discover any traces of it. The transformation undergone by this salt by reason of the oil with which it is mixed, will be made the subject of another paper.—*Comptes-Rendus*.

On the Origin and Progress of the Phosphorus and Match Manufactures, by G. GORE.

(Continued from page 32.)

THIS wet and finely-divided substance having been dried, sifted, and packed in air-tight tins, is ready for sale.

It is well known in modern chemistry that a substance may exist in two or more physical states, possessing very different physical and chemical properties, and that there may be as great a difference in the properties of the same substance in its different states of aggregation as there is between two chemically different substances. For instance, there is as great an amount of physical difference between carbon as it exists in the diamond and as it exists in pure lamp-black as between copper and silver or silver and gold. The two kinds of phosphorus we have described are, then, precisely the same chemical substance, but in different states of aggregation. The following is a comparison of their properties. We will, for convenience, term them white and red phosphorus:—

White.	Red.
Poisonous.	Innocuous.
Evolves a strong odour.	Nearly odourless.
Phosphorescent—luminous in the dark.	Not phosphorescent—perfectly illuminous.
Melts at 108° F.	Melts at above 500° F.
Very transparent.	Opaque.

White.	Red.
Almost colourless.	Varies in colour from nearly black (with metallic lustre) to iron-gray, brick-red, crimson, and scarlet.
Freely soluble in various liquids.	Nearly insoluble in all liquids.
Distinctly crystalline.	Destitute of all crystalline structure (amorphous).
Soft, may be indented by the nail.	Hard as a common red brick.
Flexible as copper or lead.	Brittle as glass.

The great and most conspicuous fact is that red phosphorus may be kept in the dry state, exposed to the air, without inflaming; whilst the ordinary variety, under the same circumstances, quickly ignites. The minutest quantity of ordinary phosphorus in the red or amorphous variety can be detected by digesting the latter, in the state of powder, in bisulphide of carbon, and then letting fall a single drop of the clear liquid upon a saucer floating upon boiling water in a dark place; luminosity will immediately appear if white phosphorus is present.

There are several uses to which phosphorus has been applied, but, as far as quantity is concerned, almost the only important application is in the manufacture of lucifer matches. It is remarkable that, although the property of phosphorus of igniting by friction was known soon after its discovery, it was not until about the year 1833 that it was successfully applied to the manufacture of matches. It was then sold wholesale at four guineas a pound; in 1837, at two guineas; and at the present time, at less than half-a-crown. Manufactories of it exist in Great Britain, France (Lyons), Bavaria, Baden, Austria, and Sardinia.

Since the commencement of the manufacture of phosphorus upon a large scale in England, in 1845, the value of phosphorus imported into this country has regularly and rapidly decreased. According to the reports of the Great Exhibition of 1851, the value of phosphorus imported from all parts into Great Britain in 1844 was 2567*l.*, and in 1850 only 3*l.* And since that period it has become an article of constant export to the Continent and other parts, the proportion consumed in the United Kingdom being comparatively small. In his "Catechism of Agricultural Chemistry," Professor Johnston states that 200,000*lbs.* of phosphorus are consumed in London alone per annum. Persons conversant with the actual consumption know that at that time, and probably at the present hour, it would not exceed 20,000*lbs.*

It is also stated in a recent publication on Chemistry, by Professor Muspratt, (article "Phosphorus," p. 68*c.*) that phosphorus "is prepared as an article of manufacture in large quantity in London and Paris. Kane has calculated that in the latter city alone about 200,000 *lbs.* are yearly produced." There may have been once a very small production in London and Paris, but it has long since entirely ceased. The enormous consumption of fuel necessitates that, for an economic production, the manufactory be placed where coal is cheaply obtained; and the quantity of phosphorus produced in all France, at the date of Dr. Kane's book, is not likely to have been at all more than 20,000 *lbs.* It is plain that, by mistake, a cypher too many has been added.

Reserving a few remarks upon the importance of the labours of Brandt, Boyle, and others who investigated phosphorus, until the close of this article, I will now

give a brief account of the gradual development and present condition of the lucifer match manufacture.

The earliest recorded means of obtaining a light appears to have been by means of rubbing together two dry sticks. A hole having been made, by means of a sharp stone, in a dry piece of wood, and, by the same means, a piece of stick of different wood—laurel with ivy for instance—having been cut to a point at one end, the sharp point was inserted in the hollow, and rapid rotation imparted to it by means of the hands, similar to the motion of a chocolate stirrer, until, in a few minutes, the friction of the point generated sparks, from which a fire was obtained. Sulphured matches are also of very ancient origin.

The next method—that of the common flint and steel—was in use for centuries, and can even now be remembered by every adult person. It consisted in striking the edge of a small piece of flint against the edge of a rod of iron or hardened steel, in such a direction that the sparks which occurred fell upon pieces of tinder (charred linen), contained in a shallow tin vessel, and ignited them. By gently blowing the ignited tinder, the combustion spread and became sufficient to inflame a sulphured match. If the tinder was at all damp, it failed to ignite.

The discovery of phosphorus in 1669, with its luminous and igniting properties, at once indicated the possibility of using it to obtain a light; but no successful experiments of the kind at that period have been recorded. It was not until 1833 that it was successfully applied. In the meantime, a number of inventions for obtaining a light, some involving the use of phosphorus and others not, were adopted to a limited extent. Phosphoric tapers, consisting of ordinary wax tapers, with their wicks coated with phosphorus, and enclosed in air-tight glass tubes, were for some time in use. To obtain a light with them, the end of the glass tube was broken off, and the phosphorus, now exposed to the air, oxidised quickly and inflamed.

The next plan consisted of a well-corked bottle, coated internally with phosphorus; a sulphur match was introduced into it, and a little of the phosphorus adhering to the top of the match, caused the sulphur to inflame on coming into contact with the air. If it failed to ignite, the action was assisted by rubbing the match upon the cork of the bottle.

Homburg's pyrophorus, or fire-bearer, consisted of a dried and roasted mixture of three parts of alum and two parts of flour or sugar contained in a well-closed bottle. A little of this powder exposed to the air upon cotton wool ignited the cotton. This mixture contained no phosphorus. There were several other mixtures of a similar kind by Gay Lussac, Dr. Hare, and others, but they were only used by the curious and scientific.

The "inflammable air-lamp" of Volta consisted of a stout glass vessel containing zinc and diluted oil of vitriol. Hydrogen gas was generated by these substances, and on turning a cock the issuing jet of gas was ignited by an ingenious contrivance, consisting of an electrophorus, which passed an electric spark through it at the moment.

The "light syringe" consisted of a vertical brass cylinder and piston, with a small piece of tinder placed inside at the bottom and closed end. On compressing the piston very rapidly and with great force, heat was evolved by the condensation of the air and ignited the tinder, which was then removed by various contrivances.

In 1804, a little bottle, containing a mixture of phosphorus, oil, and wax came into use. A sulphur match

was dipped into the composition and then ignited by rubbing it upon the cork of the bottle. This was succeeded by a similar bottle of composition, with matches tipped with a mixture of sulphur and chlorate of potash.

In 1807, the "chemical matches," composed of sulphur matches tipped with a mixture of chlorate of potash, sugar, gum, vermilion, and sometimes camphor, were invented. They were ignited by dipping them into a bottle containing asbestos fibre moistened with sulphuric acid.

About the year 1820, Döberienner discovered that finely-divided platinum has the property of causing a mixture of oxygen and hydrogen gases to unite and inflame, and invented his well-known hydrogen lamp. This consisted of an apparatus somewhat similar to Volta's for generating hydrogen by means of zinc and diluted sulphuric acid in a strong glass vessel. On turning a small tap a jet of the gas issued forth and impinged upon a small ball of the platinum powder which immediately became red-hot and ignited it; from this a taper was lighted. By a peculiar mechanical contrivance in this apparatus, when the gas issued from the orifice, the acid and water came in contact with the zinc and generated a fresh supply of gas; and as soon as the tap was closed and a sufficient stock of the gas was produced, the liquid was expelled by the gas from contact with the zinc, and the generation of gas ceased.

In 1826 "lucifers," or light-bearing matches, were invented. They consisted of ordinary sulphur matches, tipped with a composition of chlorate of potash and sulphuret of antimony, and were ignited by drawing them briskly between folded glass-paper. The defect of these was that they required a considerable effort to ignite them, and the composition was apt to be torn off by the violence of the friction.

These were succeeded in 1828 by the "Prometheans," consisting of a small roll of gummed paper, containing at one end a dried paste of sugar, chlorate of potash, gum, and water, together with a small bulb of glass containing oil of vitriol. They were ignited by means of a small pair of pliers supplied with the box of matches.

In 1830, bottles, containing a mixture of one part of manganese and two parts of phosphorus, were patented for producing a light. Into these bottles common sulphur matches were dipped, and ignited by oxidation of the phosphorus on exposure to the air.

All these inventions failed in superseding the old-fashioned tinder-box with its flint and steel, chiefly on account of their greater expense, but also on account of their dangerous and uncertain action. About the year 1833 phosphorus was at last successfully introduced into match composition, and from that period the tinder-box rapidly declined in use, and the new matches became a successful and extensive branch of manufacture.

The phosphorus match did not, however, at once acquire the degree of perfection it now possesses. A variety of substances were tried in the preparation of the match-splints and in the composition of the inflammable paste: vegetable wax, bees' wax, sulphur, stearine, oil, and fat to make the wood ignite; glue, gum, and sugar to make the mixture adhere; chlorate of potash, saltpetre, sulphur, sulphuret of antimony, antimonial saffron, red lead, and peroxide of manganese to increase the combustibility; powders of charcoal, cork, pitch, resin, magnesia, lime, or chalk, to make the composition more porous; powdered glass, sand, or emery to increase the friction; and vermilion, red lead, red ochre, Prussian blue, smalts, ultramarine, or chromate of lead, as a colouring matter, and to disguise the other ingredients.

(To be continued.)

PHARMACY, TOXICOLOGY, &c.

Caffein as an Antidote to the Poisonous Effects of Opium.

Professor Henry Fraser Campbell, of Georgia, in the *Southern Medical and Surgical Journal*, publishes an article on this subject. After giving some account of caffein, and the use of coffee as an excitant, he describes the patient as a young man, 24 years old, who had, in a fit of mental depression, swallowed an ounce and a-half of laudanum, nearly an hour before his visit, 8 p.m. Narcotism had proceeded so far that emetics could not be employed; the muscles were greatly relaxed, the tongue falling back in the mouth and tending to stop respiration, and respiration exceedingly feeble. Cold water was applied to the head for a time, artificial respiration resorted to, and the stomach-pump then vigorously applied. At twelve o'clock the same night the case seemed more hopeless, respirations but four to the minute. Artificial respiration in a sitting posture was then resorted to, which seemed to have a temporary good effect. Dr. Campbell then thought of coffee, but the patient could not swallow, and he doubted the propriety of using the stomach tube in the then condition of the patient. Finding it difficult to get a strong infusion of coffee, the idea of employing caffein occurred, and having dissolved 20 grains in a quantity of infusion of coffee, it was administered as an injection by means of a syringe. In half-an-hour the respirations were eight per minute; an hour had not elapsed before he "forcibly jerked his left arm from the assistant," and told them "to let him alone." The narcotism slowly passed off, and at ten o'clock next day, when Dr. Campbell called, the patient had left the hotel and gone home. Although the effects of the narcotism were apparent for several days, they soon wore off, and the patient entirely recovered. Dr. Campbell believes that the caffein in this case was largely concerned in the recovery, and points to that alkaloid as probably possessing valuable antidotal powers in cases of narcotic poisoning from this drug.

On the Alkaloid and other Constituents of Coca Leaves, by Dr. A. NIEMANN.

THE inaugural dissertation of the author is published in *Wittstein's V. Schrift.*, ix. 489-524. It contains many interesting facts, but is too lengthy for translation; we therefore give in the following a condensed account of it.

After discussing the importance of the vegetable alkaloids in a medicinal point of view, and for the examination of crude drugs, the author disapproves of such incomplete investigations as enrich our knowledge with nothing else but names, and then considers the interest attached to those stimulating vegetables denominated by Von Bibra "narcotic victuals," like coffee, tea, cacao, and tobacco. The coca leaves are employed for similar purposes by the natives of Peru and Bolivia.

The Austrian frigate *Novara* brought from Lima a quantity of good coca leaves, which Professor Wöhler handed to the author for investigation.

After treating at length of the botanical characteristics of the order *Erythroxylaceæ*, which comprises only the genus *Erythroxylon*, and its geographical extension, he gives a minute description of *E. coca*, its culture, and the use and properties of its leaves, referring to the observations and opinions of Von Martius, Poeppig, Von Tschudi, Weddell, and Scherzer, some of whom have

advocated the introduction of the coca into the European navies and armies on account of its well-known qualities to preserve life and strength for a considerable time without food.

The author reviews the various incomplete investigations of Wackenroder, Johnston, Gaecke, and MacLagan, and states that the alkaloid prepared by Pizzi, of La Paz, and brought by Tschudi to Europe, was analysed by him and found to be gypsum.

The following is the author's process for preparing the alkaloid, which he proposes to name *cocaina*:—The leaves are exhausted with 85 per cent. alcohol, containing $\frac{1}{50}$ th sulphuric acid, the tincture treated with milk of lime, the filtrate neutralised with sulphuric acid, and the alcohol regained from the filtrate by distillation. The syrupy residue is treated with water to separate resin, the liquid is precipitated by carbonate of soda, when an odour reminding of nicotine is emitted, and the alkaloid is exhausted by repeated agitation with ether, distilling off most of the ether, and evaporating the residue spontaneously. The yield is about $\frac{1}{2}$ per cent of crude *cocaina*, consisting of colourless crystals mechanically mixed with a yellowish-brown matter, of a disagreeable, almost narcotic odour, which cannot be removed by animal charcoal or re-crystallisation, but may be separated by washing with cold alcohol. Less loss is sustained if the ethereal solution is agitated with diluted sulphuric acid, the aqueous solution of the sulphate precipitated by carbonate of soda, and the precipitate freed from the last traces of colouring matter by alcohol. The alkaloid crystallises best on evaporating spontaneously an alcoholic solution previously diluted with water.

Pure *cocaina* is in colourless, transparent, rather large prisms, inodorous, soluble in 704 parts water, at 12° C. (53.6° F.), more in alcohol, freely in ether. The solutions have an alkaline reaction, a bitterish taste, promote the flow of the saliva, and leave upon the tongue a peculiar numbness, followed by a sensation of cold. The crystals fuse at 98° C. (208° F.), and congeal again to a transparent mass, which gradually assumes a crystalline structure. Heated above its fusing point it becomes coloured and is decomposed. It runs up the sides of the vessel, and it is uncertain yet whether a small portion will sublime unaltered. It fuses upon platinum foil, and burns with a bright flame, leaving a charcoal, burning with some difficulty.

No physiological experiments have as yet been made with it.

The alkaloid is readily soluble in all diluted acids, forming salts which are of a more intensely bitter taste than uncombined *cocaina*. It absorbs dry hydrochloric acid gas, fuses and congeals to a grayish-white transparent mass, which crystallises after some days; the solution crystallises in long, tender, radiating crystals. The sulphate and nitrate dry to an amorphous, colourless mass, which gradually changes to crystals, the latter with great difficulty. The acetate readily assumes the form of a crystalline, sometimes herbaceous-looking mass.

The following is the behaviour of a diluted solution of the hydrochlorate to re-agents:—

Fixed alkalis: a white precipitate, slowly soluble in a large excess.

Ammonia and carbonate of ammonia: the same precipitate, readily soluble in excess.

Carbonate of soda: white precipitate, insoluble in excess, becoming crystalline.

Sulphocyanide of potassium: a slight turbidity.

Protochloride of tin: a white, curdy precipitate, soluble in much nitric acid.

Picric acid: a pulverulent, sulphur-yellow precipitate, becoming resinous.

Tetrachloride of gold, even in the most diluted solutions: a voluminous, light-yellow precipitate, becoming flocculent, soluble in the heated mother-liquor, and crystallising on cooling in beautiful yellow scales, resembling iodoform; it crystallises from hot alcohol in golden yellow scales or in a dense granular powder.

Bichloride of platinum: a dirty, grayish-yellow precipitate, becoming crystalline, soluble on heating, scarcely soluble in muriatic acid.

Tannin: a very slight turbidity; in the presence of muriatic acid a voluminous, pearly-white precipitate, shrinking and becoming resinous in appearance.

Bichloride of mercury: a voluminous, soon flocculent precipitate, soluble in muriatic acid and in chloride of ammonia. It does not crystallise from its solution in alcohol.

Iodo-hydrargyrate of potassium: a curdy-white precipitate, soluble in muriatic acid and chloride of ammonia.

Molybdate of ammonia in the most dilute solutions: a voluminous, yellowish-white precipitate.

Iodine: a kermes-brown precipitate.

Iodide of potassium: scarcely a faint turbidity; when containing iodine a strong brownish-red precipitate, which darkens gradually.

Tincture of iodine: no turbidity in the alcoholic solution of *cocaina*. No reactions were obtained with phosphate of soda, bicarbonate of potassa, tartar emetic, acetate and subacetate of lead, sesquichloride of iron, and iodic acid.

Cocaina is dissolved by fuming nitric and by concentrated nitric, hydrochloric, and sulphuric acids, without colouration; when heated, only the latter darkens and ultimately blackens it. A characteristic test with oxidising agents has not been observed. In its chemical and also physical properties, it resembles atropia to some extent.

The atomic weight of the alkaloid was determined from the double salt with chloride of gold, and was found from the amorphous compound = 283, crystallised from hot water = 280, crystallised from alcohol = 288. On heating the dry double salt, a sublimation of benzoic acid took place, the first observation of the kind of any known alkaloid. Ultimate analysis showed the following composition:—

I.		II.	Calculation.		
C	66.8	66.8	C	32	66.20
H	7.1	7.5	H	20	6.90
N	5.4	5.4	N	14	4.83
O	20.7	20.3	O	8	22.07
	100.0	100.0	C ₃₂ H ₂₀ NO ₈	290	100.00

Vegetable Wax from Coca Leaves.—The precipitate occasioned by milk of lime in the alcoholic tincture was treated with cold ether, which, on evaporation, left a soft, sticky, brown mass, wholly soluble in boiling alcohol, from which, on cooling, a white body was separated, appearing, after repeated solution in hot alcohol, as a snow-white, granular mass. It fuses at 70° C. (167° F.), is slowly soluble in hot alcohol, readily in ether, and is not acted on by solutions of alkalis or acids. By elementary analysis 80.2 C., 13.4 H., and 6.4 O. were obtained, which leads to the empirical formula, C₈₀H₅₂O₄. The composition of this body, called *coca wax* by the author, closely corresponds with that of Mulder's grass wax and syringa wax.

Cocainic Acid.—After precipitating the cocaina by carbonate of soda, as above, the dark red filtrate is freed from ether, neutralised by nitric acid, the sulphuric acid removed by nitrate of baryta, the excess of the latter by carbonate of ammonia, and the liquid neutralised by nitric acid; acetate of lead now produced a voluminous, dirty brownish-green precipitate, which was decomposed by sulphuretted hydrogen, and yielded a clear, yellowish-red solution, of slightly astringent taste, reddening litmus, and giving the following reactions:—Alkalies and their carbonates, a deep red colour; sesquichloride of iron, a dark-brownish yellow colour; glue and bichloride of mercury, no turbidity; the nitrates of mercury, dirty yellow precipitates, soluble in acids; tartar emetic, a flocculent, brownish-yellow precipitate; albumen, dirty yellow; acetate of baryta and lead, dirty yellow, soluble in acetic acid; terchloride of gold, dark brown precipitate, soon after a mirror of metallic gold; nitrate of silver, a turbidity, on heating a dirty yellow precipitate, afterwards a dark brown powder; hydrochlorate of cocaina, no turbidity. The tannin solution reduced chromate of potassa, and on boiling, also, Fehling's solution of copper; it dried on evaporation to a transparent brownish-red mass, containing some inorganic matter.

The filtrate from the precipitate with acetate of lead yielded with subacetate of lead, a yolk-yellow precipitate, apparently containing the same tannin, partly modified through the influence of the nitric acid.

Odeorous Principle.—On distilling 100 grammes of the leaves, a slightly turbid distillate was obtained, which, on re-distilling it with chloride of sodium, yielded white globular masses, lighter than water, and possessing the peculiar, tea-like, strong odour of the leaves. The quantity was insufficient for further analysis.

PHYSICAL SCIENCE.

Some Observations on the Sensibility of the Eye to Colour, by JOHN Z. LAURENCE, F.R.C.S., M.B. Lond., Surgeon to the South London Ophthalmic Hospital.

If, closing one eye—say the right—any highly luminous white ground, such as some portions of the sky on a sunny day, is viewed with the left through a dark tube, so as to exclude all extraneous light, after a little the eye will begin to feel fatigued, and a librating circular smoky spectrum will be perceived at the end of the tube. When the tube is laid aside and both eyes are directed to the sky, a similar spectrum will be observed, projected, as it were, on the surface of the heavens, but much darker. But if after a time each eye is alternately opened and closed, a rose-coloured spectrum is seen with the left eye, a pale-green one with the right. These appearances are seen still better if, instead of the sky, a white screen is used as the plane of projection in the second part of the experiment. At first an almost black circular disc is seen; this becomes lighter and lighter, till it is finally succeeded in the left eye by a bright rose-colour disc, surrounded by a violet border; in the right eye by an equally bright green, with a rose border. These spectra sometimes appear as if upon the surface

of the screen, sometimes, on the contrary, as if originating within the eyeball itself, and indeed may be even seen with both eyes closed. To see the above phenomena in all their intensity, a slightly different plan must be adopted. As the field of projection, a sheet of dead black paper in a dark room is to be used; the spectra then seen with either eye are the same, and their colours most splendid, both as regards brightness and tint. At first an emerald-green disc appears, surrounded by a narrow carmine, or, perhaps, more accurately, magenta border; the magenta tint is then seen to encroach more and more upon the green, till the whole disc is of the former colour, surrounded by a bluish-violet border; this last, in its turn, invades the magenta, till the final spectrum is of one uniform indigo-violet colour.

The above is the general sequence of colours which I, and other persons whom I have asked to perform the experiment, have observed; but these are liable to exceptions. Occasionally, the librating spectrum observed at the end of the tube in the first part of the experiment, acquires a faint rose, green, or violet tint. Sometimes, I have seen the spectra of the right and left eyes, in the second part of the experiment, reversed as regards colour.

These facts appear to prove the following propositions:—

1. That colour sensations may be excited in the retina, or brain, altogether independently of any external colour-stimulus.
2. That as an optical analysis of white light may be effected by a prism, so with the eye we possess the power of effecting, what may be called, its physiological analysis.
3. The last proposition tends to the conclusion that white light consists of three fundamental colours—magenta, emerald-green, and indigo-violet—corroborating in a remarkable manner the opinions of Professor Maxwell and Dr. Young on the same subject.
4. That a colour sensation excited in one eye is generally felt in the other, although this latter has not been exposed to the influence of light in any part of the experiment; that, in a word, a very close sympathy exists in the two retinæ, of which the consentaneous action of the two irides is probably but a reflex nervous consequence.

I may here allude to a distinction in ocular spectra, which has, I believe, not been taken much account of by observers of these phenomena. Some spectra seem as if projected on the plane to which we direct the eye, and in that case appear, as I have found from numerous measurements, linearly magnified in proportion to the distance of the eye from the plane of projection. Other spectra, on the contrary, are perceived, so to say, in the eyeball itself, and are of a subjective nature. Independent of the differences of their apparent seats, the two classes of spectra present certain other well-defined distinctions. Projected spectra are only perceived with the eyes open, and are generally but faint in colour; while subjective ones may be seen with the eyes shut, and are always intense in colour. At the same time I am disposed to ascribe the differences of colour, in a certain degree, to the diluting influence of extraneous light; for projected spectra are always seen more vivid in a dark room than in daylight.

The green spectrum observed on a sheet of white paper, after prolonged contemplation of a red wafer, has been commonly explained thus—"When the eye has been for some time fixed on the red wafer, the part of

¹ Since writing this paper, my attention has been directed to a series of elaborate disquisitions by Brücke and Fehner in Poggendorff's *Annalen der Physik u. Chemie*, vols. lxxxiv., 1., and xlv., to which I beg to refer my readers.

the retina occupied by the red image is deadened by its continued action, and insensible to the red rays which form part of the white light from the paper; consequently will see the paper of that colour which arises from all the rays in the white light of the paper, but the red; that is, of a bluish-green colour, which is therefore the true complementary colour of the red wafer."³

That this explanation is not correct seems to me to be proved by the following experiment:—

I, at night, made a room (which is provided with thick American-leather blinds for ophthalmoscopic purposes), to all appearances, absolutely dark, then viewed with the left eye a small aperture in a dark box, covered with a piece of emerald-green glass, behind which was the nearly white flame of a lamp. The right eye was kept closed, and covered with a thick handkerchief. After a time I blew out the light in the box, and looked at a screen covered with a sheet of dead-black paper. With the left eye a large carmine-coloured projected spectrum of the flame could be seen; with the right eye I generally perceived no spectrum at all, or if any, but of a very faint tint. But, if the latter eye was exposed to a white light during the first part of the experiment, I invariably perceived the same spectrum with this eye as I did with the left one.

This experiment shows that the presence of white light is not necessary for the perception of complementary ocular spectra, and further would appear to indicate that for a sympathetic spectrum to be excited in the eye which has not been exposed to the colour-stimulus, the excitation of some light is necessary.

M. Plateau painted one half of a piece of paper red, the other green; and after alternately directing the eyes to each half, covered them with a handkerchief, and observed a black image, having on each side a complementary coloured image.⁴ He hence inferred that "the combination of accidental colours produces black." Sir D. Brewster very properly objects to this conclusion, "because the eye has been in succession rendered insensible to the two colours which compose white light itself."⁴ Elsewhere the same author says, "If we take the two complementary colours, namely, the red and the green tints forming the ordinary and extraordinary pencils in the polarised ring, which, by overlapping, form white light, then it is manifest that the accidental colour of the overlapping part is black, and hence the sum of the action of the red and green acting separately must also be black."⁵

Notwithstanding, however, the authority of Sir D. Brewster, the following experiment which I have performed appears to me rather to corroborate Plateau's view. If the two halves of a card painted red and green respectively, be illuminated by a green or red light, they appear black. In the same way, but depending on a different cause, the two halves of the card, if viewed through green or red glass, appear black.

Another set of observations, connected in a degree with the preceding, may be here noticed. Chevreul⁶ distinguishes two chief species of contrast of colours, simultaneous and successive contrasts. But an examination of these distinctions shows them in my judgment to be more apparent than real, and but the expression of

one fundamental fact, viz., that the eye on perceiving any one colour acquires a tendency to see its complementary. Thus, to take an example of Chevreul's simultaneous contrast:—If a strip of red and one of yellow paper be viewed side by side, near the line of contact, the red paper inclines to violet, the yellow to green. The rationale of this is at once obvious: the red mingling with the complementary of yellow, i.e., blue, produces the violet tint; whilst the yellow mingling with the complementary of red, i.e., green, produces a light green; and this same law holds good in the juxtaposition of any two colours whatever. By the term successive contrast, Chevreul designates the familiar phenomena of complementary ocular spectra, of which a most comprehensive history has been given by Darwin, in the *Philosophical Transactions*, vol. lxxvi. p. 33, *et seq.* Du Tour⁷ thought that the two eyes cannot perceive each a separate colour at once. He says that if, e.g., a blue disc be presented to one eye and a yellow one to the other, the result is that the mind perceives alternately the one or the other colour, but not the two at once. But I would submit that these two statements do not include the whole facts of the case. I took two tubes, each 10½ inches long, and applying the end of one to each eye, viewed the sky through them. I found that when the contiguous edges of the tubes at their further ends were some inches apart, two distinct white circles of sky were seen; these circles touched when the edges of the tubes were from 2½ to 2⅝ inches apart, and, when closer, the two circles appeared as one. If now the further end of one tube was covered with a piece of green glass, the end of the other with a piece of red, as long as the ends of the tubes were kept not closer than 2½ to 2⅝ inches asunder, the two coloured discs were perceived perfectly distinct from one another; no alternation of either colour to the exclusion of the other, as in Du Tour's experiment, ensued, so long as the tubes were inclined to each at this or any greater degree of divergence.

Another very interesting series of phenomena depending on the intrinsic sensibility of the eye to the impressions of colours are those of coloured shadows. The first exact observations on these were made by Count Rumford.⁸ He observed that the two shadows of an object placed in front of a white ground, from a white and a coloured light, were of the two colours complementary to the latter. I have investigated this fact a little more closely. The method adopted has been to throw a white and a coloured (red) circle of light from two magic lanterns on a white screen, before which a slender wooden rod was placed. It is easy to satisfy ourselves that the red shadow is produced by the (otherwise colourless) shadow cast from the interception of the white light being simply illuminated by the other red light. The green shadow is the shadow produced by the interception of the red light, illuminated by the white light. These coloured shadows have, by Rumford and many subsequent observers, been ascribed to the effect of contrast. But this appears an inadequate explanation; for if, with one magic lantern, a half-white and a half-red circle of light be thrown on a screen, a shadow thrown across the two fields is simply dark, without any colour at all. If, again, a red and a white disc of light be thrown from two magic lanterns respectively on a screen, so as partially to overlap, where the overlapping

³ Brewster's "Optics," 1831, p. 305.

⁴ *Annales de Chimie* for 1833.

⁵ London and Edinburgh *Philosophical Magazine* for May, 1839, p. 395.

⁶ *Op. Cit.* for December, 1839, p. 457.

⁷ "The Principles of Harmony and Contrasts of Colours," by M. E. Chevreul.

⁸ "Mémoires de Mathématiques et de Physique présentés à l'Académie Royale des Sciences," iii. p. 514, iv. p. 499; Paris, 1760-65.

⁹ "Philosophical Papers," by Benjamin, Count of Rumford London: 1802. Vol. I. p. 333.

takes place two complementary shadows of any object are seen, but in the other two parts of the field only one colourless dark shadow is seen.

The following facts seem to form the basis of the explanation of coloured shadows:—Firstly, the experiment of Rumford,⁹ that a piece of gray paper placed next to a piece of coloured paper, both on a black ground, with the exclusion of extraneous light, appears tinged with the complementary colour. Secondly, I found by my own experiments that if, in a dark room, the screen is illuminated with a red circle of light from a magic lantern, the greenness of the shadow and the redness of the ground on which it appears are inversely proportional to one another. By approximating the red light to the screen this becomes redder, whilst the shadow of the rod placed before it becomes less green and darker, till it becomes an ordinary black shadow; that, on the other hand, removing the red light till it leaves the white screen but faintly tinged with red, brings out the green shadow very prominently, and on admission of light into the room, a second faint red shadow comes out.

Meusnier observed, "that when the sun shone through a hole a quarter of an inch in diameter on a red curtain, the image of the luminous spot was green." Another observer, Mr. Smith, of Fochabers,¹⁰ states, "If we hold a narrow strip of white paper vertically, about a foot from the eye, and fix both eyes upon an object at some distance beyond it, so as to see it double, then if we allow the light of the sun, or a light from a candle, to act strongly upon the right eye without affecting the left, which may be easily protected from its influence, the left hand strip of paper will be seen of a bright green colour, and the right hand strip of a red colour."

From all these facts, I think the conclusion arrived at by Sir David Brewster appears highly probable, that "as in acoustics, where every fundamental sound is actually accompanied with its harmonic sound, so in the impressions of light, the sensation of one colour is accompanied by a weaker sensation of its accidental or harmonic colour."¹¹ To this might perhaps be added that there is a tendency in the eye to, as it were, decompose white light into two complementary colours; and, further, that the predominant decomposition is into red and green.

Applying this theory to the phenomena of coloured (e. g., red and green) shadows, the red shadow has already been shown to be simply due to the illumination of a colourless shadow by a red light; whilst, on the whole of the rest of the field of the white screen, the red tint cast from the magic lantern is sufficiently powerful to overcome the green tint which the eye would otherwise perceive, excepting at one spot,—namely, that which does not receive any red light on account of the interposition of the opaque rod. Here the green (harmonic) colour, having no antagonistic red to overcome it, is rendered sensible to the eye.

An Old Cathartic Revived.—The leaves of the *Globularia Alyssum*, which were known in the middle ages as a purgative, have been recently re-introduced by Dr. Planchon, of Montpellier, who claims for them that they are efficient as a purgative, without producing nausea or griping, and that their bitter properties give them a tonic action.

⁹ *Op. Cit.*, p. 336.

¹⁰ Brewster's "Optics," p. 405; *London and Edinburgh Philosophical Magazine* for October, 1832, vol. I. p. 249.

¹¹ Brewster's "Optics," p. 309.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Saturday, March 16, 1861.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE IX.

HAVING considered in our previous Lectures the principal properties of the elements oxygen, hydrogen, nitrogen, and carbon, and of their chief compounds, we are now in a position to make ourselves acquainted with the atmosphere surrounding our globe, and I intend to devote the present Lecture to some statements with regard to this, to us very important, mass of gaseous matter. The atmosphere consists of a mixture of gaseous, vaporous, liquid, and even solid matters. But the two latter may be regarded as abnormal, and, consequently, we may confine ourselves to the gaseous constituents of this envelope of our globe. Just as the ocean may be presumed to contain a little of everything that is soluble, so we may with equal propriety assume that the atmosphere contains a little of everything that is volatile; but the quantities of most of these substances are so infinitesimally small, that they entirely elude all our attempts even to detect them; nevertheless, even such very unærial matters as potash, lime, magnesia, iron, and manganese, have been found in the air in very minute quantities under particular circumstances. Since the experiments of Faraday, which have proved that there is a limit of temperature, below which the less volatile substances do not form vapour, we cannot now assume that these solid bodies to which I have just referred are present in the air in the form of gases or vapours; they must be present there in a solid form, and that such solid bodies really do exist in the atmosphere, and are a long time suspended in the air, there can be no doubt. Every sun-beam that passes through a room shows us an infinite number of these very particles hovering in the air. You know if a sun-beam should pass through an opening in a shutter at one end of a room and out of another opening at another side of the room it ought not to illuminate the apartment at all; but the fact is, that it does illuminate it, and that you see the path of the beam through it. Every time a beam of electric light passes through this theatre you see the track of the light—the direction in which it goes,—by means of these little particles reflecting the light and rendering themselves in this way visible. I think it will not be impossible for me to render visible to you the presence of solid particles in the air—particles of matter which cannot really be volatile, which only exist there, in fact, in a state of mechanical suspension,—by means of this nearly colourless gas flame. If you notice it attentively, you will see that there are little yellow particles in it. These yellow particles are due to the presence of common salt in the atmosphere, the little particles of which entering the flame, are there ignited, and communicate the intense yellow colour which each particle is seen to produce when it passes through the flame. Here we have some chloride of sodium. I will simply rub it in this mortar, and then agitate it a little, in order to throw the particles into the air. I dare say the effect will not take place immediately, but if you watch the flame for some little time after I have agitated this chloride of sodium, you will see that those little yellow particles already floating in the room before we commenced the experiment will be greatly increased in number. There are some of them distinctly visible, although the flame is at a great height above the mortar. This proves that these little particles of chloride of sodium, although so many hundred times heavier than the air, still continue to float in the atmosphere, by virtue of their very minute

division, and consequently we can have even permanently solid matters present in the air, just as such matters as gold, for instance, can be suspended for months, or even for years, in water, without sinking to the bottom. If the gold be finely enough divided, it requires months, sometimes it requires even years, to subside through the space of two or three inches of water.

Our atmosphere, as you are aware, extends to a height of some forty-five miles from the surface of the earth, but such is the varying density of the air that when you have risen to the height of three miles in the atmosphere, you have then about one-half of the total weight of the air below you. At that height you might draw a line through the atmosphere which would contain one-half of the atmosphere below it, and one-half above it. Although we generally regard the air as an exceedingly light material, still its absolute weight is far from inconsiderable. I might show you that even a small flask of it contains enough to be very appreciable to a balance of moderate delicacy. It is 815 times lighter than water, and more than 11,000 times lighter than mercury, still, if you conceive a column of atmospheric air extending from this table upwards to the extreme limits of the atmosphere, and measuring only an inch square, that column of air weighs fifteen pounds. A moderate sized room contains sufficient air to crush a man to death if it could be collected together and placed upon him, and this theatre contains considerably more than one ton of atmospheric air. Therefore, you see, although we almost forget its presence around us, still this gaseous envelope of our globe possesses a very considerable absolute weight.

The composition of the atmosphere has for many years engaged the attention of chemists. The first experiments that were made upon the composition of air after it had been ascertained that it consisted chiefly of two gases, namely, nitrogen and oxygen, led to the inference that its components were liable to very considerable variations. It was found that sometimes there was 18 per cent. of oxygen, at other times 19 per cent., at other times 20, and at another time 24 or 25 per cent. of oxygen; and it was imagined that the salubrity or otherwise of any district depended upon the amount of oxygen contained in the air of that district. But these apparent differences in the composition of the atmosphere were due entirely to the defects of the modes of experiment at that time. The modes of estimating the quantity of nitrogen in the air were so defective as to give rise to errors as serious as those to which I have alluded. When this process of analysis became further perfected, the opposite error was fallen into for a short time, and the indications of this more perfect method showed a complete uniformity in the composition of atmospheric air. Specimens of air taken from crowded cities, from the country, and from great heights, were all perfectly uniform in composition. As the methods of analysis, however, still further improved, the incorrectness of this conclusion was also discovered, and it was found that there are variations in the composition of the atmosphere—variations, it is true, within very small limits, indeed, but still far within the reach of experiment to detect. The extent of these variations, and the composition of the air itself taken from different localities, you will see on one of the diagrams before you. Before referring to that, however, perhaps it will be better that I should direct your attention to this table, containing the normal constituents of air in 100 volumes:—

100 volumes contain	
Oxygen	20.61 volumes
Nitrogen	77.95 "
Carbonic acid	.04 "
Aqueous vapour	1.40 "
Nitric acid, ammonia, and fire-damp	Traces
100.00	

The first four are the only substances in the air which are capable of exact estimation. Attempts have been made to estimate the ammonia, and probably we have come within a very small fraction of the truth; but we may represent the remaining constituents—nitric acid, ammonia, and fire-damp—by the term "traces," which a chemist usually applies to a quantity of matter so small as to be practically incapable of accurate determination by experiment; the quantities of these materials are so infinitesimally small that they bear scarcely any appreciable relation to the other constituents. Now, you will see that, of these four principal constituents two of them, namely, nitrogen and oxygen, constitute nearly the entire bulk of the air; and if you extract from the atmosphere the aqueous vapour and the carbonic acid, then you have left the oxygen and nitrogen in the proportions indicated in this second table, which contains the results of an extensive series of analyses. It shows you the per-centages of oxygen and nitrogen contained in air that has had its carbonic acid and water (or aqueous vapour) removed from it. There are the results of two analyses of air in nearly every case. Here are two analyses of atmospheric air at St. Bartholomew's Hospital; the two lines of figures represent the two different examinations, and show that the errors of analysis fall within a very small variation. You will see also, from an inspection of the table, that there really is a variation in the composition of air when taken from different localities.

Composition of Atmospheric Air from various Localities.

	Oxygen.	Nitrogen.
St. Bartholomew's Hospital	20.885	79.115
	20.999	79.001
Paris	20.913	79.087
	20.999	79.082
Lyons	20.918	79.082
	20.966	79.034
Toulon	20.912	79.088
	20.982	79.018
Berlin	20.908	79.012
	20.998	79.002
Madrid	20.916	79.084
	20.982	79.018
Geneva	20.909	79.091
	20.993	79.007
Montanvert	20.963	79.037
Summit of Pichincha, 16,000 feet	20.949	79.051
	20.988	79.012
North American Prairie	20.910	79.090
South America	20.960	79.040
Liverpool to Vera Cruz	20.918	79.082
	20.965	79.035
12,000 feet above London	20.880	79.120
Manchester	20.876	79.124
	20.888	79.112
Algiers (June 5, 1851)	20.420	79.580
	20.395	79.605
Bay of Bengal (February 1, 1849)	20.460	79.540
	20.450	79.550
Ganges (March 8, 1849)	20.387	79.613
	20.390	79.610
Southern Asia	20.910	79.090
	20.928	79.072
Polar Sea	20.850	79.150
	20.940	79.060
Grands Mulets (1859)	20.815	79.185
	20.787	79.213
Summit of Mont Blanc (1859)	20.962	79.038
	20.963	79.037
Chamounix (1859)	20.905	79.095
	20.882	77.117

Some of these specimens were taken during a voyage from Liverpool to Vera Cruz; and another in a balloon at a height 18,000 feet above London. The specimen taken at Algiers in 1851, may be regarded as furnishing, what we may term, the first abnormal result; it contains, you see, a comparatively small proportion of oxygen. Our averages have been something like 20.9 of oxygen, but this at Algiers contained only 20.420, or, as determined by the second experiment, only 20.395 of oxygen. You see the two experiments agree very nearly, so that there can be no doubt whatever of the correctness of the two analyses. The analysis of the air of the Ganges was taken at the time when a very violent attack of cholera was breaking out. All the other examples give us almost precisely the same average; and there can be but little doubt that air collected at Algiers and in the Bay of Bengal and on the Ganges would, if examined carefully at the present time, be found to possess also the same composition as the other specimens; but the presence of organic matter in these specimens prevented an accurate determination of the oxygen; in fact, it was noted in the case of the air upon the Ganges, that there were organic emanations proceeding from the river; there was a dense vapour over the river and a very perceptible odour of organic matter in the air. Now, what would be the effect of this organic matter? The effect would be this—that in the analysis of the air, the organic matter would diminish the quantity of oxygen as determined by means of combustion with hydrogen which is the best, and in fact the only correct mode of determining the quantity of oxygen contained in the air. The presence of organic matter would have the effect of reducing the apparent amount of oxygen, and thus the abnormal result would be obtained with the same kind of analysis which was performed upon the other samples. They were all analysed by substantially the same process; but in the case of the air containing these abnormal quantities of organic matter, the amount of oxygen indicated by experiment would be smaller than that actually contained in the air at the time; and there is very little doubt in my own mind that the abnormal results of the examination of these three samples of air are due to this cause—the presence of organic matter.

The mode of estimating the oxygen in atmospheric air is very simple,—at least one of the modes of doing it; and I think I shall be able, in the course of a single minute, or little more, to make the experiment before you.

Here is a tube graduated into 100 divisions with decimals. These divisions are not, of course, large enough to be seen at a distance; but I have no doubt we shall see the proportion of the oxygen due to its absorption by the re-agent we shall use to remove it. In all these gaseous analyses it is necessary that the gas, both before and after the application of the re-agent, and before each determination of gaseous volume, should be brought to one uniform temperature and pressure; and this we do in the present instance, as nearly as possible, by immersing the whole apparatus in this vessel of water, which maintains during the whole course of the experiment almost a uniform temperature. Now I think we have got the right quantity of air. The water contained in the lower limb of this endiometer prevents communication between the internal air or gas contained in the tube and the external atmosphere. In reading the volume I bring the two liquids—that is the water outside and that inside the tube—to the same level. Now we have almost exactly 100 volumes of air in this tube. From this liquid in the open limb of the tube I remove a certain quantity by depressing this glass tube open at both ends, and taking out each time a little of the water; and now I am going to pour into it a quantity of caustic potash, which will, of course, have the effect of removing the carbonic acid contained in the air; and we might also, if we could afford the time, determine the amount of carbonic acid contained in this sample of air,

by the agitation of the air with this caustic potash; but the absorption would be so trifling, that it is almost impossible to get the result by an experiment of this kind. It is only the oxygen that can be estimated in this way; the carbonic acid, as you have seen from the table, is so minute in quantity, that we may in fact, in this experiment, disregard it altogether, and assume that the air we are operating upon is free from carbonic acid. I am now going to put into the tube some pyrogallate of potash and pyrogallallic acid, which has the property of absorbing oxygen with very great rapidity. I have mixed these two solutions, and I will agitate them with a quantity of the air in this tube. You will notice that this solution of pyrogallate of potash, which was first colourless, has become of an intensely red colour, and in the course of a few minutes it will, I dare say, become so dark that it will appear to you almost black. This colour is due to the product of oxidation of the pyrogallallic acid, which oxidation takes place at the expense of the oxygen of the air contained in the tube. In this way, by a few minutes' agitation, the whole of the oxygen is removed, and at the same time of course the carbonic acid is removed too; but the latter being in such small quantity we may, as I have said, neglect it altogether. Now, when we allow the liquid to subside here, and you will then observe that there will at once be a rise of the liquid in the tube. [The extremity of the tube was accordingly inserted in the water as described.] You see it has now risen to about this point [pointing out the level of water on the scale of the endiometer]. We have, in fact, absorbed the oxygen; and now if we restore our temperature to its original normal condition by immersing the apparatus in this vessel of water, we shall be able to read the volume of gas, which will be, of course, the original volume of air we took minus the quantity of oxygen which has been absorbed by that pyrogallate of potash. The temperature is now adjusted by immersion in the water, and the tube brought to the same position as at the commencement of the operation. We have now about 20.4; the liquid has risen between 20.4 and 20.5 in this tube. Now, this, for a rough experiment, is sufficiently near perhaps for our purpose. It ought to have risen 20.9 volumes. If we had time to regulate the temperature and the pressure, and to make our observations with the leisure with which one can make them in a laboratory, we should have obtained by this means very nearly the result of exploding atmospheric air with an excess of hydrogen—40.8 or 20.9 per cent. of oxygen.

Now, we have thus demonstrated the presence of oxygen; and the presence of nitrogen we have demonstrated in a previous Lecture, for you will recollect we prepared our nitrogen in one experiment by burning away the oxygen of the air, when we found that the residue was nitrogen. The presence of aqueous vapour, or moisture, in the air is also very easy of demonstration. Here, for instance, we have had a freezing mixture suspended in this room for some length of time, and it has cooled the outside of this vessel considerably below the freezing point; and you see the moisture contained in the air of this room has crystallized as ice, or hoar frost, upon the outside of this cone of tin. If we allow the vessel to stand until the close of the Lecture, I dare say we shall have some few drops of water, collected from the air, dropping from the end of the cone; in fact, we have on the cone already a few drops of water from the condensation of the aqueous vapour of the atmosphere. Most of it is congealed, and is in a solid form. It will require some little time yet for the temperature of the vessel to rise sufficiently high to melt ice. There is, however, another mode here by which we can illustrate the presence of moisture in the air. We will reread a quantity of air which contains moisture, or is nearly saturated with moisture. The temperature of the air under all circum-

stances where it is rarefied, by being pumped out of a receiver in this way, will fall; and air cannot contain so much moisture at a low as at a high temperature. Therefore, if we rarefy the air in the receiver, that air being saturated with moisture will deposit a quantity of moisture in the receiver, on account of the cold produced by the rarefaction; and you will see a cloud within the receiver. This cloud is due to the moisture deposited in the form of very minute drops within the receiver. [The air-pump was worked, and a cloud of moisture was seen within the glass receiver.] You see the glass is now comparatively dim; and there is a kind of cloud or vapour within the receiver. This moisture present in the air is estimated by various instruments which are termed hygrometers. Here is one of them which is that most generally used—Daniell's hygrometer, which consists of two bulbs connected by a glass tube. They contain ether; and as we pour ether upon the outside of one of these bulbs it reduces the temperature even of the other bulb, and the moisture of the atmosphere is deposited upon the black bulb just as it was deposited here upon this tin cone. The operation is watched until that moisture is just beginning to deposit upon the black bulb, and then the temperature of the thermometer in the interior of the bulb is noticed, and we know that the moment when that moisture is deposited upon the bulb, the air at that particular temperature is just saturated with moisture; we further know what amount of moisture is necessary to saturate the air at a particular temperature, as we have already made experiments on this subject. We are thus able to tell from the indications of this instrument the quantity of moisture contained in the air at the time the experiment is made. This table, representing the possible amount of aqueous vapour contained in 100 volumes of atmospheric air at various temperatures, gives us at once that result in per-centage quantities as determined by this instrument.

Temperature F.	Per-centage of Aqueous Vapour	Temperature F.	Per-centage of Aqueous Vapour
0	0.1	47	1.1
10	0.2	50	1.2
20	0.3	52	1.3
25	0.4	54	1.4
29	0.5	56	1.5
32	0.6	58	1.6
35	0.7	59	1.7
38	0.8	60	1.8
41	0.9	70	2.4
44	1.0	80	3.5

If we found, for instance, that we required to reduce the temperature to the zero of Fahrenheit's scale in order to get moisture deposited, we should know then that we had $\frac{1}{10}$ th of a volume present in 100 volumes. If we reduce the temperature to 10° before the moisture was deposited, we should know that we had $\frac{2}{10}$ ths of a volume present in 100 volumes of air. Thus air at these particular temperatures may contain any amount of aqueous vapour below that in the table, but it cannot possibly contain any amount above the quantity stated there. When the temperature falls below these points, then the vapour is deposited and becomes visible in the form of dew, or rain, or some precipitation of that kind.

The presence of carbonic acid in the air is very readily demonstrated by exposing lime-water, our delicate test for this gas, freely to the atmosphere. Here is a vessel containing lime-water which has been exposed for some hours, and you see it is covered with a film of considerable thickness of carbonate of lime, due to the absorption of carbonic acid from the air.

(To be continued.)

NOTICES OF BOOKS.

A Practical Treatise on Coal, Petroleum, and other Distilled Oils. By ABRAHAM GESNER, M.D., F.G.S., Consulting Chemist, Author of "Industrial Resources," "History of New Brunswick," "Geological Reports," &c., &c. New York: Baillière Brothers, 440, Broadway.

It is now some time since we noticed a kindred work by Dr. Antisell—kindred not only in subject but in treatment. Certainly Dr. Gesner's book has the advantage of being correct so long as its author speaks of what he knows. Dr. Gesner is doubtless a good practical coal distiller, but, scientifically speaking, a poor chemist. We doubt not that he has stood by and directed many hundreds of distillations, and that he has prepared thousands of gallons of his favourite kerosene. Why, then, does he not write solely on practical subjects? and why does he injure his reputation, and bring ridicule upon his book, by an affectation of knowledge upon points where his information is most limited. Dr. Gesner, it is true, surpasses Antisell in his practical knowledge, but, unhappily, he also surpasses him in his blunders. In fact, it would be difficult for any good student of a chemical class to accumulate so many in so short a space. Dr. Gesner's great source of scientific information is the late lamented Dr. Gregory's excellent "Handbook of Organic Chemistry." He has not even obtained the last edition, but has rested satisfied with that of 1852. But, in order to give a fair outline of his book, we will discuss some of his chapters *seriatim*.

Chapter I. is chiefly occupied with a sketch of the history of coal distilling. It is exceedingly brief, but perhaps sufficient, and what is there is written with candour. The descriptions of the varieties of coal, and the results of their analysis, are also extremely brief. The first glaring blunder which attracted our notice is on page 14. The Author, describing the metamorphosis undergone by plants during their conversion into coal, says:—"The changes that have taken place in the original plants during their passage from woody fibre into coal are ascribed to the evolution of a part of their carbon, hydrogen, and oxygen, as there is less of those elements in the coal than in wood." The Author, instead of writing "those elements," should have said less *hydrogen and oxygen*, because coal, as everyone knows, contains far more carbon than wood. A ridiculous passage also occurs on page 16, whether originally due to the printer or Author we cannot tell. Certainly, however, the Author is responsible for permitting such mistakes to pass in the press proofs. Alluding to the pyrogenous derivatives of organic acids, he says:—"Organic acids distilled with strong bases part with the elements of carbonic acid, which uniting with the base and the acid, minus the carbonic acid, comes over in the form of a new product." The passage is in every way inadequate to tell the true story of the formation of ketones, &c. An approach to sense might, however, have been made by writing and punctuating the passage thus:—"Organic acids distilled with strong bases, part with the elements of carbonic acid, which unites with the base; and the acid, minus the carbonic acid, comes over in the form of a new product."

Chapter II. (on the materials employed for the production of oils) is valuable to the coal and tar distiller, and contains much practical information. It is interesting from its allusions to the oil wells which are now attracting so much attention in America.

Chapter III. is on the nature of the products obtained from bituminous substances, and on the apparatus used in distilling. Several stills and ovens are described with their condensers, &c., and the descriptions are practical and tolerably well up to the present state of coal and tar distilling.

Chapter IV. is devoted to the products of the distillation

of wood, coals, asphaltum, bitumen, petroleum, and other substances capable of yielding oils. The Author, like so many of his brother writers on chemistry in America, cannot spell. Capnomor with him is copnomor; pittacal is pittical; aniline is analine; ampeline is ampaline; rosolic is rosalic; carbazotic is carbozotic; St. Evre is St. Evrre; margarie is margonic; basic acid is made into the paradox basic acid. (See pages 36, 58, 59, 60, 62, 64, 68, 69, 75, 79, 80, &c.) The proof that the errors alluded to are not misprints, is found in the fact of their continual repetition. The Author, in his wretched description of eupion, states that "With nitric acid it forms several new combinations analogous to those of benzole," and that it "Does not exist ready formed in the tars, but is produced by the action of strong acids and alkalies upon the distillates of crude oils." Both these statements are hopelessly incorrect. The little that the Author writes about eupion that is his own, is incorrect, and what he borrows from Dr. Gregory, which includes the chief portion, is taken from an old edition of his work published before the true nature of eupion was made out.

The Author is, we need scarcely say, entirely unacquainted with the modern researches on the nature of the eupion, &c., from Boghead coal.

The Author is fond of dragging in quotations from Dr. Gregory about homologous series, and yet that he is entirely ignorant of their true value in science, is evident from the fact that his lists of homologous bases and hydrocarbons are never correctly written. In a list of "The oily substances obtained by the distillation of coal-tar," cumole is put before toluole; and, among other errors in the list of bases, parvoline is put before lutidine, and the formula of the latter is written C_9H_7N , instead of $C^{14}H^9N$; moreover, pyridine, the first and principal base of the series, is altogether omitted. The same remark applies to lepidine and cryptidine. The Author's judgment is scarcely shown by leaving his tables in the state indicated, and instantly rushing, in the most uncalled-for manner, into an enumeration of the chlorinated and brominated substitution derivatives of naphthaline and aniline, which, with great presumption, considering his evident extreme ignorance of the subject he calls "uncertain combinations."—P. 63.

At page 64 carboxotic acid (*sic*) is one moment called Welter's bitter, and the next, the discovery of it is attributed to M. Guinon, of Lyons, and its formula is given $C_{12}H_2N.O_4O_2$! Would it be believed that after these painful displays of ignorance the Author devotes his next Chapter (V.) to an account of homologous series, with a sketch, copied from Regnault, of the so-called law of atomic weights, being multiples of that of hydrogen; immediately after which, he drags in Dr. Gregory's excellent sketch of the theory of types, but as he commences with the words, "A series of types has therefore been discovered," becomes absolute nonsense, following as it does the quotation from Regnault. It would be of no use to pursue the subject further. Dr. Gesner is probably a very good tar distiller, and may make excellent kerosene (or whatever he chooses to call the fluid known in this country as photogene or paraffine oil); but he is so deficient in his organic chemistry that the best advice we can give him is, to purchase the last edition of his favourite work, and read it carefully before attempting to teach others. It is only fair to mention that the descriptions of the methods of preparing photogene are tolerably clear and practical.

NOTICES OF PATENTS.

Obtaining Pulp for the Manufacture of Paper from Indian Corn. H. DOFFENNES, Brussels. (Not proceeded with.)
For the production of a pulp suitable for paper-making, the inventor subjects the waste portions of maize, or Indian

corn to certain mechanical processes for the purpose of disintegrating the cellular tissue, and to an after chemical treatment, whereby the plant is divested of small quantities of colouring and extractive matters. The vegetable cellulose prepared in this manner is made to furnish a material like pulp, ready for conversion into paper by the ordinary processes of manufacture. The specification directs that the maize stalks and heads, and in the case of damaged corn, the grain also, be chopped into small pieces in a chaff-cutting machine, then threshed, to remove the sand and earthy impurities. The matters thus purified are washed and pulped in any suitable apparatus, first with water alone and then with soda, again washed with water, and, finally, bleached by the action of chlorine. The white pulp ultimately produced is said to furnish excellent paper.

The failure of this British patent may very possibly be attributable to the want of raw material. In the South of Europe, where maize is more abundantly cultivated, the process would, doubtless, be capable of great results. The modes of treatment, both chemical and mechanical, appear well adapted to the production of a serviceable pulp from maize if only the supply could be obtained.

Manufacture of Colouring Matters and Pigments from Coal-oil, Raw Naphthaline, and from the Waste Lime from Gas-works. L. C. E. VIAL, Paris. (Not proceeded with.)

THIS invention relates to the production of a red colouring matter from crude naphthaline or coal-tar oil, under the combined influence of an alkaline base and of atmospheric oxygen. To purify it from certain associated matters, the coloured principle is dissolved out by treatment with carbonate of soda, and re-precipitated from this solution by acetic or a cheaper mineral acid. The specification describes also a process for obtaining a green or bluish colour from the lime waste which has been employed for the purification of gas. It consists merely in a simple treatment with nitric acid, when the material is precipitated as a deep green or bluish colour, which requires only to be washed to render it fit for use.

Apart from the consideration of expense incident to all processes wherein large amounts of nitric acid would have to be employed, a manufacture based upon the action of acid on products so innately offensive as the "blue Billy" of the gas-works would require to be conducted in remote localities; and, even this point overcome, there will be, according to the showing of the specification, uncertainty in regard to the tint of colour ultimately produced.

CORRESPONDENCE.

Dyeing Magenta.

To the Editor of the CHEMICAL NEWS.

SIR,—Perhaps you will do me the favour to insert the following reply to "Magenta," *alias* "Mares-nest," and as I do not intend to trouble you again on this subject, I hope this will at once remove unfavourable impressions from the minds of gentlemen who manufacture articles which fail to meet with that general approval from practical men which was anticipated, and hope such gentlemen will in future search into the practical objections before publishing to the world assertions that dyers object to use an article simply because it is given to them in a powder.

It is a well-known fact that dyers have suffered, both in pocket and character, by repeated attempts to use the powder, and such other solutions as could be obtained, and after obtaining results with them quite unsatisfactory, they have been compelled to keep goods weeks, instead of days, at no small inconvenience, till they could obtain

some of the well-known makes of solutions, which at one time commanded almost fabulous prices.

I will now lay before you the method most approved by practical men to produce a magenta on Cobourgs, Orleans, &c. After the goods have been properly scoured to remove all foreign matter, the hot-bath is prepared by adding sufficient colouring to produce the colour required, the quantity of hot water being regulated by the amount of cloth to be dyed. The goods are then worked in the bath at about 180 F., till the colour has got even upon the cloth. The cloth is then taken out and washed in cold water, when it will be found that the worsted only has received the dye, the cotton still remaining white, or having but a very slight tint upon it. The cotton is then prepared to receive the colour, by working the goods in a cold solution of galls or sumac, the former being preferable, after which they are worked in a cold dilute solution of neutral chloride of tin. When sumac is used those processes give a buff tint to the cotton, which is objectionable, and can be removed by working the goods in a cold solution of oxalic acid, which will not deteriorate the mordant. The goods are well washed in cold water after the chloride of tin, and also after the oxalic acid. When that article is used the cotton has now received a mordant which enables it to take up the colour, either hot or cold, but experience has proved that the colour is brighter when the cotton is dyed cold; and, consequently, it must be dyed last. The only thing requisite is to add colouring sufficient to a proper quantity of cold water, and work the goods in it till the colour is even upon the cloth, after which they are washed in cold water, then dyed, and finished.

The objections to the colour supplied in powder in the above processes are, that in dyeing worsted a duller colour is produced, and there is an additional objection, which presents itself in the cold process, which bath is not emptied for weeks together, and when the colour is dissolved from the powder by boiling water, and the solution added to the cold bath, it immediately begins to precipitate, from the fact of there being no solvent present to hold the colour in solution. (As I stated before, hot water will hold the colour in solution, but when the aqueous solution becomes cold, it will precipitate.) The sides of the vessel receive a coating of the precipitate, and then the goods come in contact with it, when, in addition to being a bad or inferior colour, they are all stained with the precipitate. There are other objections, but the above will show that dyers are more willing to give things a fair trial, than some manufacturers are to admit their errors.—I am, &c.

JAMES SHARP.

Leeds.

MISCELLANEOUS.

Emery.—The trade in emery is not, as formerly, monopolised by the Greek Government, which now levies a tax of 5 drachms per hundred weight when exported. Other hard minerals are sometimes fraudulently substituted for it, to detect which, and to determine the value of the emery, the following method is employed in Smyrna:—A plate of glass of known weight is rubbed with a certain quantity of the emery until it has no further effect; the lighter the glass has become, the better is the quality of the emery. The emery diggings of the Government are on the island of Naxos. A ferruginous clay silicate has lately been discovered upon the island of Skyno, which is not inferior in hardness to emery.—*Wittst. V. Schr.* ix. 535.

Commercial Products of the Asphodel.—Every one is familiar with the pretty, lovely-looking, white-flowered asphodel of our gardens. In the South of Europe, and apparently on both sides of the basin of the

Mediterranean, the plant (*Asphodelus ramosus*) is extremely abundant; though it has never, so far as I know, been turned to any account, except that in times of scarcity its acrid fasciculated roots, after much coiling, have been eaten by the poor. In the Paris Exhibition of 1855 there were shown bottles of alcohol extracted from the asphodel; specimens of the residuum of the roots after being twice distilled; paper stuff from the stalks and leaves of asphodel—card-paper, cards, paper and writing-papers, of various qualities, manufactured from the same, and mixed in various proportions with rags and common paper stuff. M. de la Bertoche in a pamphlet, asserts that asphodel roots contain upwards of 27 per cent. of alcoholic principle, or more than double the quantity which resides in the root of beet. The stalks and leaves contain a remarkably tenacious fibre, fine, strong, and flexible. The distillation of asphodel root has been already pursued, and with considerable success, in Algeria; but the immense abundance of the plant in Tuscany, where it has hitherto been considered only a pernicious and most ineradicable weed, points to the advantage of endeavouring to turn it to account. The fasciculated roots, after cleansing and crushing, are mixed with water, and the fluid is exposed to heat, so as to facilitate fermentation. The alcohol which it yields is pure and colourless, perfectly transparent, and has the colour of alcohol itself. It contains neither acid salt, nor oily matter; it burns without leaving any residue, and the flame is remarkably bright. But at the present time, when material for paper seems likely to fail, a most important succedaneum is afforded by the remains of asphodel. It is undeniable that the residuum of the roots after distillation, together with the other parts of the plant, are eminently adapted to this object. Three processes are necessary; the separation of the useful portions,—the bleaching,—and the reducing the substances into a homogeneous and tenacious pulp. The first is better effected by crushing than by grinding, as the latter mode is apt to destroy the fibre. The second operation involves most difficulty, as the root is covered with a skin which contains a tanning principle; and it is necessary, unless the expensive mode of hand-picking the root be adopted, to expose the substance to air and light, aided by immersion in diluted chlorine, which brings the substance to a very pale brown tint, which is not objectionable for many sorts of paper. For the third process, that of reducing the whole mass to a smooth and tenacious paste, the paper manufacturers must supply the details.—*Sir W. J. Hooker, K.H., F.R.S., in the Technologist.*

ANSWERS TO CORRESPONDENTS.

. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

. All *Editorial Communications* are to be addressed to the EDITOR; and *Advertisements and Business Communications* to the PUBLISHER at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

Permanent Marking Ink.—Indian ink, or the finest lamp-black, with a solution of shellac in borax gives an ink which is not affected by chlorine or cyanide of potassium, or by acids. No nitrate of silver is necessary.

Varnish for Iron.—The best varnish for iron chains is made of pure linseed oil well boiled and rendered quick drying by adding some litharge—about four ounces to the gallon. Colour it black by adding some lamp-black and a little asphalt. When resin is added to oil it renders it more liable to scale off. The chains should be thoroughly dried in a hot room before they are used.

Pigmy.—The subject is one which has occupied the attention of scientific men for some years. It has, however, never yet been really accomplished.

F. R. G.—In September next.

A Victim.—Your case is hard, but you would gain nothing by taking proceedings. What is the use of winning an action against a pauper? He could not pay any expenses.

THE CHEMICAL NEWS.

VOL. IV. No. 87.—August 3, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Estimation of Commercial Saltpetres, by M. J. PERSOZ.

THE best method for estimating the water is to melt carefully from 50 to 200 grammes of nitre in a platinum capsule, taking care that the temperature is not raised much above the fusing point. Weigh the crucible immediately after cooling. If the saltpetre contains nitrate of lime or magnesia add about 1 gramme of well-dried neutral chromate of potash, which prevents loss of nitric acid from these nitrates, so easily decomposed by heat.

To estimate insoluble matters, treat the melted mass with water so as to form a determinate volume of solution N. Filter this carefully; collect, wash, and weigh the insoluble precipitate.

Gay-Lussac's method employed for the estimation of chlorides is to use two standard solutions, one containing 27 grammes and the other 2.7 grammes of pure silver to the litre. Each cubic centimetre of the stronger solution corresponds to 0.01466 grammes of chloride of sodium or 0.01864 grammes of chloride of potassium.

To estimate sulphates, a standard solution of chloride of barium is used, containing 259.8 grammes of salt, each cubic centimetre representing 0.179 grammes of sulphate of soda, or 0.208 grammes of sulphate of potassa. The operation is conducted in the following manner:—Measure 200 cubic centimetres of the solution N of nitre; after adding a few drops of acid, introduce it into a platinum capsule and boil it freely; then carefully pour in a slight excess of standard solution. At this point of the process fill a graduated burette with the solution N, and add it, little by little, until the excess of barytic standard solution is exactly precipitated. This last operation is somewhat tedious and difficult, because the solutions do not readily become clear, and it is often necessary to filter small trial quantities. The relation between the total volume of N employed and the volume of barytic standard solution gives the proportion of sulphate in a given weight of nitre.

Nitric acid is estimated in a very elegant manner, by using dry bichromate of potash, which expels nitric acid from the alkaline nitrates without decomposing the chlorides. The operation is thus conducted:—On a lump of saltpetre, melted and cooled, weighing from 2 to 5 grammes, and placed in a small but sufficiently large platinum crucible, place about twice its weight of previously melted and pulverised bichromate of potash. Weigh, and then carefully heat the whole. At first the reaction is somewhat energetic, and it frequently happens that considerable quantities of the mixture are carried off and are condensed on the inner surface of the cover, which should be shaped like a deep capsule, so that the

fusible matter projected against it may fall in the crucible. In proportion as the nitrous vapours diminish raise the temperature to a dull red heat, taking care to equalise the temperature of the cover by directing a gas jet on it. When the operation is finished leave the crucible uncovered for a few instants, that the air in it may be renewed; allow it to cool, and weigh it. The loss of weight indicates the quantity of nitric acid displaced, from which may be calculated the corresponding proportion of nitrate of potash or soda.

If the nitrate analysed contains at the same time potash and soda an excess will be found by converting the estimated nitric acid into nitrate of potash; and, on the contrary, a deficiency, by converting the acid into nitrate of soda. This excess or deficiency allows of the calculation of the relative proportion of the two alkaline nitrates in the salt analysed.—*Annales du Conservatoire des Arts et Metiers.*

On the Preparation of an Ethereal Solution of Binoxide of Hydrogen.

THIS has been recommended by Dr. H. Storer as a test for chromic acid. Professor R. Böttger proposes the following method for preparing a solution which will keep unaltered for a long time. A piece of sodium, of the size of a hazel-nut, and freed from petroleum, is heated in a rather flat, somewhat inclined porcelain capsule, over a Bunsen gas burner, to fusion, so that it ignites when stirred in contact with the air by means of an iron rod. As soon as the burning and glowing ceases, it is transferred to a well-stoppered vial. Of this impure binoxide of sodium, small particles are carried into about an ounce of very dilute sulphuric acid (1 to 24 water) contained in a narrow cylinder which is kept cool. After the third or fourth introduction, ether is added, the liquids agitated, and after separation the ether removed; this may be continued until the ethereal solution ceases to produce an intensely blue colour with a diluted solution of chromic acid. With little material, a considerable quantity of this new reagent is produced in a short time.—*Wittst. V. Schr.* ix. 546.

On the Employment of Hypochloric Acid as an Oxidiser and Solvent, by M. BRUNNER.

ORES, attacked with considerable difficulty, are easily oxidised by nascent hypochloric acid—for instance, chrome iron, sulphide of molybdenum, uranium ore, &c. M. Brunner recommends mixing the ores, reduced to an impalpable powder, with eight times their weight of chlorate of potash, placing the mixture in a flat-bottomed capsule, and pouring into it sulphuric acid diluted with one-half to once and even twice its volume of quite cold water.

The more easily attacked the ore, the more should the

sulphuric acid be diluted. Acid with half its volume of water may be used for chrome iron. It requires stirring from time to time without the application of heat, and not till the expiration of twenty-four hours can the operation be terminated by the application of a gentle heat. 15 cubic centimetres of sulphuric acid are sufficient for 1 gramme of chrome ore, and it should be added in two portions. Crystals of chromic acid gradually appear in the mixture.

By avoiding raising the temperature, and the use of an acid too concentrated, there need be no fear of explosions, in which there is really no danger, but which may occasion loss of material.—*Repertoire de Chimie.*

TECHNICAL CHEMISTRY.

On Aluminium Bronze, by BARNARD S. PROCTOR.

THE chemical relations of aluminium and the mechanical properties of aluminium bronze have had frequent notice, and may be said to have been fairly introduced to the notice of those who are likely to make use of either material in the arts or manufactures; but the question of how far the chemical affinities of the copper are modified by its union with aluminium appears to have had less attention than it deserves.

The following simple experiments comparing the relative powers of aluminium bronze and ordinary brass to withstand the action of various agents were designed to point out how far the new material could offer advantages over brass for the construction of apparatus, philosophical instruments, or domestic utensils:—

BRONZE FILINGS.

BRASS FILINGS.

No. 1. *With Nitric Acid, 1.42.*

Acted freely, not violently. Acted violently.

No. 2. *Dilute Nitric Acid, P. L.*

No evident action while cold, acted freely while boiling, but ceased again on cooling. A feeble action while cold, a free action while hot boiling, but reduced again on cooling.

No. 3. *Dilute Hydrochloric Acid, P. L.*

No evident action, either cold or hot. The acid being boiled for a minute upon the filings and decanted gave no indication with ammonia, but with sulphuret of ammonia, on standing a few hours, there was evident dark precipitate. In every particular the same as the bronze.

No. 4. *Dilute Nitric and Hydrochloric Acids Mixed.*

Slowly but completely dissolved. Dissolved a little more quickly.

No. 5. *Strong Hydrochloric Acid.*

No evident action while cold, and only feeble on boiling, the colour produced with ammonia being just visible, and with the sulphuret clearly evident. Similar to the bronze, but the action a little more evident.

No. 6. *Dilute Sulphuric Acid.*

With a few seconds' boiling, did not give indication with ammonia, but coloured strongly with the sulphuret. Acted the same as the bronze, but a little more strongly.

No. 7. *Acetic Acid, P. L. Strength.*

A few filings in the bottom of the tube containing the acid were considerably acted upon in the course of fourteen hours, the liquor being dark green, and many small green crystals upon the sides of the tube, apparently from the action of the vapour upon the filings which had adhered to the glass. Not evidently more acted upon than the bronze.

No. 8. *Solution of Ammonia, '960 specific gravity, in the bottom of a tube.*

The action of air, moisture, and ammonia for twenty-four hours dissolved the few filings which had been placed in the tube, producing a blue liquor and white precipitate. Similar to the bronze, but without the precipitate.

A piece of bronze cut from the ingot, hammered flat, filed to a smooth surface, and finished with fine emery-paper, and a piece of brass tube treated in the same manner were subjected to the following experiments, being cleaned with emery-paper between each:—

No. 9. The metals were wrapped in paper and placed in a jar of sesquicarbonate of ammonia. After a few hours the brass was black, and the paper stained blue; the bronze did not seem to have been acted upon. After twenty-four hours a slight action was perceptible upon the bronze, there being a few blue specks upon the metal and blue spots upon the wrapper, but the general surface of the metal still clean. After several days, the spots had increased, but the general surface was scarcely acted upon.

No. 10. Wrapped in paper and placed in a jar of sulphuret of potassium, were not tarnished perceptibly in twenty-four hours. On being wetted and returned to the jar, they were both tarnished, but the bronze less than the brass.

No. 11. Sprinkled with carbonate of potash, which was allowed to deliquesce upon their surface, in two hours the brass was evidently tarnished; in twelve hours considerably tarnished. The bronze unaltered after twenty-four hours.

No. 12. Sprinkled with acetate of potash, in two hours brass was evidently acted upon; in twelve hours considerably tarnished, the spots where the salt had deliquesced being almost black. In twenty-four hours the bronze had suffered nothing, its surface being clean and simply moist with the deliquesced salt.

No. 13. Half covered with a weak solution of tartaric acid, both became gradually tarnished, and in forty-eight hours were covered with thin, adhering crusts, white on the bronze, black on the brass. The crusts did not wash off nor rub off, except with hard friction. The water with which the acid had been washed off gave distinct but not copious blue with ammonia.

No. 14. As No. 13, but with citric acid. Both were tarnished, but much less so than in the last. The coating upon the bronze was thin, green, and tarnish-like. The washings gave with ammonia much stronger indications than No. 13, showing that the discoloration of the surface is not a fair indication of the quantity of metal dissolved.

No. 15. Sprinkled with cream of tartar and water

and allowed to dry. The tartarate upon both had become greenish, and the surface tarnished, but less upon the bronze than the brass.

No. 16. Treated as No. 15, but with binoxalate of potassa. The action similar but stronger. The bronze again was least acted upon.

No. 17. Exposed to a moist atmosphere containing ammonia. Eight hours: the brass was much blackened, the bronze slightly tarnished; twenty-four hours: the corrosion of the brass had gone on rapidly, its surface being black, rough, and covered with small drops of blue liquid; the bronze was dull, but not much discoloured, and there were few small specks of blue liquid upon its surface.

No. 18. Exposed to the fumes of a solution of hydro-sulphuret of ammonia, a drop of the solution also being placed upon the metals. In five minutes the brass was considerably acted upon by the solution, but scarcely at all by the vapour. The bronze was almost instantly tarnished by the vapour, but very little acted upon by the solution. In half-an-hour the brass was tarnished yellow over its general surface, and the edges were black. The action on the bronze did not seem to have increased.

No. 19. Exposed to moist air containing sulphuretted hydrogen. The bronze was almost instantly discoloured, but the brass scarcely visibly so. After remaining a few days under this action the metals appeared acted on to much the same extent, but the brass had an orange coloured tarnish, spotted with black; the bronze a greenish-grey tarnish, likewise spotted with black. (Compare Experiment No. 10.)

No. 20. Exposed to a moist air containing chlorine. Eight hours produced a very slight tarnish upon both. The quantity of chlorine being considerably increased so as to smell pungent, the action became considerable; in the course of a few hours a green crust was formed upon both, which, when scraped off left the brass with a dirty surface, but the bronze almost clean.

No. 21. Exposed to the fumes of nitric acid, specific gravity 1.42, eight hours. Both metals were acted upon and moist, the brass being black, the bronze not much discoloured, but having a loose precipitate which easily wiped off its surface.

No. 22. Exposed to the fumes of hydrochloric acid, specific gravity 1.16, one hour. The brass was growing black at the edges, and its whole surface somewhat dull; the bronze almost equally dull, but not black at the edges. Thirty-six hours: brass covered with a strongly adhering, smooth black crust, upon which was standing a considerable quantity of green solution containing green crystals; the bronze was covered with a pale slate-grey precipitate, moist, and easily rubbed off, leaving the surface perceptibly roughened. Examined with a one-inch lens, it had the appearance of crystalline structure. The brass, after cleaning with ammonia and sulphuric acid, exhibited a surface somewhat less roughened and not crystalline.

No. 23. Exposed to the action of moist air containing sulphurous acid. The change was slow; eight hours rendered the surfaces dull, that of the bronze somewhat copper-coloured. The quantity of sulphurous acid being increased so as to smell pungent, the brass soon acquired a thin but quite perceptible film upon its centre. In fourteen hours the brass was dark green-brown, and coated with crystals; the bronze of a copper-brown, with crystals and solution by deliquescence. Washing with water removed most part of the film from both. Under a one-inch lens the brass was distinctly pitted;

the bronze was pitted much more minutely, being scarcely distinguishable with one inch focus.

No. 24. Exposed to the vapour of iodine at 70° F. Both were acted upon about equally quickly. After eight hours the bronze had a grey-white film, which easily rubbed off, leaving the surface little altered; the brass had a grey-black film, part of which rubbed off readily, and left the surface bright but black.

No. 25. Exposed to the action of salt water. Twelve hours produced a little discoloration of the brass, but no visible change upon the bronze. Twenty-four hours: the salt having crystallised upon the metals, the bronze was slightly dull after washing with water; the brass dull grey, tarnished, the metallic lustre being slightly visible through the coating.

No. 26. Exposed to vinegar, which was allowed slowly to evaporate upon them. The brass was much discoloured, and had green crystals on its surface; the bronze very little discoloured, but slightly more crystals upon it. Under the microscope the brass seemed most uniformly acted upon, there being a general roughness over its surface. The bronze in places was corrugated, as if from an indistinct crystalline texture.

No. 27. Exposed to ordinary coal-gas within the gas-fittings, where a slow current passed over them for eight or ten hours per day, the sample remaining in a stationary charge of gas during the remainder of the twenty-four hours. One week produced scarcely any change; two weeks, the general surface of the metals was tarnished, and the brass strongly discoloured at the edges.

No. 28. The metals, after being burnished, were exposed to the air in the shop (Pharmaceutical). Five days: bronze had an orange-yellow tarnish; brass a greenish-grey tarnish about equal in depth.

No. 29. Burnished and exposed to the weather on the roof of the house. Five days: rain, wind, and sunshine repeatedly alternated. Bronze little tarnished, and little spotted; the brass was considerably more tarnished, spotted, had a black discoloration round the edge, and was more difficult to clean.

No. 30. A repetition of No. 28, the surfaces instead of being burnished were cleaned with fine powdered pumice and oxalic acid. Results the same as No. 28.

No. 31. A repetition of No. 29, with cleaned instead of burnished surfaces, gave similar results—if anything, still more strongly in favour of the bronze.

From the above experiments, it appears that the bronze has a little advantage over ordinary brass in its power of withstanding corrosion, and that its surface, when tarnished, is more readily cleansed. This should give it a general preference where cost of material is not an important consideration, especially if strength, lightness, or durability is at the same time desirable. It would be out of my province to say anything with regard to its fitness for delicate machinery, such as clocks and watches, except that its chemical examination has revealed nothing which can detract from the preference which its mechanical superiority should give it.

Being so much less acted upon by ammoniacal salts and coal gas (see Experiments 9, 17, 18), it appears to offer advantages for the construction of gas-metres, stop-cocks, and moveable joints in gas-fittings.

Its withstanding the action of some deliquescent salts (see Experiments 11, 12), suggests its suitability for chemists' scales and weights, scoops, &c. Its resistance to the action of the weather, and the ease with which the tarnish is removed, renders it specially applicable for door-plates, bell-handles, &c. Its mechanical strength

and chemical inactivity together recommend it for hinges, &c., which are to be exposed to the weather.

In Experiments 18, 22, 27, 29, the tendency of brass to corrosion at its edges, and at any roughness upon its surface, will be observed, and that the bronze is free from this defect.

In several cases (e.g. Nos. 18 and 19), the bronze seemed to be most quickly covered with a slight tarnish, which did not increase perceptibly, probably the tarnish acting as a protection to the metal; but the brass, though less rapidly discoloured, continued to be corroded, apparently with increased speed as the action was continued.

In Experiments No. 20, 21, 22, 29, we find the bronze to be much more easily cleaned, a circumstance of considerable importance in its application to many purposes.

For culinary vessels its superiority to the materials now in use appears questionable.

Drawing instruments, pneumatic apparatus, stage fittings for microscopes, telescope tubes, and various other philosophical instruments are among the purposes for which the use of the bronze appears advantageous.

Undoubtedly, the greatest obstacle to its extensive application is its high price, resulting partly, it is said, from the great difficulty of procuring copper in a state of sufficient purity for the production of a good bronze, the presence of a small portion of iron, &c., being very prejudicial. But surely, the talent which has removed aluminium from its position among the rare minerals of the cabinet to an article of commerce, which has already reduced its price from 60*l.* to 6*s.* per lb., will not be long in devising the means of producing both aluminium and pure copper at a still more moderate price.

11, Grey Street, Newcastle-upon-Tyne.

P.S.—With a home-made pen of aluminium-bronze I add a postscript to my article, to suggest that this material is well worthy the attention of steel-pen makers. From two days' work with this badly-constructed pen, I anticipate that the material will, when well worked, be found to possess both chemical and mechanical properties to recommend it for this purpose.

*On the Best Form for Chimneys, by L. C. LEVOIR,
Phil. Nat. Doct., Leyden University.*

It is a long discussed question, whether chimneys should be made conical with the largest base beneath, cylindrical, or conical with the largest opening at the outlet in the air. Some time ago I tried an experiment which apparently showed that the last form was the best one.

When two adjacent gas-flames are caused to burn from the same tube, the outlets being equal, and about a quarter of an inch wide, and the pressure very low, the flames have the same length when placed in the same horizontal level. But as soon as one is placed higher than the other that one becomes taller by the diminution of pressure higher in the atmosphere.

By putting a conical tube of about three feet in length on one of the flames, when they are burning equal, the greatest suction results when the conical tube is placed with the largest end upwards. The greater suction is not resulting from the higher temperature which the smaller end of the tube assumes, owing to its being placed so much nearer to the flame, for a stream

of cold water flowing round the tube does not occasion a greater length of the flame on which the conical tube is not adapted.

This experiment is a confirmation of a fact which several engineers have long since observed. The reason why so many chimneys are still built cylindrical, or conical, with the widest section beneath, seems to me to be that the influence of the winds—especially those occurring in the daytime, which seem to have a less horizontal direction than those at night—interferes more with the flowing out of the products of combustion as the outlet of the chimneys become wider. Suitable covering head-pieces, to turn with the wind, would highly improve the suction in conical chimneys, and also prevent the smoke from blowing down in so inconvenient a manner.

PHARMACY, TOXICOLOGY, &c.

On the Use of Phenic Acid for Disinfecting Purposes, and its Mode of Action, by M. J. LEMAIRE.

SINCE MM. Corne and Demeaux's paper on "Coal-tar" was presented to the Academy, M. Dumas has stated that the disinfecting properties of this body are due to phenic acid. M. Lemaire's experiments prove this opinion correct. He has witnessed the efficacy of phenic acid dissolved or made into emulsion in the preservation of animal matters. Phenic acid made into an emulsion and injected into the bodies of animals will preserve them unaltered in contact with the air; a human adult body can be preserved with less than 50 centimes.

The author has already indicated an important application of phenic acid to the destruction of parasites. An aqueous solution of 1 per cent. of this acid, and 40 per cent. of acetic acid removes tinea in 30 or 40 days, and cures itch speedily. Acetic acid is added to the preparation, to enable the medicament to penetrate the epidermis, so as to reach the bulbs of the hair. I am of opinion that the same solution would serve to destroy domestic insects; the powders used for this purpose are effective, but the solution would penetrate better into the crevices of furniture.

*On Medicinal Aconitine and its Substitutes,
by WILLIAM PROCTER, jun.*

THE high price and variable quality of aconitine has rendered its use as a medicinal agent so expensive and uncertain, that many physicians never employ it, depending upon the stronger tinctures of the root, in cases requiring the external use of aconite. In the manufacture of organic chemical products very much is added to their cost by the complications and loss rendered necessary or unavoidable in their purification from colouring matter, or strongly adherent resinous or other inert substances in minute quantity, which, while their presence impairs the market value of the chemicals, often do not greatly reduce their medicinal power. In asking the attention of pharmacutists to the following modification of Headland's process for aconitine, it is with the view of furnishing them with a practicable means of supplying their own wants in regard to this potent alkaloid.

It is proper to premise that aconite root contains a

¹ It will give us much pleasure to receive further communications from this Correspondent.—Ed. C. N.

green fixed oil, solid below 70° Fahr., which it is important to remove entirely from the solution, before attempting to extract the alkaloid by the agency of ether, a precaution only partially carried out in the published process of Dr. Headland. When a tincture of aconite root in alcohol of sp. gr. .835, whether prepared in the cold by percolation or by digestion at the temperature of boiling alcohol, as recommended by Headland, is evaporated to one-half the weight of the root treated, a quantity of the green fatty oil above noticed separates and floats upon the surface of the liquid. Most of this may be strained out, if the temperature is below 70° Fahr., but a portion, together with some resin, remains intimately combined in the clear liquid, and it is this which is not removed previously to adding the ammonia, in the process of Headland. Further, aconitic acid is soluble in ether, and aconite of ammonia may be slightly so, in which case it also would tend to contaminate the aconitine in that process.

Take of aconite root, in fine powder, five pounds; alcohol .835, ether, and strong solution of ammonia, of each a sufficient quantity. Moisten the aconite root with two pints of alcohol, and let it stand twenty-four hours in a covered vessel, then, having packed it closely in a cylindrical percolator, pour on alcohol until three gallons have slowly passed, or until the root is so far exhausted that the passing liquid has little, if any, taste of aconite. To the tincture thus formed, add an ounce of lime, previously hydrated and in powder, and agitate them together, separate the precipitate which forms, by straining through a close cloth, and saturate the liquid with diluted sulphuric acid till slightly acid. Filter the liquid through a close filter, and distil off the alcohol, until the remaining liquid measures about a pint and a-half. Remove any fixed oil which may separate on standing, and having continued the evaporation to a syrupy consistence, agitate it well with four fluid ounces of ether in a suitable bottle, and decant the ether when it has separated. Then add the solution of ammonia in excess, agitate thoroughly, wash the mixture with repeated portions of ether, by agitation and decantation, and having mixed these last ethereal liquids, allow the ether to evaporate spontaneously from a capsule, until the aconitine remains as a dry, amorphous, shining residue, which should be removed from the capsule, powdered, and preserved for use in a stoppered vial.

As thus obtained, aconitine is a powder of greenish or brownish-white colour, uncrystallised, very soluble in alcohol, ether, and chloroform, saturates acids, and has a strong alkaline reaction. It irritates the nose powerfully when a minute particle is inhaled, but does not excite sneezing.

Modes of Using Aconitine.—When aconitine is to be employed in the form of an ointment, its activity may be much increased by adding to it a sufficiency of acetic acid to salify it, which renders it more readily absorbable in contact with the skin. The powder should first be triturated with a few drops of alcohol, and the acid added to this, in the proportion of a drop of acetic acid to each grain of the alkaloid.

Glycerole.—When aconitine is mixed with a slight excess of acetic acid, it readily dissolves in glycerin, and in this state may be applied by friction to the surface requiring its anæsthetic action. The proportion of the alkaloid to each preparation must be regulated by the prescriber; two or three grains to the drachm of prepared lard is a maximum strength for the ointment, and from this down to one grain.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting, Friday, June 7, 1861.

The Duke of Northumberland, K.G., F.R.S., President, in the Chair.

On the Physical Basis of Solar Chemistry, by JOHN TYNDALL, Esq., F.R.S., Professor of Natural Philosophy, Royal Institution.

OMITTING all preface, the speaker drew attention to an experimental arrangement intended to prove that gaseous bodies radiate heat in different degrees. Behind a double screen of polished tin was placed an ordinary ring gas-burner; on this was placed a hot copper ball, from which a column of heated air ascended; behind the screen, but so placed that no ray from the ball could reach the instrument, was an excellent thermo-electric pile, connected by wires with a very delicate galvanometer. The thermo-electric pile was known to be an instrument whereby heat was applied to the generation of electric currents, the strength of the current being an accurate measure of the quantity of the heat. As long as both faces of the pile were at the same temperature no current was produced; but the slightest difference in the temperature of the two faces at once declared itself by the production of a current, which, when carried through the galvanometer, indicated by the deflection of the needle, both its strength and its direction.

The two faces of the pile were in the first instance brought to the same temperature; the equilibrium being shown by the needle of the galvanometer standing at zero. The rays emitted by the current of hot air already referred to were permitted to fall upon one of the faces of the pile; and an extremely slight movement of the needle showed that the radiation from the hot air, though sensible, was extremely feeble. Connected with the ring-burner was a holder containing oxygen gas; and by turning a cock, a stream of this gas was permitted to issue from the burner, strike the copper ball, and ascend in a heated column in front of the pile. The result was, that oxygen showed itself, as a radiator of heat, to be quite as feeble as atmospheric air.

A second holder, containing olefant gas, was also connected by its own system of tubes with the ring-burner. Oxygen had already flowed over the ball and cooled it in some degree. Hence, as a radiator, in comparison with oxygen, the olefant gas laboured under a disadvantage. It was purposely arranged that this should be the case; so that if, notwithstanding its being less hot, the olefant gas showed itself a better radiator, its claim to superiority in this respect would be decisively proved. On permitting the gas to issue upwards, it cast an amount of heat against the adjacent face of the pile sufficient to impel the needle of the galvanometer almost to its stops at 90°. This experiment proved the vast difference between two equally transparent gases with regard to their power of emitting radiant heat.

The converse experiment was now performed. The thermo-electric pile was removed and placed between two cubes filled with water kept in a state of constant ebullition; and it was so arranged that the quantities of heat falling from the cubes on the opposite faces of the pile were exactly equal, thus neutralising each other. The needle of the galvanometer being at zero, a sheet of oxygen gas was caused to issue from a slit between one of the cubes and the adjacent face of the pile. If this sheet of gas possessed any sensible power of intercepting the thermal rays from the cube, one face of the pile being deprived of the heat thus intercepted, a difference of temperature between its two faces would instantly set in, and

the result would be declared by the galvanometer. The quantity absorbed by the oxygen under those circumstances was too feeble to affect the galvanometer; the gas, in fact, proved sensibly transparent to the rays of heat. It had but a feeble power of radiation; it had an equally feeble power of absorption.

The pile remaining in its position, a sheet of olefiant gas was caused to issue from the same slit as that through which the oxygen had passed. No one present could see the gas. It was quite invisible; the light went through it as freely as through oxygen or air; but its effect upon the thermal rays emanating from the cube was what might be expected from a sheet of metal. A quantity so large was cut off that the needle of the galvanometer, promptly quitting the zero line, moved with energy to its stops: thus, the olefiant gas, so light and clear and pervious to luminous rays, was a most potent destroyer of the rays emanating from an obscure source. The reciprocity of action established in the case of oxygen comes out here; the good radiator is found by this experiment to be the good absorber.

This result, which was exhibited before a public audience this evening for the first time, was typical of what had been obtained with gases generally. Going through the entire list of gases and vapours in this way, we should find radiation and absorption to be as rigidly associated as positive and negative in electricity, or as north and south polarity in magnetism. The gas which, when heated is most competent to generate a calorific ray, is precisely that which is most competent to stop such a ray. If the radiation be high, the absorption is high; if the radiation be moderate, the absorption is moderate; if the radiation be low, the absorption is low; so that if we make the number which expresses the absorptive power the numerator of a fraction, and that which expresses its radiative power the denominator, the result would be, that, on account of the numerator and denominator varying in the same proportion, the value of that fraction would always remain the same, whatever might be the gas or vapour experimented with.

But why should this reciprocity exist? What is the meaning of absorption? What is the meaning of radiation? When you cast a stone into still water, rings of waves surround the place where it falls; motion is radiated on all sides from the centre of disturbance. When the hammer strikes a bell, the latter vibrates; and sound, which is nothing more than an undulatory motion of the air, is radiated in all directions. Modern philosophy reduces light and heat to the same mechanical category. A luminous body is one with its particles in a state of vibration; a hot body is one with its particles also vibrating, but at a rate which is incompetent to excite the sense of vision; and, as a sounding body has the air around it, through which it propagates its vibrations, so also the luminous or heated body has a medium, called ether, which accepts its motions and carries them forward with inconceivable velocity. Radiation, then, as regards both light and heat, is the transference of motion from the vibrating body to the ether in which it swings; and, as in the case of sound, the motion imparted to the air is soon transferred to the surrounding objects, against which the aerial undulations strike, the sound being, in technical language, absorbed; so also with regard to light and heat, absorption consists in the transference of motion from the agitated ether to the particles of the absorbing body.

The simple atoms are found to be bad radiators; the compound atoms good ones; and the higher the degree of complexity in the atomic grouping, the more potent, as a general rule, is the radiation and absorption. Let us get definite ideas here, however gross, and purify them afterwards by the process of abstraction. Imagine our simple atoms swinging like single spheres in the ether; they cannot create the swell which a group of them united to form a system can produce. An oar runs freely edgeways

through the water, and imparts far less of its motion to the water than when its broad, flat side is brought to bear upon it. In our present language, the oar, broad side vertical, is a good radiator; broad side horizontal, it is a bad radiator. Conversely, the waves of water, impinging upon the flat face of the oar-blade, will impart a greater amount of motion to it than when impinging upon the edge. In the position in which the oar radiates well it also absorbs well. Simple atoms glide through the ether without much resistance; compound ones encounter this, and yield up more speedily their motion to the ether. Mix oxygen and nitrogen mechanically, they absorb and radiate a certain amount. Cause these gases to combine chemically and form nitrous oxide, both the absorption and radiation are thereby augmented 250 times!

In this way we look with the telescope of the intellect into atomic systems, and obtain a conception of processes which the eye of sense can never reach. But gases and vapours possess a power of choice as to the rays which they absorb. They single out certain groups of rays for destruction, and allow other groups to pass unharmed. This is best illustrated by a famous experiment of Sir David Brewster's, modified to suit the requirements of the present discourse. Into a glass cylinder, with its ends stopped by discs of plate glass, a small quantity of nitrous acid gas was introduced; the presence of the gas being indicated by its rich brown colour. The beam from an electric lamp being sent through two prisms of bisulphide of carbon, a spectrum seven feet long and eighteen inches wide was cast upon a screen. Introducing the cylinder containing the nitrous acid into the path of the beam as it issued from the lamp, the splendid and continuous spectrum became instantly furrowed by numerous dark bands, the ray answering to which were struck down by the nitric gas, while it permitted the light which fell upon the intervening spaces to pass with comparative impunity.

Here, also, the principle of reciprocity, as regards radiation and absorption, holds good; and could we, without otherwise altering its physical character, render that nitrous gas luminous, we should find that the very rays which it absorbs are precisely those which it would emit. When atmospheric air and other gases are brought to a state of intense incandescence by the passage of an electric spark, the spectra which we obtain from them consist of a series of bright bands. But such spectra are produced with the greatest brilliancy, when, instead of ordinary gases, we make use of metals heated so highly as to volatilise them. This is easily done by the voltaic current. A capsule of carbon was filled with mercury, which formed the positive electrode of the electric lamp; a carbon point was brought down upon this; and on separating one from the other, a brilliant arc containing the mercury, in a volatilised condition passed between them. The spectrum of this arc was not continuous, like that from the solid carbon points, but consisted of a series of vivid bands, each corresponding in colour to that particular portion of the spectrum to which its rays belonged. Copper gave its system of bands; zinc gave its system; and brass, which is an alloy of copper and zinc, gave a splendid spectrum made up of the bands belonging to both metals.

Not only, however, when metals are united, like zinc and copper, to form an alloy is it possible to obtain the bands which belonged to them. No matter how we may disguise the metal—allowing it to unite with oxygen to form an oxide, and this again with acid to form a salt; if the heat applied be sufficiently intense, the bands belonging to the metal reveal themselves with perfect definition. Holes were drilled in a cylinder of retort carbon, and these being filled with pure culinary salt, the carbon was made the positive electrode of the lamp: the resultant spectrum showed the brilliant yellow lines of the metal sodium. Similar experiments were made with the chlorides of

strontium, calcium, lithium,¹ and other metals; each salt gave the bands due to the metal. Different salts were then mixed together, and rammed into the holes in the carbon. A spectrum was obtained which contained the bands of them all.

The position of these bright bands never varies, and each metal has its own system. Hence the competent observer can infer from the bands of the spectrum the metals which produce it. It is a language addressed to the eye instead of the ear; and the certainty would not be augmented if each metal possessed the power of audibly calling out, "I am here!" Nor is this language affected by distance. If we find that the sun or the stars give us the bands of our terrestrial metals it is a declaration on the part of these orbs that such metals enter into their composition. Does the sun give us any such intimation? Does the solar spectrum exhibit bright lines which we might compare with those produced by our terrestrial metals, and prove either their identity or difference? No. The solar spectrum, when closely examined, gives us a multitude of fine dark lines instead of bright ones. They were first noticed by Dr. Wollaston, were investigated with profound skill by Fraunhofer, and named from him Fraunhofer's lines. They have been long a standing puzzle to philosophers. The bright lines which the metals give us have been also known to us for years; but the connection between both classes of phenomena was wholly unknown, until Kirchhoff, with admirable acuteness, revealed the secret, and placed it at the same time in our power to chemically analyse the sun.

We have now some hard work before us. Hitherto we have been delighted by objects which addressed themselves rather to our æsthetic taste than to our scientific faculty. We have ridden pleasantly to the base of the final cone of Etna, and must now dismount and march wearily through ashes and lava if we would enjoy the prospect from the summit. Our problem is to connect the dark lines of Fraunhofer with the bright ones of the metals. The white beam of the lamp is refracted in passing through our two prisms, but its different components are refracted in different degrees, and thus its colours are drawn apart. Now the colour depends solely upon the rate of oscillation of the particles of the luminous body; red light being produced by one rate, blue light by a much quicker rate, and the colours between red and blue by the intermediate rates. The solid incandescent coal-points give us a continuous spectrum; or in other words, they emit rays of all possible periods between the two extremes of the spectrum. They have particles oscillating so as to produce red, others to produce orange, others to produce yellow, green, blue, indigo, and violet respectively. Colour, as many of you know, is to light what *pitch* is to sound. When a violin player presses his finger on a string he makes it shorter and tighter, and thus, causing it to vibrate more speedily, augments the pitch. Imagine such a player to move his finger slowly along the string, shortening it gradually as he draws his bow, the note would rise in pitch by a regular gradation; there would be no gap intervening between note and note. Here we have the analogue to the continuous spectrum, whose colours insensibly blend together without gap or interruption, from the red of the lowest pitch to the violet of the highest. But suppose the player, instead of gradually shortening his string, to press his finger on a certain point, and to sound the corresponding note; then to pass on to another point more or less distant, and sound its note; then to another, and so on, thus sounding particular notes separated from each other by gaps which correspond to the intervals of

the string passed over. We should then have the exact analogue of a spectrum composed of separate bright bands with intervals of darkness between them. But this, though a perfectly true and intelligible analogy, is not sufficient for our purpose. We must look with the mind's eye at the very oscillating atoms of the volatilised metal. Figure these atoms connected by springs of a certain tension, and which, if the atoms are squeezed, together push them asunder, or if the atoms are drawn apart pull them together, causing them, before coming to rest, to quiver at a certain definite rate determined by the strength of the spring. Now the volatilised metal which gives us one bright band is to be figured as having its atoms united by springs all of the same tension, its vibrations are all of one kind. The metal which gives us two bands may be figured as having some of its atoms united by springs of one tension, and others by a second series of springs of a different tension. Its vibrations are of two distinct kinds. So, also, when we have three or more bands we are to figure as many distinct sets of springs, each set capable of vibrating in its own particular time and at a different rate from the other. If we seize this idea definitely we shall have no difficulty in dropping the metaphor of springs, and substituting for it mentally the forces by which the atoms act upon each other. Having thus far cleared our way, let us make another effort to advance.

Here is a pendulum—a heavy ivory ball suspended from a string. I blow against this ball; a single puff of my breath moves it a little way from its position of rest; it swings back towards me, and when it reaches the limit of its swing I puff again. It now swings further; and thus by timing my puffs I can so accumulate their action as to produce oscillations of large amplitude. The ivory ball here has absorbed the motions which my breath communicated to the air. I now bring the ball to rest. Suppose, instead of my breath, a wave of air to strike against it, and that this wave is followed by a series of others which succeed each other exactly in the same intervals as my puffs; it is perfectly manifest that these waves would communicate their motion to the ball and cause it to swing as the puffs did. And it is equally manifest that this would not be the case if the impulses of the waves were not properly timed; for then the motion imparted to the pendulum by one wave would be neutralised by another, and there could not be that accumulation of effect which we have when the periods of the waves correspond with the periods of the pendulum. So much for the kind of impulses absorbed by the pendulum. But such a pendulum set oscillating in air produces waves in the air; and we see that the waves which it produces must be of the same period as those whose motions it would take up or absorb most copiously if they struck against it. Just in passing I may remark, that if the periods of the waves be double, treble, quadruple, &c., the periods of the pendulum, the shocks imparted to the latter would also be so timed as to produce an accumulation of motion.

Perhaps the most curious effect of these timed impulses ever described was that observed by a watchmaker, named Killicott, in the year 1741. He set two clocks leaning against the same rail; one of them, which we may call A, was set going; the other, B, not. Some time afterwards he found, to his surprise, that B was ticking also. The pendulums being of the same length the shocks imparted by the ticking of A to the rail against which both clocks rested were propagated to B, and were so timed as to set B going. Other curious effects were at the same time observed. When the pendulums differed from each other a certain amount A set B going, but the re-action of B stopped A. Then B set A going, and the re-action of A stopped B. If the periods of oscillation were close to each other, but still not quite alike, the clocks mutually controlled each other, and by a kind of mutual compromise they ticked in perfect unison.

But what has all this to do with our present subject?

¹ The vividness of the colours of the lithium spectrum is extraordinary. It contained a blue band of indescribable splendour. It was thought by many, during the discourse, that I had mistaken strontium for lithium, as this blue band had never before been seen. I have obtained it many times since, and my friend Dr. Miller, having kindly analysed the substance made use of, pronounces it chloride of lithium.—J. T.

They are mechanically identical. The varied actions of the universe are all modes of motion; and the vibration of a ray claims strict brotherhood with the vibrations of our pendulum. Suppose ethereal waves striking upon atoms which oscillate in the same periods as the waves succeed each other, the motion of the waves will be absorbed by the atoms; suppose we send our beam of white light through a sodium flame, the particles of that flame will be chiefly affected by those undulations which are synchronous with their own periods of vibration. There will be on the part of those particular rays a transference of motion from the agitated ether to the atoms of the volatilized sodium, which, as already defined, is absorption. We use glass screens to defend us from the heat of our fires; how do they act? Thus:—The heat emanating from the fire is for the most part due to radiations which are incompetent to excite the sense of vision; we call these rays obscure. Glass, though pervious to the luminous rays, is opaque in a high degree to those obscure rays, and cuts them off, while the cheerful light of the fire is allowed to pass. Now mark me clearly, The heat cut off from your person is to be found in the glass, the latter becomes heated and radiates towards your person; what then is the use of the glass if it merely thus acts as a temporary halting-place for the rays and sends them on afterwards? It does this:—It not only sends the heat it receives towards you, but scatters it also in all other directions round the room. Thus the rays which, were the glass not interposed, would be shot directly against your person, are for the most part diverted from their original direction, and you are preserved from their impact.

Now for our experiment. I pass the beam from the electric lamp through the two prisms, and the spectrum spreads its colours upon the screen. Between the lamp and the prism I interpose this snapdragon light. Alcohol and water are here mixed up with a quantity of common salt, and the metal dish that contains them is heated by a spirit lamp. The vapour from the mixture ignites and we have this monochromatic flame. Through this flame the beam from the lamp is now passing; and observe the result upon the spectrum. You see a dark band cut out of the yellow—not very dark, but sufficiently so to be seen by everybody present. Observe how the band quivers and varies in shade as the amount of yellow light cut off by the unsteady flame varies in amount. The flame of this monochromatic lamp is at the present moment casting its proper yellow light upon that shaded line; and more than this, it casts, in part, the light which it absorbs from the electric lamp upon it; but it scatters the greater portion of this light in other directions, and thus withdraws it from its place upon the screen, as the glass, in the case above supposed, diverted the heat of the fire from your person. Hence the band appears dark; not absolutely, but dark in comparison with the adjacent brilliant portions of the spectrum.

But let me exalt this effect. I place in front of the electric lamp the intense flame of a large Bunsen's burner. I have here a platinum capsule into which I put a bit of sodium less than a pea in magnitude. The sodium placed in the flame soon volatilizes and burns with brilliant incandescence. Observe the spectrum. The yellow band is clearly and sharply cut out, and a band of intense obscurity occupies its place. I withdraw the sodium, the brilliant yellow of the spectrum takes its proper place; I re-introduce the sodium, and the black band appears.

Let me be more precise:—The yellow colour of the spectrum extends over a sensible space, blending on one side into orange, and on the other into green. The term "yellow band" is therefore somewhat indefinite. I want to show you that it is the precise yellow band emitted by the volatilized sodium which the same substance absorbs. By dipping the coal-point used for the positive electrode

into a solution of common salt, and replacing it in the lamp, I obtain that bright yellow band which you now see drawn across the spectrum. Observe the fate of that band when I interpose my sodium light. It is first obliterated, and instantly that black streak occupies its place. See how it alternately flashes and vanishes as I withdraw and introduce the sodium flame!

And supposing that instead of the flame of sodium alone I introduce into the path of the beam a flame in which lithium, strontium, magnesium, calcium, &c., are in a state of volatilization, each metallic vapour would cut out its own system of bands, each corresponding exactly in position with the bright band which that metal itself would cast upon the screen. The light of our electric lamp then shining through such a composite flame would give us a spectrum cut by dark lines, exactly as the solar spectrum is cut up by the lines of Fraunhofer.

And hence we infer the constitution of the great centre of our system. The sun consists of a nucleus which is surrounded by a flaming atmosphere. The light of the nucleus would give us a continuous spectrum, as our common coal-points did; but having to pass through the photosphere, as our beam through the flame, those rays of the nucleus which the photosphere can itself emit are absorbed, and shaded spaces, corresponding to the particular rays absorbed, occur in the spectrum. Abolish the solar nucleus, and we should have a spectrum showing a bright band in the place of every dark line of Fraunhofer. These lines are therefore not absolutely dark, but dark by an amount corresponding to the difference between the light of the nucleus intercepted by the photosphere, and the light which issues from the latter.

The man to whom we owe this beautiful generalisation is Kirchhoff, Professor of Natural Philosophy in the University of Heidelberg; but, like every other great discovery, it is compounded of various elements. Mr. Talbot observed the bright lines in the spectra of coloured flames. Sixteen years ago Dr. Miller gave drawings and descriptions of the spectra of various coloured flames. Wheatstone, with his accustomed ingenuity, analysed the light of the electric spark, and showed that the metals between which the spark passed determined the bright bands in the spectrum of the spark. Masson published a prize essay on these bands; Van der Willigen, and more recently Plücker, have given us beautiful drawings of the spectra, obtained from the discharge of Ruhmkorff's coil. But none of these distinguished men betrayed the least knowledge of the connection between the bright bands of the metals and the dark lines of the solar spectrum. The man who came nearest to the philosophy of the subject, was Angström. In a paper translated from Poggendorff's "Annalen" by myself, and published in the "Philosophical Magazine" for 1855, he indicates that the rays which a body absorbs are precisely those which it can emit when rendered luminous. In another place, he speaks of one of his spectra giving the general impression of reversal of the solar spectrum. Foucault, Stokes, and Thomson, have all been very close to the discovery; and, for my own part, the examination of the radiation and absorption of heat by gases and vapours, some of the results of which I placed before you at the commencement of this discourse, would have led me in 1859 to the law on which all Kirchhoff's speculations are founded, had not an accident withdrawn me from the investigation. But Kirchhoff's claims are unaffected by these circumstances. True, much that I have referred to formed the necessary basis of his discovery; so did the laws of Kepler furnish to Newton the basis of the theory of gravitation. But what Kirchhoff has done carries us far beyond all that had before been accomplished. He has introduced the order of law amid a vast assemblage of empirical observations, and has ennobled our previous knowledge by showing its relationship to some of the most sublime of natural phenomena.

ROYAL INSTITUTION OF GREAT BRITAIN.

Saturday, March 16, 1861.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE IX.

(Concluded from page 54.)

Now, these constituents of the atmosphere are not contained in the air in chemical union, but are present in a state of mechanical mixture. The oxygen and nitrogen are not chemically combined with each other, neither are any of the other constituents contained in this table [pointing to the first table given above] with the exception of the ammonia and nitric acid, or carbonic acid. When the air contains, at one and the same time, these substances, they combine, one possessing positive and the other negative energy; in other words, they act as an acid and a base, and exist in the form of a salt; but in all other cases the mixture is a purely mechanical one and not chemical. At one time it was very generally thought—at least many persons imagined—that the air was a chemical compound of oxygen and nitrogen, and even at the present day we sometimes meet with people who entertain this idea. But one of the facts which tells most against this theory is, perhaps, this:—If you agitate a quantity of atmospheric air with recently-boiled water, you will find that the atmospheric air contained in the vessel in which this agitation has taken place becomes richer in nitrogen and poorer in oxygen; that it now contains no longer the same per-centages of these two gases, but several per cent. more of nitrogen than it did before; and it is found also that the water has dissolved a certain quantity of the air according to the proportion in which the two gases, oxygen and nitrogen, exist in that air. You would find, also, on expelling that air again from the water, that it contained more than 30 per cent. of oxygen and less than 70 per cent. of nitrogen. Now, we know of no instance in which a chemical compound can be in this way decomposed by mere solution in a solvent; therefore, it must be a mere mechanical mixture, for the oxygen is washed out from the nitrogen to such an extent as to render the remaining atmospheric air poorer in oxygen. We have also a further proof that the atmosphere is a mixture, by making artificially that mixture. We have here a vessel containing four volumes of nitrogen and one, of oxygen, which is about the proportion, as we have seen, of these gases in the air. Such a mixture has all the properties of atmospheric air, and animals can breathe it for exactly the same length of time as atmospheric air, and a taper can burn in it with exactly the same appearance; and, in fact, it has all the properties, physical and chemical, of atmospheric air. You see no difference in the combustion of this taper when it is lowered into the vessel; it exhibits the same appearance as when burning in the natural atmospheric air, although this is an artificial mixture.

But it may be asked, How is it, if the atmospheric air be a mechanical mixture, that these substances of which it is composed are all so uniformly mixed together, as to give us these almost identical results, as to composition, from whatever source we take our sample of air for analysis? How is it that gases possessing such different specific gravities as nitrogen, oxygen, ammonia, carbonic acid, nitrous acid, and bodies of this kind, do not separate in layers like oil and water? How is it that the nitrogen in the air does not ascend into the higher regions of the atmosphere, and the oxygen accumulate near the surface of the ground, and the carbonic acid form a thinner and distinct layer beneath the oxygen, because carbonic acid has a specific gravity about one-half more than that of air? Oxygen possesses a greater specific gravity than nitrogen, so that one might suppose that we ought to have layers

of these two bodies—in fact, that we ought to have an atmosphere very similar to that which we shall have here if we place two jars, one containing oxygen and the other nitrogen, one above the other, the vessel containing oxygen being placed below, and that containing nitrogen being placed above. There is our jar of nitrogen, and this lower one is the jar of oxygen. I will endeavour to bring them over each other with as little agitation as possible, and then we will remove this plate from between the two gases. [The jars were arranged as stated.] Now, I think we have here these two gases in layers—the nitrogen above, which will, I think, put out our taper, and the oxygen below, you see, which re-ignites it. We do not get this result in the air, for we have, as we have said, the gases uniformly mixed. How is it that this mixture takes place? It has been found that whenever two gases of different specific gravities are placed in contact, even though we place the heavier gas below and the lighter gas above, the two gases will not for any length of time remain in this position; the heavier gas will ascend into the lighter gas above it, and the lighter gas from above will descend into the lower gas. What is termed “diffusion” will take place between those two gases. We have a simple arrangement here which will, I trust, illustrate this fact. Here are two common soda-water bottles which are united by a narrow glass tube, and they have been so united for several hours. The upper of these bottles contained hydrogen, and the lower bottle contained oxygen at the commencement of the experiment, when the bottles were filled some hours ago; so that, you see, we had in the upper vessel a gas which is sixteen times lighter than the gas contained in the lower one; and yet the heavier gas ascends through the narrow glass tube into the upper bottle, and diffuses itself into the lighter gas; whilst at the same time the light gas contained in the upper vessel makes its way, against gravity, downwards through this tube, and mixes with the gas in the lower bottle. By simply applying a light to the mouth of these bottles we shall find that we have an explosive mixture of oxygen and hydrogen which will manifest itself in the ordinary way. [A light was applied to the mouth of the upper bottle, and the contents burnt with a slight explosion.] You see from this that the hydrogen originally in that bottle was obviously mixed with oxygen which must have come up from the lower bottle; and we shall find that some of the hydrogen has gone down into the lower bottle. [A light was also applied to the mouth of the lower bottle with the same result as in the other case.] There also we have an explosive mixture, so that these gases, notwithstanding their very great difference of specific gravity, have yet mixed with each other through this very narrow tube.

Different gases diffuse in this way with different velocities. The lower the specific gravity of the gas, the greater the velocity with which it diffuses. These different velocities have been determined by Professor Graham, and the results are contained in the table which you have here.

Diffusion of Gases.

	Sp. Gr.	Diffusion V Sp. Gr. Volumes.
Hydrogen	0061	2633 457
Ammonia	5898	7681 130
Air	1000	1000 100
Carbonic acid . . .	15239	12345 81
Chlorine	24700	15716 64

I think that this difference in the diffusibility of gases may be clearly shown by a simple experiment. Here is a comparatively narrow glass tube terminating in a wider one, and the upper part of the wide tube is closed with a plug of plaster of Paris about half-an-inch in thickness. We will fill this compound tube with hydrogen by displacement; gradually withdraw this gas delivery tube and plunge the lower and open end of the compound tube into a coloured liquid, and now we shall have the hydrogen

rushing out and the air rushing in through this plug; but the hydrogen rushes out more rapidly than the air can get in, and consequently we have a rarefaction produced in the interior; and you see the effect of it in the rising of the coloured liquid, which is ascending very rapidly into the narrow tube, thus showing that the hydrogen is diffusing through that plaster of Paris plug much more rapidly than the atmospheric air, which is quite in accordance with the numbers in that table. Here is another experiment which illustrates the same thing to some extent. Here is a beaker, containing atmospheric air, which has been closed by means of very thin caoutchouc, and it is surrounded by a jar containing hydrogen which passes into the beaker to a much greater extent than the air gets out of the beaker. The hydrogen passes into the glass more rapidly than the air gets out, and the consequence is that we have a protuberance of the membrane which was at first perfectly flat. Here [exhibiting a similar beaker] we have the experiment inverted. This glass vessel was filled with hydrogen, and covered with a caoutchouc membrane. You see how the hydrogen has got out of the vessel more rapidly than the atmospheric air got in, and therefore this membrane is pressed into the jar to a very great extent.

Diffusion is one mode, then, by which the constituents of the atmosphere are kept mixed together, or prevented from separating into layers at all events. Then the mixture is rendered still more uniform, and any accumulation of gaseous matter at one particular place is rendered almost impossible, by atmospheric currents. I can only allude very slightly to the chief of them, namely, the trade winds, which, as you are aware, extend somewhat more than 30° on each side of the equator, and by means of which the atmospheric air from the temperate regions is carried into the torrid zone, and is then brought back again, by an upper current in an exactly opposite direction, into the colder regions. In this way there is a constant interchange of air between the Temperate, and even Polar regions, and the Tropics; and thus the whole atmosphere is kept in a state of constant mixture by the action of these trade winds.

But we have various operations going on in the atmosphere which must have a tendency, if they were not counteracted, to produce a gradual and permanent change in the comparative amount of each constituent. We have seen that combustion and respiration produce considerable quantities of carbonic acid gas; decay and putrefaction produce also immense quantities of the same gas. In the course of a single day by the combustion and respiration going on in London alone there are given into the atmosphere of our city no less than 150,000,000 cubic feet of carbonic acid. When we consider the vast extent to which these processes are going on all over the inhabited portion of the surface of the globe, we might be led to expect that the quantity of carbonic acid would be gradually increasing in the atmosphere; but there is another operation going on simultaneously, which tends in the opposite direction. This operation is the function of plants in taking carbonic acid from the air and restoring the oxygen of that carbonic acid. Just as animals, in breathing, take in the oxygen of the air, and give out carbonic acid, so plants take in by their leaves, which are really also their lungs, this very carbonic acid, and give out oxygen again in the same volume as it is contained in the carbonic acid which they absorb. Thus, whilst respiration, combustion and decay are sending into the air immense quantities of carbonic acid, all the green parts of vegetables are absorbing that carbonic acid, and restoring its oxygen into the atmosphere. Combustible substances remove oxygen from the air in forming carbonic acid, and the plants restore the same amount of oxygen back again into the atmosphere. By the simultaneous action of these two classes of processes, something like an equilibrium in the quantities of oxygen

and carbonic acid present in the atmosphere at any one time is produced.

You will perceive from what I have just said that both the oxygen and the carbonic acid contained in the air have very definite functions to perform; but we have hitherto seen no use for the nitrogen contained in the atmosphere. The nitrogen would appear, if we look at it only superficially, to be quite a useless element. If the nitrogen were removed from the air, the same volume of atmospheric air would then contain exactly the same amount of oxygen as it does at the present moment, because the oxygen would spread itself out throughout the same space as it now occupies; therefore, if no nitrogen were present, you would have only the same amount of oxygen in a cubic yard of air as you have at the present moment; but still the relation of the atmosphere to combustion, and, no doubt, to respiration also, would be very different from what it is at present. In all cases of combustion in air the nitrogen takes up large quantities of the heat generated, and keeps down the temperature of the burning body, and this, in fact, prevents the combustion from becoming altogether uncontrollable. We have seen the difference between the burning of charcoal, for instance, and phosphorus, and other bodies, in pure oxygen and in atmospheric air; but you may say that if the oxygen were rarified in this way, the combustion would not proceed more rapidly or with greater intensity than it does when mixed with four volumes of nitrogen. We will just try this. Here is a receiver containing air which we will extract, and we will then fill the vessel with oxygen; when we have done that, we will pump out of it four-fifths of the oxygen that it contains, so as to leave in that receiver only the same amount of oxygen that there would be originally present in it when filled with air. Now we are admitting into that receiver a quantity of oxygen. [The jar having been filled with oxygen, the air-pump was applied until the oxygen was rarified to a pressure of six inches.] We have there a pressure of only one-fifth of the atmosphere, or six inches of mercury. We have an arrangement in the inside of the receiver by which we can ignite a piece of phosphorus, and then you will see whether the phosphorus burns with anything like the same brilliancy as in oxygen at the ordinary pressure. Now that phosphorus is burning there in the oxygen which is only of the same density as the oxygen in atmospheric air. The result is strictly the same, and the light is quite as brilliant as if we were burning the phosphorus in oxygen of the ordinary density at the full pressure of the atmosphere. Thus it is the nitrogen, and the nitrogen alone, which reduces the temperature of combustion, and renders the ordinary combustion controllable. We might also show you by a similar experiment with watch-spring that the pressure of the oxygen does not affect the combustion, but we will forego that experiment, as it would occupy too much time.

Then, again, the atmospheric nitrogen reduces the rate of evaporation, by putting an additional pressure upon fluids. We should have water boiling at a temperature of about 150° , or even below that, if no nitrogen were present in the atmosphere; and the effect of that would be that we should have a far greater quantity of aqueous vapour in the air than we have at the present time. It is true that when saturated the atmosphere would not contain more vapour than it does now, but the point of saturation would be attained in far less time, and the atmosphere would be at all times, quite, or almost, completely saturated with aqueous vapour, if no nitrogen were present in it. I will show this by placing a vessel of warm water, at a temperature of about 150° under the receiver of the air-pump, and then exhausting the air to one-fifth the present pressure. The water is so little heated that I can bear my finger in it for a short time. When we reduce the pressure to the point I have mentioned, I think you will see that that water will boil. [The experiment was

Now these organic substances have, undoubtedly, a most important influence upon the health of the inhabitants; and we have invariably seen that where an improvement has been fairly tried, in respect to the drainage of towns, which has an important effect upon the air, it has had a most remarkable influence in reducing the mortality in those towns. Here is an instance upon this table showing such a result in the case of Croydon:—

Mortality per Thousand at Croydon.

1853	.	.	.	.	.	28
1854	.	.	.	.	.	26
1855	.	.	.	.	.	20
1856	.	.	.	.	.	17
1857	.	.	.	.	.	16

In Croydon, in the year 1853, the drainage was excessively bad, and at that time the mortality there was 28 per thousand per annum. In 1854 some little improvement was made—in fact, the drainage works were commenced, and the mortality was slightly less. In the year 1857, when the works were completed, the mortality was reduced to 16 per thousand, and has remained about that down to the present time. We may, therefore, regard the difference in the number of lives between 16 and 28 as absolutely saved by a better sewerage in that place. Here again we have similar results in the case of Ely:—

Annual Mortality per Thousand in Ely.

From 1843 to 1849	.	.	.	.	.	26
„ 1851 to 1855	.	.	.	.	.	20
1856	.	.	.	.	.	17
1857	.	.	.	.	.	17

You have again in another table a representation of the comparative mortality in many of our large towns:—

Of 1000 Persons Living in the Following Places there Die Yearly:—

In Liverpool	.	.	.	.	.	33
Manchester	.	.	.	.	.	31
Birmingham	.	.	.	.	.	27
London	.	.	.	.	.	24
Small towns and villages	.	.	.	.	.	20
Estimated normal death-rate	.	.	.	.	.	11

The estimated normal death-rate is only 11 persons in 1000 annually. From these tables you will be enabled to judge of the influence upon the atmosphere of these sewer emanations. While the gases escape as they do from the sewers into our atmosphere, they will undoubtedly exercise a most insalubrious effect upon the localities where they are let loose, and there cannot be a doubt that all improvements by which the gases are confined within the sewers, or are not allowed to escape into the atmosphere without passing through charcoal filters, such as are now being applied to some of the London sewers, would very considerably reduce the mortality of our large cities.

NOTICES OF PATENTS.

Treating Coal, and other Bituminous Materials, and Peat, for the Purpose of Obtaining Solid and Liquid Hydrocarbons. W. M. WILLIAMS.

THIS specification refers to an improved method of distilling coal and other bituminous minerals, under conditions of increased pressure, whereby the yield of paraffin, paraffin-oil, and other liquid and solid hydrocarbons is considerably augmented, as compared with processes of distillation at ordinary pressures, when the production of illuminating gas is the chief object of manufacture. The treatment of peat by the same process constitutes an additional claim under this patent, and is said to furnish similar products.

The process here described may be cited as an interesting example of the action of mechanical forces in determining the molecular arrangement of compound bodies. In the instance of the destructive distillation of coal, whether the hydrocarbons take the form of paraffin or illuminating gas, depends entirely on the physical conditions operating at the moment of production; these substances are but allotropic modifications of the same chemical compound, which, although identical in constitution, exhibit in their

general characters widely different properties. In this respect the present specification bears a remote physical resemblance to the process of Mr. Young for producing paraffin from coal by distillation conducted at low temperatures.

Coating Metals and Metallic Alloys. W. BAGLEY and W. MINCHER, Birmingham. (Not proceeded with.)

FOR the purpose of coating metals, such as iron or steel, with lead, or a metallic alloy composed of lead with a small quantity of copper, the inventors proceed to clean the surfaces of the original metal firstly by the process of scaling, and afterwards by immersion in dilute hydrochloric acid. When thus prepared, the sheet iron is subjected to the action of a mixed solution, containing chloride of lead in combination with the hydrochloric solution of arsenious acid; there it remains until a coating both of lead and arsenic is formed by reduction of these metals on the iron surface. Acting as a flux, this coating enables the iron to take lead freely, when the plate is afterwards dipped into a bath containing the latter metal in a molten state. The patentees prefer, however, to employ for the final coating, instead of pure lead, an alloy of lead with a small proportion of copper; and the article (plate or otherwise) is removed after dipping, cooled, and finished in the ordinary manner.

A practical trial would be necessary before a precise opinion could be given in regard to the necessity for employing an arsenical solution of so volatile a nature as that here recommended. Considering that arsenic plays so small a part in the ultimate composition of the metallic coating, it would be highly desirable in experimenting with this process to endeavour to avoid altogether the use of a poisonous agent, the fumes of which would certainly be prejudicial to the health of the workmen engaged.

Coating Metals and Alloys of Metals. W. BAGLEY and W. MINCHER.

THE patentees secured provisional protection, but did not proceed with another suggestion for the same object as the last. This consists in a preliminary preparation of the iron surface as before, and immersion in a solution of chloride of zinc; then, by galvanic agency, depositing upon the iron a coating of metallic zinc, which serves to facilitate the attachment of the lead when afterwards the article is dipped into a bath of molten lead, employed pure, or previously alloyed with a small proportion of copper.

It is a common practice to employ zinc for coating iron prior to its receiving a thicker surface of lead; and the chloride of zinc is also frequently used as a flux. The suggestion of employing an electro-deposit of zinc for this special application appears to be novel, but the advantage of the new process, especially in regard to expedition, does not seem manifest.

Insulating the Conducting Wires used for Telegraphic Purposes. W. HIRSCH. (Not proceeded with.)

THIS specification refers to a method for securing the more perfect insulation of telegraphic wires by enveloping them in coils of spun glass, and afterwards filling up the capillary interstices between the vitreous fibres with resinous, tarry, or other non-conducting fusible material.

Glass, although one of the best insulating materials known, would, at the same time, we fear, be one of the most expensive. In the condition of spun filaments, its cost of production would be considerably augmented; and, unless it be proved that the resinous coating would itself afford additional protection, it is very questionable whether glass, in this state of division, could resist for any length of time the corrosive action of water.

An Improved Method of, and Machinery for, Amalgamating and for Effecting the Separation of Gold from Earthy and other Matters containing the same. B. G. SLOPER. (Not proceeded with.)

THIS invention refers principally to a mechanical contrivance for assisting the action of the mercury in the process of extracting gold from crushed quartz and other rocks containing it. An Archimedean screw, and another working vertically, are kept in rotation by suitable machinery, and the apparatus is in part surrounded with a steam jacket, so that, by a more elevated temperature, the mercury may have its amalgamating powers to some extent augmented.

Preserving Organic Substances. W. RATTNAY, Aberdeen.

THIS patent refers to the application of sulphurous acid and its compounds with the alkalies and alkaline earths, as means of preventing the decomposition or decay of sundry vegetable and animal substances.

Considering that these agents have long been employed for checking the fermentation of beer, in the drying and preservation of hops, and in a few similar instances with well-tried efficacy, there is little doubt that the use of sulphurous acid for the purpose contemplated would be equally successful.

Filters. W. CLARK. A Communication. (Not proceeded with.)

THIS specification describes the construction of a portable filter. It consists of an india-rubber bottle divided into upper and lower compartments by a porous plate or diaphragm of filtering material. Both extremities are provided with stop-cocks, that above being connected with the water supply-pipe. When in use the filtering bag or bottle is necessarily kept distended, so that the apparatus requires to be supported in a light frame work, but when dismantled the india-rubber collapses upon the porous diaphragm, and the whole arrangement occupies very little space.

CORRESPONDENCE.

Phosphorus Matches.

To the Editor of the CHEMICAL NEWS.

SIR,—In the *Chemical News* of July 27 there is a description of a well-corked bottle, coated internally with phosphorus. "But," says Mr. G. Gore, "It was not until 1833 that it (the phosphorus) was successfully applied (for lighting matches)." Now, Sir, there lived in Dublin, on the Quay, near the Four Courts, a chemist, who made his fortune in 1816 by the manufacture of precisely such bottles as Mr. Gore describes, and which never failed to give a light. William Kertland was the name of that chemist, and there are still living those who remember to have seen him in their youth jovially spending the Sunday evening at Quinn's Bray after a ramble through the Daryle at Powerscourt, so prosperous was his business.

I am, &c.

ZAMIEL.

Salts of Manganese.

To the Editor of the CHEMICAL NEWS.

SIR,—Referring to an inquiry in a recent Number of the CHEMICAL NEWS, I beg to state that I am manufacturing "solid chloride of manganese," and should be obliged if you would inform your correspondent of this. I will furnish him with particulars on application.

I am, &c. B. W. GERLAND.

Newton-le-Willows.

To the Editor of the CHEMICAL NEWS.

SIR,—I can supply your correspondent with any quantity of chloride of manganese, pure or commercial, crystallised or in large lumps. I am, &c. WM. LORBERG.
Queen Street, Bermondsey, S.E.

On the Adulteration of White Lead.

To the Editor of the CHEMICAL NEWS.

SIR,—I am glad to see in your last Number of the CHEMICAL NEWS an *exposé* of the frauds in white lead. Some time ago I analysed a portion of white lead that came from a wholesale colour house near Cheapside, and it was adulterated 30 per cent. Shortly afterwards I had a sample sent me for analysis that came from a large manufacturer not far from Holborn Hill as "extra superfine," and it was adulterated 32 per cent. In both instances the principal of the firm admitted that they were adulterated, but excused themselves by saying that it was a mistake of the warehousemen.

I had occasion to analyse some linseed oil that came from a City crusher, and it was mixed with 20 per cent. of resin oil.

Trade has now become a system of adulteration, and that manufacturer who is the cleverest at adulteration is the one who makes the largest fortune and secures the greatest trade.

I am, &c.

GEO. JAS. JOHNSON
258, High Street, Chatham, and Beacon Hill, Luton.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Perchromic Acid.—Aschoff (*Journal für prakt. Chemie*, Bd. lxxxi. s. 401) confirms the existence of a definite acid Cr_2O_7 , formed when binoxide of nitrogen acts on a dilute solution of bichromate of potash. The blue compound formed in this reaction is, as our readers know, soluble in ether, and when the ethereal solution is shaken with a dilute solution of potash added in such a quantity that the blue colour is hardly apparent, the aqueous solution which separates is of a deep violet-brown colour, and perfectly neutral. It becomes again blue on the addition of an acid, but an excess of potash completely decomposes it, just as in the case of the acid Mn_2O_7 . The colouring power of perchromic acid is immense, a decigramme being sufficient to give a deep blue colour to 500 cub. cents. of water. The author could not obtain a definite salt with the acid.

Permanent Source of Ozone.—Boettger says (*Zeitschrift für Chem. und Pharm.*, Bd. iii. s. 718) that a mixture of two parts of permanganate of potash and three parts of sulphuric acid forms a pasty mass, which will continue to give off ozone for several months.

Preparation of Permanganate of Potash.—Boettger recommends the following process, (*Ibid.*)—Fuse together in an iron crucible, 4 parts of hydrate of potash, and 2 parts of chlorate of potash, and when oxygen begins to be evolved remove the crucible from the fire, and stir in gradually one part of peroxide of manganese. Heat again until the mixture solidifies, after which boil it with 80 parts of water, and then pass carbonic acid until the liquid becomes red. The solution is now decanted, filtered through gun-cotton, evaporated to one-half and set aside to cool, whereupon large crystals of the permanganate are obtained.

Artificial Chalybeate Water.—Hauer points out (*Journal für prakt. Chem.*, Bd. lxxxi. s. 391) that reduced iron dissolves with facility in a solution of carbonic acid, the solubility increasing, of course, under pressure, and being diminished by the presence of alkaline carbonates

in the water. Our readers ought to know, however, that the above way of making chalybeate water has been patented in England for a year or more.

II. ORGANIC CHEMISTRY.

Purification of Acetic Ether.—The following method of rectifying acetic ether is given by Engelhardt (*Zeitschrift für Chem. und Pharm.* Bd. iii. s. 618): The rough product contains free acetic acid and alcohol. In consequence of the presence of the latter, it is soluble in an equal volume of water; but when pure, it requires at least seven times its volume to dissolve. The author's process is based on this fact. He begins by neutralizing the acetic acid with calcined magnesia, he then dilutes with an equal volume of water, and afterwards adds sufficient common salt to make a saturated solution. When no more salt dissolves, the whole is shaken up, and then allowed to rest, whereupon the mixture separates into two layers, the lower of which is a saturated solution of common salt in dilute spirit, and the upper acetic ether nearly anhydrous (0.89) and quite pure. No further rectification is necessary.

New Compound by the Action of Hydrogen on Nitrobenzine.—Jumel (*Bulletin de la Société Chimique de Paris*, No. II., p. 47), passed simultaneously dry hydrogen and the vapour of nitrobenzine over platinum black contained in a porcelain tube and heated. At first white vapours were copiously produced, then these ceased and a yellowish-brown oil condensed and fell to the bottom of the water in the receiver. After some time the production of this oil stopped, very heavy yellow vapours passed, and then a large quantity of thick, black oil condensed. In several experiments the same phenomena presented themselves in the same order. The yellow oil dissolved in acetic acid, the solution treated with hypochlorite of lime gave a dirty violet-red colour like wine lees; bichromate of potash produced no colour. Tested with the hypochlorite and without acid, it gave a blue colour, dirty at first, but which deepened in time, and eventually looked like an alkaline solution of sulphate of copper. This blue colour, the author found, was reddened by acids and restored by alkalis just like litmus. The blue matter is soluble in water and alcohol, and dyes, but the colour washes out. The black oil obtained, the author found to be only a product of the decomposition of vapour of nitrobenzine heated to about 400° C.—for after a time the platinum becomes covered with carbon and does not act.

Veratric Acid = $C_{10}H_{10}O_8$, according to W. Merck, yields with strong nitric acid yellow nitro-veratric acid $C_{10}H_7NO_{12}$, which is soluble in water, crystallizes from alcohol and yields binitroveratric acid with more NO_2 . By heating moderately with 3 parts of baryta, veratrol = $C_{10}H_{10}O_4$ is obtained, which has an aromatic odour, crystallizes at 15° (59° F.), possesses the specific gravity of 1.086, boils between 202° and 205° (396° to 401° F.); it yields with fuming nitric acid nitro- and binitroveratrol, and with bromine bibromoveratrol = $C_{10}H_6Br_2O_4$.—*Annal. der Ch. und Ph.* xxxii. 58—62.

III. ANALYTICAL CHEMISTRY.

Estimation of Iodine in Tincture.—Ranwez (*Polytech. Notizblatt*, Bd. xvi. s. 47) shakes the tincture with a known weight of granulated zinc. When the solution is colourless, he separates the metal that remains, dries, and weighs it. The amount of iodine is calculated from the loss. Pure zinc must, of course, be employed.

IV. TECHNICAL CHEMISTRY.

Extraction of Silver from Galena.—Sulphide of silver and chloride of lead mutually decompose when fused together. The reaction has been applied with success to the extraction of silver from galena. The galena is

mixed with one per cent. of chloride of lead and ten per cent. common salt, and the mixture is fused. Chloride of silver is formed, and, together with the chloride of sodium, swims on the surface, forming, when cold, a crystalline crust, which is easily separated. The crust will contain an excess of chloride of lead. The reduction of the metals may be proceeded with in the ordinary way.—*Polytech. Notizblatt*, s. 95.

MISCELLANEOUS.

Practical and Theoretical Chemistry.—The labours of physiological chemists have pretty well settled the general principle, that the material by which our bodies are sustained, may be conveniently arranged under the heads of blood-producing food, and food which is principally consumed, during the process of respiration, in generating animal heat. The blood-producing food, consisting chiefly of albumen, fibrin and casein, contains nitrogen; while the heat-producing food is devoid of that element. Very complicated formulæ are assigned to the blood-producing principles, giving, according to some authorities, more than one hundred equivalents to two of their elements, namely, carbon and hydrogen. Such formulæ cannot be supported by analysis with any show of probability. Nevertheless, they are chemists, who take these improbable formulæ as proved; and venture to trace the transformations which the blood-producing principles of the food undergo, during their conversion into blood. Thus, they say, that casein, with a certain proportion of water, may yield blood-albumen and chondrin, the latter being the principle of cartilage; and, again, that two equivalents of albumen and two of water are equal to one equivalent of blood-fibrin, one equivalent of choleic acid, and one equivalent of gelatin. The equations here given are true equations, on the assumption that the composition of the substances concerned is correctly represented; but this assumption is wholly gratuitous. Admitting, however, that the analyses are correct, can it be believed that equations like these represent the truth of nature? These chemists, drawing on their fancy, do not hesitate to make any transformation that may suit their theories. If they have too much oxygen to suit their views, oxygen is alleged to be given off; if there is present too much of both oxygen and hydrogen, then water is formed; and, if carbon is redundant, they do not hesitate to say that the portion in excess is removed, in the form of carbonic acid, by the oxygen of the air! This scientific facility reminds us of an anecdote which is related of one of our Commissioners of Patents, who was in office about the beginning of the present century. He was a man of general information, but had no pretensions to a knowledge of science; and, in his own opinion, everything was easy to accomplish. Being simple minded and credulous, he was easily imposed upon. A wag applied to him for a patent for making boards out of sawdust. "Oh! my friend," said the Commissioner, "that art has long been known, and is very easy." "But," said the applicant, "I fear, sir, you do not exactly understand my purpose. My invention consists in making pine boards out of oak sawdust." "Oh! is that it," rejoined the Commissioner; "but that is very easy also, for you have only to add a little turpentine!"

ANSWERS TO CORRESPONDENTS.

W. X. Y.—The sample of oil which you have sent is largely adulterated with olive oil, and is not worth a quarter the sum you name.

Y. C. Bell.—Steel beams of balances can be protected from injurious fumes of the laboratory by being lacquered, or better still by being electro-gilded. For a small blast furnace apply to Griffin and Co., Bunhill Row.

J. D. Ferriss.—We will inquire, and communicate with you.

THE CHEMICAL NEWS.

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SCIENTIFIC AND ANALYTICAL CHEMISTRY.

*On the Exact Separation of the Ethyl Bases,¹
by M. CAREY LEA, Philadelphia.*

It has been known for some time that when ammonia was acted upon by the halogen ethers, the ethylamine produced was always accompanied by a variable quantity of the secondary and tertiary ethyl bases and the ammonium base, and I have shown that the same is the case with respect to the secondary and tertiary ethyl bases in the reactions of oxy-ethers. The ammonium base I have not looked for, but it is, doubtless, present also. The primary, secondary, and tertiary bases resemble each other so closely in their properties, that their separation is attended with great difficulty, and has generally been effected by the different solubilities of their chloroplatinates.² I find, however, that it may be accomplished with great ease by means of picric acid. The picrates of the different bases exhibit the most opposite degrees of solubility, the picrate of triethylamine rivalling the picrate of potash in insolubility, while that of diethylamine is soluble to an almost unlimited extent in water, alcohol, and ether, without being in the least deliquescent. Its affinity for ether is so great that its concentrated ethereal solution will remain exposed to the atmosphere for days in a syrupy condition, gradually crystallising on the surface, and eventually all through, to a radiated mass. Between these two extremes the picrate of ethylamine occupies an intermediate position, and the differences of solubility are so well marked as to render the separation perfectly easy in the manner to be indicated below.

1. Separation of the Mixed Bases from Ammonia.

This has been generally directed to be done by treating the mixed hydrochlorates either with absolute alcohol, or with a mixture of strong alcohol and ether, whereby the sal ammoniac is supposed to be left undissolved.

This process I have always found to the last degree unsatisfactory, and necessarily so; for if absolute alcohol be allowed to stand over sal ammoniac in powder for a time and then be evaporated in a watch-glass, a certain quantity of sal ammoniac will be deposited, and the same is true of a mixture of strong alcohol and ether. By operating on the ammonias in the form of sulphates, a much more satisfactory result is obtained. For this purpose the mixture as poured out from the pressure-tube is treated with a sufficient quantity of pure sulphuric acid to displace the nitric acid (or if bromide or iodide of ethyl have been employed, the bromhydric or iodyhydric acid), and the solution is evaporated as far as possible at a temperature of about 250° F. The paste

mass is exhausted with strong alcohol, the alcoholic solution is evaporated as far as possible at the same temperature, and the residue exhausted with a small quantity of absolute alcohol. Two exhaustions at least are necessary, even when absolute alcohol is used the first time. In this way a satisfactory result is obtained, which cannot be accomplished with the chlorhydrates.

2. Separation of the Mixed Bases from each other.

TRIETHYLAMINE.

After being completely freed from ammonia, as above described, the mixed sulphates were distilled with caustic potash, in the usual way, and the bases were saturated with crystallised picric acid. Sufficient heat was applied to re-dissolve the precipitate which formed toward the close of the saturation, and the hot (not too concentrated) solution allowed to crystallise. The first crop of crystals thus obtained were purified by several re-crystallisations, the chlorhydrate of the base was formed, and from this the chlorplatinat, which was obtained in large and beautiful crystals. The platinum salt was analysed.

·4858 grammes substance gave ·1567 platinum,

Per cent.

This corresponds to 32·26

Chloroplatinat of triethylamine contains 32·23.

The close correspondence of the result with the number given by theory indicates the perfect purity of the triethylamine. Its proportion is but inconsiderable compared with that of ethylamine and diethylamine, and it is chiefly contained in the first portion of the gaseous products which pass over in the distillation with caustic potash.

Picrate of triethylamine crystallises in delicate yellow needles, which dissolve sparingly in cold water and alcohol, abundantly in hot. The decrease of solubility in an aqueous or alcoholic solution as it cools is very sudden, so that a hot solution deposits almost the whole of the salt as it begins to cool. Placed on platinum foil and gently heated, it melts, turns first red, then black, at the same time boiling up, takes fire, and leaves a residue of charcoal.

ETHYLAMINE.

After the least soluble salt had been removed in the manner just described, the mother-water deposited another crop of crystals, which were purified by repeated re-crystallisation, the chlorhydrate of the base was formed, and its chlorplatinat was analysed.

·4429 grammes substance gave ·1728 platinum.

Per cent.

This corresponds to 39·02

Chloroplatinat of ethylamine contains 39·29.

Picrate of ethylamine presents great differences of appearance according to accidental circumstances. When first crystallising out of the mixed solution, it usually forms groups of short, brown prisms, adhering to the bottom of the basin. If these be re-crystallised, we obtain long, yellow, flattened prisms and laminæ, extending in every direction through the liquid; few substances

¹ American Journal of Science and Art.

² Dr. Hoffman has lately described an elegant mode of separation, differing, however, entirely from that here proposed,—viz., by means of oxalic ether.

exhibit more beautiful crystallisations. A dilute alcoholic solution often deposits flat prisms, bevelled at the extremities, and sometimes hexagonal plates.

In solubility in water this substance approximates to the picrate of ammonia, to which, after it has been subjected to repeated re-crystallisations, it bears considerable resemblance in appearance.

DIETHYLAMINE.

After the picrate of ethylamine has been for the most part removed, the mother-waters exhibit a curious phenomenon, which greatly simplifies the further purification of the diethylamine salt left in solution. When these mother-waters have been a little reduced by spontaneous evaporation, or by a gentle heat, the liquid spontaneously separates into two layers, a pale yellow, lighter, and a dark brown, heavier. The evaporation is continued until the heavier layer constitutes rather more than half of the whole, and then the two layers are separated, either at once by a separating-funnel, or ether may be added and thoroughly shaken up, when the brown layer dissolves in the ether and rises to the top. After separation it is allowed to crystallise and is once or twice re-crystallised from a small quantity of ether, by which it is obtained quite pure. A specimen was converted into chlorhydrate and then into chlorplatinate, and gave the following results on analysis:—

*3884 grammes substance gave	1377 platinum.
	Per cent.
This corresponds to	35.45
Chloroplatinate of diethylamine should contain	35.45.

It appears, therefore, that the salt obtained in the above-described manner is perfectly pure.

Picrate of diethylamine, without being deliquescent, is soluble to almost any extent in water, alcohol, and ether. By spontaneous evaporation, it crystallises in a radiated mass: Sometimes at the bottom of the vessel beautiful transparent brown-yellow crystals form, which belong to the monoclinic system. Combination observed $\infty P \infty$, [$\infty P \infty$], ∞P , ∞P , $+ P$, and a clinodiagonal doma.

The approximate proportions in which the different ethyl bases were produced by the reaction here made use of were, perhaps, ten per cent. of triethylamine, thirty or forty of diethylamine, and fifty or sixty of ethylamine.

The method here described gives with facility a very exact separation of these bases. The only precaution necessary to be observed, especially in operating on small quantities, is, that the solution must not be too concentrated at the outset, otherwise the picrate of triethylamine will be mixed with picrate of ethylamine. But the delicate needles of the former are easily distinguished from the broader ones and prismatic crystals of the latter, and two or three re-crystallisations suffice to separate them completely.

I feel assured that picric acid will be found to be a most valuable re-agent in effecting separations of cognate organic bases. It may be doubted if there exists any other acid whose salts, taken as a whole, exhibit so marked a tendency to ready crystallisation, and the great differences in the solubility of picrates of allied bases adds, as in the foregoing example, to the value of this acid in effecting separations between them.

On Didymium, Lanthanum, Cerite, and Lanthano-Cerite,
by R. HERMANN.

R. HERMANN has studied the preparation of the oxides of cerium, lanthanum, and didymium. These oxides

are still often separated, according to Mosander's process, with diluted nitric acid, and crystallising the sulphates of lanthanum and didymium. The substances thus prepared are, however, never perfectly pure.

After treating the oxides with dilute nitric acid, the undissolved protoxide of cerium is best purified by solution in sulphuric acid, mixing the concentrated solution with much water, and then heating, by which means perfectly pure sulphate of cerium is separated.

In order to purify the oxides of lanthanum and didymium perfectly, advantage may be taken of the circumstance that oxide of lanthanum is a more powerful base than oxide of didymium.

With this object the two substances are first separated by the crystallisation of their sulphates. This operation is facilitated by the fact that sulphate of lanthanum is more difficultly soluble than sulphate of didymium. From a concentrated solution of the two salts prepared in the cold, sulphate of lanthanum separates out first on the application of heat, while the sulphate of didymium remains for the most part in solution. This solution is then evaporated at the temperature of the air to dryness. A mixture is thus formed of crystals of sulphates of didymium and lanthanum. When these are treated with cold water, the sulphate of didymium is dissolved in preference, while the sulphate of lanthanum, which, in the crystallised state, is very difficultly and slowly soluble in a concentrated solution of sulphate of didymium, remains to a great extent behind. This solution of sulphate of didymium is again suffered to evaporate to dryness, and the sulphate of didymium is once more dissolved in cold water. This operation must be repeated as long as sulphate of lanthanum continues to separate. Finally, by these crystallisations tolerably pure sulphates of lanthanum and didymium are obtained.

In order to purify these salts completely, a portion of each solution is precipitated with ammonia, and the precipitates well washed. These precipitates are then mixed with the remaining portion of the same salts and allowed to remain a few days at a gentle heat, with frequent stirring.

If impure sulphate of lanthanum has been so treated, the basic sulphate of lanthanum precipitates the sulphate of didymium still present in the solution completely, and there remains nothing but sulphate of lanthanum in solution, which can be crystallised by the evaporation of the solution.

If, on the other hand, impure sulphate of didymium has been employed, the basic sulphate of lanthanum dissolves perfectly in the liquid, and basic sulphate of didymium is precipitated. After a few days the sediment will consist of nothing but pure basic sulphate of didymium. This may be collected on a filter, dissolved in sulphuric acid, and crystallised.

Examination of some Didymium Compounds.—The compounds of didymium were examined by Mosander, and subsequently very fully by Marignac. Marignac fixed the atomic weight of didymium at 600¹, according to the author it is 593.5.

Oxide of Didymium, obtained by precipitating the sulphate with oxalate of ammonia, heating the oxalate to redness, in the presence of air, and subsequently strongly heating the peroxide so obtained in a covered crucible, appears as white pieces with an earthy fracture, sp. gr. 6.54. By treating peroxide of didymium in a

¹ The oxygen = 100 scale is adopted throughout this paper.

current of hydrogen, it forms white pieces of the texture of the peroxide. It consists of

Di 85.58
O 14.42

100.00

Peroxide of Didymium, prepared by heating nitrate of didymium in the air, forms a silky-looking, spongy mass of a brown colour. By heating the oxalate in the air, with constant stirring, it is obtained in the form of a chestnut-brown powder. Heated in a current of hydrogen, it alters its colour immediately from brown to white, water being formed at the same time. On the other hand, the white oxide immediately becomes brown when heated in the air.

If peroxide of didymium is heated in a covered platinum crucible, so that the crucible lies deep in the flames, and all access of atmospheric air be thus cut off, the action of the deoxidising gases of flame, however, being facilitated, a perfect reduction to the state of white oxide takes place. If the peroxide be dissolved in the oxygen acids, it is reduced with evolution of oxygen to the white oxide. With hydrochloric acid it evolves a small quantity of chlorine.

The composition of peroxide of didymium may be very accurately determined; if a weighed quantity be treated in a close vessel with the solution of the iodide of potassium in diluted hydrochloric acid, and then be allowed to dissolve perfectly, iodine is thus set free as an equivalent of the oxygen which the peroxide contains more than the protoxide. With standard solutions of hyposulphite of soda and iodine, the quantity of iodine set free may be determined. In this way 100 parts of peroxide of didymium set free 7.070 of iodine, which expresses 0.446 oxygen. According to this, the composition of peroxide of didymium may be expressed by the formula $\text{Di}_{23}\text{O}_{33}$:—

Di	85.20	32 =	18992.0	85.19
O	14.80	33 =	3300.0	14.81
	100.00		22292.0	100.00

Attempts to prepare a peroxide of didymium containing a greater proportion of oxygen met with as little success with the author as with Marignac. When a previously-fused mixture of nitrate of didymium and nitrate of potassium was projected into a fused mixture of chlorate of potassium and hydrate of potassium, a brown oxide was obtained, which by the iodine test yielded exactly as much iodine as the peroxide prepared by heating the nitrate of didymium. Also, when peroxide of didymium was treated with a quantity of nitric acid insufficient for complete solution, an oxide remained behind of the same composition as it was before this treatment. Peroxide of didymium, as well as the protoxide, dissolves easily, and with evolution of heat, in sulphuric, nitric, hydrochloric, and acetic acids, and forms with these acids rose-red solutions, which are precipitated white by phosphoric and oxalic acids, and by ferro-cyanide of potassium. With the help of the blow-pipe the oxides of didymium form with borax, both in the outer and inner flame, rose-coloured glasses. The oxides of didymium are dissolved by phosphates with greater difficulty than by borax. A glass is formed in this case that easily becomes turbid from a white deposit; it shows, however, when strongly saturated, a distinct rose coloration. The blue colour mentioned by Mosander was only remarked by the author when the oxide of didymium contained cobalt.

(To be continued.)

On the Production of Graphite by the Decomposition of Cyanogen Compounds, by Dr. P. PAULI.

THE mother-liquors obtained from the evaporation of a solution of the so-called black ash are now commercially marked for caustic alkali. These liquors contain the following compounds :—

1. Chiefly hydrate of soda.
2. Some quantity of carbonate of soda.
3. Several sulphur compounds of sodium,—viz., sulphide of sodium, hyposulphite of soda, sulphite of soda, and sulphate of soda.
4. Sulphide of iron held in solution by the sulphide of sodium.
5. Chloride of sodium.
6. Several cyanogen compounds of sodium, and especially ferrocyanide of sodium.

These liquors are evaporated in large cast-iron pots, and in order to destroy or oxidise the sulphides of sodium and iron, as also the cyanogen compounds, an equivalent quantity of soda-saltpetre is added. All the oxidable sulphur compounds, together with the small quantity of sulphide of iron, are changed to sulphate of soda and peroxide of iron by the nitrate of soda in the boiling liquid at a temperature not below 260° to 270° F. The cyanogen compounds, on the other hand, are not decomposed by the nitre until the liquor begins to pass from the watery into the dry fusion, and the uncombined water of the hydrate of soda has been driven off. When the whole mass of alkali (generally about four tons) reaches a low red heat, a regular evolution of gas is observed : this is evidently owing to the oxygen produced by the decomposition of the nitrate and to the nitrogen from the decomposition of the cyanides; at the same time, a plentiful liberation of graphite is observed, covering the whole surface of the liquor with a bright layer of graphite. This liberation of graphite is still more plainly seen if no nitre be added to the liquor at first, or only so much as is sufficient to oxidise the sulphur compounds; but if a few pounds of nitrate of soda be added when the water has been driven off, and the mass is allowed to become red-hot, a violent reaction takes place, and a large quantity of graphite is set free. This sudden liberation of graphite proves that this substance cannot be derived from the cast iron of the pot in which the fusion is made. So violent is the evolution of gas that a complete cloud of fine particles of caustic soda is carried up into the air, rendering it almost impossible to remain in the neighbourhood of the operation. In this way all the cyanogen compounds are completely decomposed, the iron in the ferrocyanide of sodium becomes peroxide, and in a few hours falls to the bottom of the pot. If the right quantity of saltpetre has been added, a colourless mass of fused caustic soda remains; but if too large an amount of nitre has been added the liquor becomes coloured deep green, owing to the formation of manganate of soda. It is remarkable that, in the absence of nitrate of soda, the cyanogen compounds act reducingly upon the sulphide of sodium. This is seen from the fact that a portion of the soda-lye, which gives no sulphide reaction with a lead salt, produces a blackening after the caustic alkali has been heated to redness.

The graphite may be skimmed off the surface of the fused alkali, and when washed with water and hydrochloric acid it appears in the form of an extremely fine, bright powder. If allowed to swim on the top of the almost red-hot fused soda, the graphite is oxidised gradually, and, after the lapse of about three or four

hours, it altogether disappears. Heated in a platinum crucible by itself, it is incombustible; but it generally contains small particles of charcoal mixed with it, and these undergo oxidation.

The temperature at which this evolution of graphite takes place is a very low one compared with that at which graphite is liberated from cast iron; for a thin iron wire can scarcely be brought to a visible red heat by dipping it into the fused alkali.

From this peculiar decomposition, it would appear that we have good reason to assume that the carbon contained in cyanogen is present in the graphite modification; for if this be not the case, how is it that the easily-combustible charcoal can withstand the oxidising action of the saltpetre, whilst none of the iron of the ferrocyanide of sodium has been reduced to the metallic state?

It has, besides, been lately shown by M. Caron that the formation of steel—i. e., the combination of iron with carbon in the graphite modification—can only take place in presence of cyanogen compounds, and that no carbon whatever is taken up by the iron when this metal is heated with other carboniferous gases. The mode of production of graphite noticed in this communication appears to be an intermediate reaction between that from the carbide of iron and from the nitride of carbon.

As in the process of cementation, it is seen that the carbon of the cyanogen is taken up by the iron without being set free; so this reaction proves that cyanogen can be split up into its constituent parts without either of them combining with a third body.

Despretz asserts that the carbonisation of iron is always preceded by a combination of this metal with nitrogen,—a process which makes it porous and more fit for uniting with carbon. The correctness of this supposition has, however, become rather doubtful by Caron's recently-published experiments (*Comptes-Rendus*, 1860).

To conclude, I beg to say some words about the formation of native graphite. I do not think that this body has been formed from coal or diamond, but I rather believe it has been separated out of carbon compounds as graphite by processes perhaps analogous to those above described.—*Proc. Manchester Phil. Soc.*

On White Lead, by WILLIAM BAKER, F.C.S.

THE following observations on the composition of white lead, in addition to those published in a recent Number, may not be uninteresting to your readers. My analyses refer to Dutch white lead, or carbonate of lead containing some hydrate of lead produced by means of acetic acid and spent tan, and it will be seen that no fixed and definite formula can be written for the commercial dry white lead. It is rather a carbonate, containing a variable proportion of hydrate, that depends upon the conditions under which the corrosions are produced. Thus, in a single bed of corroded lead a number of samples may be selected all varying in the proportion of carbonate to hydrate. Near the side walls, where the aqueous vapour, loaded with carbonic acid, freely ascends, it will be found that, under some circumstances, corrosions of a sugary character are produced, which, upon examination, are found to be a mass of minute transparent crystals of neutral carbonate of lead. Again, on the surface of the corrosion, a thin, scaly layer sometimes separates, which is also near in composition to the neutral carbonate. The quantity and quality of the water employed in grinding

and washing the corrosions to a pulp, doubtless influences the ultimate composition of the dry white lead, especially as the water usually contains free acetic acid and acetate of lead obtained from the corrosions. In proof of this, I submit the following analyses of dry white lead from different manufactories, which agree very well with those published by Professor Mulder:—

	No. I.	Calculated.	Equivalent.
CO ₂	11.03	11.4	= 2
HO	2.23	2.3	= 1
PbO	86.11	86.3	= 3
	No. II.	Calculated.	Equivalent.
CO ₂	12.17	12.7	= 3
HO	1.66	1.7	= 1
PbO	85.37	85.6	= 4
	No. III.	No. IV.	Calculated. Equivalent.
CO ₂	13.37	13.48	13.4 = 4
HO	1.11	1.46	1.4 = 1
PbO	84.71	84.88	85.2 = 5

The hygroscopic water is generally not more than 0.50 per cent.

No. 1, London white lead; No. 2, Newcastle ditto; Nos. 3 and 4, Sheffield ditto.

I do not mean to assert that the product of these manufactories is always of the same composition; for I have made analyses of the Sheffield white lead in which the oxide of lead comes up to 86 per cent. The other constituents were not estimated; but this is sufficient to show that it was then near the composition of the London sample, No. 1. This variation in the manufactured white lead will be still better understood on consideration of the following analyses of corrosions. These have been selected to show the variability of their composition, although Nos. I. and II. may be considered of the normal kind.

No.	CO ₂ .	HO.	PbO.
1. Good solid corrosion	12.49	1.60	85.24
2. " " "	12.31	1.72	85.77
3. Shelly surface of corrosions	15.14	0.53	83.86
4. Hard, flaky corrosion	15.14	0.60	84.10
5. Colourless, crystalline, and transparent corrosions	15.71	0.75	83.53
6. Colourless, transparent crystals, picked out from a mass of sugary corrosion	16.11	0.49	83.39
7. Calculated neutral carbonate	16.50	—	83.50

It will be seen that Nos. 5 and 6 are evidently the neutral carbonate containing only a trace of hydrate. Nos. 3 and 4 illustrate the passage from the latter to the normal corrosion, which may be represented, if a formula is desired, by $\text{PbOHO} + 3\text{PbOCO}_2$.

Lead Works, Sheffield.

On Ozonised Hydrogen and Oxygen, by M. H. OSANN.¹

THE hypothesis having been published, that the author's ozonised hydrogen might derive its more energetic reducing power from the production of one of the inferior acids of sulphur accompanying the hydrogen disengaged from the voltaic battery, M. Osann endeavoured to find whether this explanation was tenable. After passing a current of ozonised hydrogen during an hour through a certain quantity of water, he found that the water remained neutral, which would not be the case supposing the gas were mingled with an acid of sulphur.

¹ *Journal für Praktische Chemie*, vol. XXXI., p. 20, 1860, No. 17.

He then wished to take account of the relative quantities of ozonised hydrogen absorbed by a solution of silver, and of the reduced silver. Electrolytic hydrogen, twice washed in distilled water, was left for some time over a solution of nitrate of silver. The gas was measured before and after absorption, and the tension of the water vapour estimated, and the reduced silver weighed. This silver did not contain the slightest trace of sulphur, which is also in contradiction to the hypothesis in question. It has been found that on an average 100 cubic centimetres of absorbed gas precipitate 1.43 grammes of metallic silver. (The results varied from 1.14 grammes to 1.80 grammes.) The proportion of absorbed gas being 1.42 to 3.52 per cent. of the volume employed. Regarding 1.43 grammes of silver as equivalent to 100 cubic centimetres,—that is to say, to 0.0038 grammes of ozonised hydrogen, 0.66 will be found the equivalent of ozonised hydrogen, or $\frac{2}{3}$ ds of the equivalent of ordinary hydrogen.

These results led M. Osann to revert to an opinion he had abandoned after Mr. Andrews' experiments, and to admit that ozone and ordinary hydrogen have different equivalents.

M. Osann thought he could establish the equivalent of ozone by analysing the yellow body produced when ozonised oxygen is passed through a solution of oxide of lead in caustic soda, or when the same solution is decomposed by the electric pile. According to the author, this body is characterised as a combination of ozone by the property it has of turning blue the alcoholic tincture of guaiacum. The most simple hypothesis possible touching this body is, that it is composed of oxide of lead and ozone ($Pb\ O, Oz$).

There has been found in the yellow body obtained by ozonised oxygen,—

Lead, 89.52. Oxygen, 10.47.

Giving 4.27 as the equivalent of ozone. The yellow body obtained by the electric pile contained,—

Lead, 88.86. Oxygen, 11.14,

from which 5.16 may be deduced as the equivalent of ozone.

From these two experiments, the author concludes that the mean is 4.71, which he finds agrees very nearly with the results formerly obtained in his numerical determinations on ozonised hydrogen and ozone.

We have reported, with some detail, M. Osann's experiments, though it is impossible for us to admit his interpretation of them. His first analysis would lead to the formula Pb_2O_3 , which requires $Pb\ 89.6, O\ 10.4$. It results from this, that the yellow substance analysed appears to be a kind of minium, having PbO_2, PbO for its formula.

Besides, it is evident that if ozone were an allotropic modification of oxygen, its atomic weight would be that of oxygen or of one of its multiples.

Analytical Notes.

Estimation of Nitric Acid.—Fresenius dissolved a known weight of fine piano-wire in an excess of muriatic acid, contained in a retort through which hydrogen passes; the nitrate is afterwards thrown in, and, after the dark colour has disappeared, the amount of protoxide in the cooled solution is determined by permanganate of potassa. The quantity of peroxide is now easily calculated, and from it the amount of nitric acid by which it

had been peroxidised.—*Ann. d. Chem. und Pharm.*, xxx. 217—219.

Estimation of Silver in the Metallic State.—Fellenberg proposes to dissolve the chloride of silver in ammonia, dilute with water, precipitate by a strip of bright copper, collect and wash the silver upon a filter, incinerate and fuse with borax; or to incinerate the filter containing the chloride, without fusing the latter, and reduce it by means of pure lead upon bone ashes. These processes avoid the tedious drying of the filter and the precipitates.—*Schweiz. Zeitschrift*, v. 121—122.

Estimation of Arsenic.—A solution of pentasulphide of arsenic (As_2S_5) in sulphide of ammonium yields instantly a precipitate of ammonio-arsenate of magnesia by a solution of magnesia. Lenssen states that the sulphides of tin and of antimony are not precipitated under the same conditions.—*Ann. d. Chem. und Ph.*—*Wittst. V. Schrift*, ix. 574.

Separation of Phosphoric Acid from Sesquioxide of Iron and Alumina.—W. Mayer states that the requisite proportions to effect this separation are, for 1000 cubic centimetres 15 grammes (1 equivalent) tartaric acid, 6 grammes (1 equivalent) anhydrous sulphate of magnesia, and 16.5 grammes (3 equivalents) of chloride of ammonium. With other proportions ammonia produces a precipitate containing besides ammoniophosphate of magnesia, also basic tartrate of magnesia.—*Ann. d. Ch. und Pharm.* xxv.

Phosphomolybdate of Ammonia.—E. Spiess, of Munich, and Professor V. Eggertz, of Fahlun, corroborate Nuntzinger's analysis of this compound, who calculated, after drying at $212^\circ F$, the following formula:— $4(H_2NO, 10MoO_3) + (H_2NO + [8MoO_3, 2PO_3]) = 3.577 NH_4O, 3.962 PO_3, 46.1 MoO_3$.—*Wittst. V. Schrift*, ix. 527—429.

Anhydrous Oxalic Acid = C_2HO_4 is, according to Erdmann, obtained at ordinary temperature over sulphuric acid; it is proposed by him for preparing the normal acid of volumetric analysis.—*Journ. f. prakt. Chem.* lxxv. 4, 5.

Silicon in Iron.—The black residue left after dissolving crude iron in muriatic acid was found by Schafhäütl to evolve hydrogen from ammonia. Wöhler has proved it to contain oxide of silicon.—*Ann. d. Chem. und Ph.*, xxviii. 374.

Reaction of Strychnia.—The solid strychnia when mixed with solid iodic acid or iodate of potassa and a drop of strong sulphuric acid assumes, according to X. Landerer, on the application of a gentle heat, a beautiful violet colour, gradually changing to red-brown, remaining unchanged for many days. The thick liquid shows, after a few hours, a beautiful iridescence, continuing for several days.—*Hirzel's Zeitschrift*, xii. 86.

Protochloride of Iron is obtained pure, according to E. Amaler, by evaporating the recent solution almost to dryness, and adding powdered iron and strong muriatic acid in the proportion of one part of the former to five of the latter; the mixture is then evaporated to dryness with continued stirring by an iron spatula. The nascent hydrogen evolved will effectually reduce all the sesquioxide of the protochloride and the dry salt will keep for a considerable time.

Tin in Vinegar.—Investigations caused by the State authorities in Potsdam have developed the fact that pure vinegar carried in bright tin measures from the cellar to the store contained a small quantity of tin, which increased on standing, and was considerable after boiling. Tin measures may probably be frequently the cause of

the deleterious effects of vinegar, looked for in other circumstances.—*Arch. d. Med. Ges.—Arch. d. Pharm.*, cli. 115.

TECHNICAL CHEMISTRY.

Report on the Sanitary Condition of the City of London, for the Quarter ending June 22, 1861, with Table of the Composition of the River, and the Water from City Pumps, by H. LETHEBY, M.B., M.A., Ph.D., Medical Officer of Health for the City of London, &c., &c.

Meteorology and Condition of the River.—The mean temperature of the season has been about one degree below the average of former years (51.4°), and the thermometer has ranged from 82° to 36° . The daily range, however, has sometimes been as high as 24° . This, conjoined with the humidity of the atmosphere, which has averaged 85 per cent. of saturation, has been the cause of the large increase of lung diseases.

In the Spring Quarters of 1858 and 1859, when the river was so offensive, the average humidity of the air was only 68 and 69, and the mean temperature was 57.6° and 59.8° . During those quarters the warmth of the river, and the evaporation from its surface were considerable: indeed in the month of June of 1858 the temperature of the water was 72.4° , and in the corresponding month of 1859 it was 67.2° . In the succeeding months it rapidly rose to the temperature of 75° , and then the putrefaction of the water was at its highest; but during the quarter which has just expired, as well as that of 1860, the temperature of the river has not exceeded 62° , and there have been but slight signs of putrefaction. The conditions, therefore, which seem to be necessary for a high state of decomposition in the river are, a continued absence of rain, a hot spring, and a dry condition of the atmosphere. The temperature of the water must be over 60° , the rainfall less than 2 inches in the month, and the humidity of the air below 80° . With these conditions there is not only a diminished supply of fresh water from the upper tributaries of the river, but the volume of the stream is still further diminished by rapid evaporation from its surface. Under these circumstances the daily supply of sewage becomes concentrated, the downflow of the current arrested, and oceanic water rises in the bed of the river to maintain its tidal level. Then it is that the mixture of sewage and sea-water putrefies and becomes a nuisance. This state of things may be predicated not only by the temperature and dryness of the air, but also by the chemical composition of the water; for when the proportion of saline matter, at high tide at London Bridge, amounts to more than 60 grains per gallon, there is imminent danger of the river becoming offensive. This is the result of almost weekly observations made over a period of four years. As far as I know, there have not been any similar consecutive examinations made, and therefore they may be of service when the question arises as to the influence of the new system of drainage on the state of the river. Last year, and up to the present month of this year, the river has been in a normal condition; for its composition has not been very different from that recorded in the early analyses of Dr. Lambe and Dr. Bostock. In the course of the whole year, the proportion of saline matter dissolved in the river-water at high tide at London Bridge has amounted to only 28.9 grains per gallon, and the organic

matter to 3.3 grains. The range has been from 19.6 to 57.3 , and the organic matter from 2.6 to 5.5 . The chief constituents of the saline matter have been as follows:—carbonate of lime 12 grains, sulphate of lime 4.5 grains, common salt 10 grains, alkaline nitrates about 2 grains, and ammonia in combination 1.3 grains. The quantity of suspended matter in the water has averaged only 6.4 grains to the gallon. At times it has risen to from 12 to 15 grains, but this has happened during high winds, as at the time of the equinoxes, when the surface of the water has been ruffled, and the mud upon the shore disturbed. The principal constituent of the suspended matter is ferruginous clay, and with this there is combined about one-fifth of its weight of organic impurity. It is remarkable that the river water near to the shore is always a little less impure than at mid-stream. This arises from the influx of fresh water, and perhaps also from the purifying influence of the atmosphere on the thinner stratum of liquid; as well as from the circumstance that the main current of the rising tide is in the middle of the stream.

The Water Supply and City Pumps.—In the course of the quarter, I have examined the water supplied to the City by the New River Company, and I have also analysed the water furnished by 22 of the City pumps. The first-mentioned supply has not been objectionable; for the quantity of saline matter dissolved in the water has amounted to only 17.4 grains per gallon, and the organic matter to less than 2 grains (1.9). The chief constituents have been as follows:—Carbonate of lime, 12.6 grains per gallon; sulphate of lime, 1.9; alkaline sulphate, 0.2; alkaline chloride, 1.2; alkaline nitrate, 1.0; silica and alumina, 0.5; and organic matter with traces of ammonia, 1.9. These results do not differ very materially from those obtained by Dr. Dundas Thomson, and reported in the Weekly Returns of the Registrar-General; and they show that the water is of good quality and is well suited for domestic purposes. Another advantageous property of the water is, that it is entirely without action on lead, and may, therefore, be stored in lead vessels, and distributed by lead pipes, without danger to the public health.

The quality of the water supplied by the City pumps is very different from that of the New River, for instead of an average of only 19 grains of solid matter per gallon, the water contains from 20 to 127 grains. Taking the pumps in the order of their foulness, they stand as follows:—Bishopsgate Street Without, 127.3 grains per gallon; Aldgate pump, 109.5; Cock and Hoop Yard, Houndsditch, 89.2; Bishopsgate Street Within, 89; Bride Lane, 84.7; Bow Church Yard, 84.5; Fenchurch Street, 83.1; Little Britain, 83; Basinghall Street, 81.6; Chequer Yard, Dowgate Hill, 81.1; Bell Yard, Gracechurch Street, 80; Idol Lane, 78.5; Ironmonger Lane, 76.2; Bartholomew Lane, 75.3; Cornhill, 74.8; Bowling Square, White Lion Street, 71; Half-moon Passage, 70.5; Great St. Helen's, 53.3; Gutter Lane, 45.7; Honey Lane Market, 44.2; Guildhall Buildings, 28; and Glovers' Hall Court, 19.8. The quantity of organic matter in them ranges from 1.5 grains per gallon to 8.8; the common salt from 2.7 to 25; the sulphate of lime, from 2 to 29; the alkaline nitrates, from 2.1 to 24.6; and the combined ammonia from 0.5 to 2 grains per gallon. These results show that the City pumps are not only charged with decaying organic matter, but also with the saline products of its oxidation; the ammonia, for instance, is a sign of present putrefaction, and the alkaline nitrates of a past; besides which, the existence of so large a quantity of common

salt is suggestive of the filthiest impurities, as, for example, the fluid matters discharged from the human body, and the percolations from cesspools and sewers. Most of these waters are bright and sparkling, and they have a cool and agreeable taste. They are, therefore, much sought after for drinking purposes; but the coolness of the beverage and the briskness of its appearance are dangerous fascinations, for they are both derived from organic decay. Dead and decomposing matters have accumulated in the soil, and have been partially changed by its wonderful power of oxidation, and thus converted into carbonic acid and nitre. These have given to the water the agreeable qualities which are so deceptive. In reality, the water from the City pumps is far worse than that from the muddy river, from which it is in great part derived; indeed, it may, at any moment, become charged with the active agents of disease; for no one can say when the salutary influence of the soil may fail, by being worn out or over-taxed, and then the putrid organic compounds will pass into the wells unchanged. Many of the pumps are in close proximity to the fat graveyards of the City, and it is more than probable that all of them derive a portion of their water from these sources, for they are the principal gathering grounds for the surface springs; in fact, they are the only open spaces through which the rain can percolate to reach the shallow wells. Hereafter, when I have completed the inquiry into all the City pumps, I shall discuss this matter more fully; now, however, I merely direct attention to the general foulness of the water, and to the danger which may arise from the use of it. In the following Table I have shown the composition of those wells which have already been examined:—

Table of the Mean Composition of the New River Water as supplied to the City, during the Spring Quarter of 1861; of the Thames at High Tide; and of the Water from some of the City Pumps.

Source of the Water.	Constituents per Imperial Gallon.										Total.
	Carbonate of Lime and Magnesia.	Sulphate of Lime.	Alkaline Sulphate.	Alkaline Chloride.	Alkaline Nitrate.	Silica.	Alumina, &c.	Organic Matter.			
	grs.	gr.	gr.	gr.	gr.	gr.	gr.	gr.			
New River Company	12.6	1.9	0.2	1.2	1.0	0.5	1.9	19.3			
Thames, High-tide at London Bridge	11.7	4.3	0.6	11.1	2.0	0.4	3.7	33.8			
Pumps:											
Bishopsgate St. Without	34.5	27.4	7.9	24.6	24.6	1.9	6.4	127.3			
Aldgate	26.2	25.8	..	25.0	24.3	1.1	7.1	109.5			
Cock & Hoop Yard, Houndsditch	26.6	20.0	..	14.7	10.0	1.0	6.9	89.2			
Bishopsgate Street Within ..	24.7	17.0	..	17.5	20.2	1.3	5.3	89.0			
Bride Lane	11.5	21.5	..	22.5	19.5	0.9	8.8	84.7			
Bow Churchyard	29.6	15.1	4.2	21.2	7.6	0.9	5.9	84.5			
Fenchurch Street	21.0	18.3	..	14.2	22.4	0.6	6.6	83.1			
Little Britain	27.9	10.2	15.2	13.8	5.3	1.9	8.2	83.0			
Basinghall Street	21.4	20.0	3.0	12.6	7.8	0.9	4.9	81.6			
Chequer Yard, Dowgate Hill ..	15.9	18.9	..	12.4	24.6	1.3	8.0	81.1			
Bell Yard, Gracechurch St. ..	19.1	12.8	..	19.1	19.6	1.0	8.2	80.0			
Idol Lane	16.8	16.6	..	16.3	22.4	1.8	4.6	78.5			
Ironmonger Lane	19.8	16.3	5.2	10.6	7.0	1.6	5.7	76.2			
Bartholomew Lane	21.3	14.1	0.4	16.7	15.1	0.9	4.6	75.3			
Cornhill	25.0	15.7	1.1	14.0	12.9	1.6	4.5	74.8			
Bowling Square, White Lion Street	18.0	25.2	5.0	8.2	7.8	1.1	5.7	71.0			
Half-moon Passage	18.0	14.5	..	18.7	15.7	0.9	2.7	70.5			
Great St. Helens	15.8	9.5	0.7	8.1	14.0	0.8	4.4	53.3			
Gutter Lane	15.5	9.0	1.6	5.7	7.9	0.9	5.1	45.7			
Honey Lane Market	15.1	6.1	3.7	11.4	4.4	1.4	2.1	44.2			
Guildhall Buildings	13.4	1.9	3.0	5.3	2.1	0.3	2.0	28.0			
Glovers' Hall Court	10.9	2.0	..	2.7	2.2	0.5	1.5	19.8			

In every case the Water from the City Pumps was clear, and of a slight greenish tinge. It was exceedingly hard, and was without action on lead.

Sanitary Work.—The proceedings have had the effect of checking the supply of bad meat to the markets, for the total amount seized during the quarter is not half so much as that of the preceding quarter, and during the last month the quantity has fallen off very considerably.

Arrangements have been entered into with Mr. Ford, of Bell Isle, for the destruction of the meat thus seized; and to ensure its not being used as food, a chemical treatment is about to be adopted, before the meat leaves the custody of the Inspectors.

The last point to which I have to refer is the working of the Act for preventing the sale of adulterated food. Only six applications have been made to me during the quarter for the analysis of articles suspected to be adulterated; two of these were for beer, one for bread, one for milk, and one for cooked ham. Both of the samples of beer were of inferior quality, and one was adulterated with a sweet material, like liquorice; the bread also was of inferior flour, but the other things were sound.

On the Origin and Progress of the Phosphorus and Match Manufactures, by G. GORE.

(Continued from page 47.)

THE following is the general process of manufacture of matches as now in use:—The wooden splints for the matches are prepared from the very best quality of pine planks, perfectly desiccated at a temperature of 400° Faht. Each plank is cut into about thirty blocks about eleven inches long, four and a-half inches wide, and three inches thick. A block is fixed with its small end downwards between two uprights, the grain of the wood being horizontal. A frame, having fixed upon its upper surface about thirty lancets one-eighth of an inch asunder, with their points projecting upwards one-eighth of an inch, reciprocates to and fro beneath the end of the block of wood by means of a crank, and in doing so scores the whole of the lower surface of the block with a number of parallel cuts, about one-eighth of an inch apart and one-eighth of an inch deep, in the direction of its fibres. Immediately upon this a sharp blade, or scythe, swings round horizontally beneath, and cuts off the scored surface to the thickness of one-eighth of an inch, in the form of square splints four and a-half inches long, which fall through a shoot into a room beneath. The block now sinks one-eighth of an inch and the operation is repeated. In this way between two and three millions of splints are readily formed by steam power in a single day. English splints are of two sizes—large and minikins: the larger ones are two and a-quarter inches long (or double-length, as first formed, i.e. four and a-half inches), and the minikins are rather shorter.

In England the splints are square in form, and produced as above; but in Germany they are cylindrical, and are prepared in this shape by placing the small block of wood with the ends of its fibres downwards upon a strong plate of steel of larger surface, perforated over the greater part of its surface with holes of the size of the splint, countersunk at their entrances to form cutting edges, and as close together as possible. Great pressure being now applied to the upper end of the block, it is forced through all the holes, and reappears on the lower side of the steel plate in the form of a heap of cylindrical splints.

Returning to the English splints—they are collected

as they are cut, and measured into bundles of six dozen each by a man who seizes a number of them in his hands from the loose and irregular heap, and by dexterous jerking, knocking, and shaking, arranges them parallel to each other, then places them in a short open gutter or cradle, across which he has previously thrown a piece of string, and when the measure is full to the level of its edges he ties up the bundle, throws it aside, and proceeds with another with equal rapidity. The bundles are then perfectly dried by exposure for several hours in a heated oven.

The bundles of splints are next coated at both ends with sulphur. A quantity of brimstone is melted in an iron pot, and one end of a bundle being quite scorched by contact with a red-hot plate is immersed into the melted sulphur to the requisite depth; then repeatedly swung in the air, to throw off the superfluous sulphur, and rolled violently upon its axis and beaten, to prevent the splints adhering together in one mass as the sulphur hardens. The other end is then similarly treated. In some cases the ends of the splints are saturated with wax instead of sulphur in a similar manner.

A child now arranges the splints in a number of successive rows, one above another, in a frame, each splint separate from the others, in the following manner:—He takes a narrow wooden frame composed of three fixed pieces, a narrow bottom cross-piece about sixteen inches long and two inches wide, with two vertical pegs about eight inches high, one at each end; and has by his side about two dozen loose strips of wood of similar length and width to the bottom piece, and with holes or notches at their ends to receive the upright pegs; each of these strips has a series of fifty transverse shallow notches upon its upper surface to receive fifty splints, and is covered on its lower side with felt or other soft material. Having placed one of these strips upon the bottom piece, he takes up a handful of splints and quickly deposits fifty of them, one in each groove, their ends projecting one and a-quarter inches on each side; he then places another strip upon the first one, and fills it in like manner, and so on until the pile of twenty-four rows is complete. A thicker strip of wood, similar to the bottom piece, covered upon its lower side with felt, is then placed upon the top, and the whole secured moderately tight by means of thumb-screws, or pegs fitting into holes in the uprights. The splints are then ready for being tipped at their ends with the phosphorus composition. Many children are employed at this kind of labour.

The exact ingredients, and their proportions, in the phosphorus composition, differ in different countries and with different manufacturers, but they all consist essentially of emulsions or mixtures of phosphorus in a solution of glue or gum arabic, with or without other matters for increasing the combustibility, for colouring, &c. In England the composition contains a considerable quantity of chlorate of potash, which imparts a snapping quality, and noisy projecting flames, and but little phosphorus, on account of the moisture of the climate; other substances are also added to give hardness and power of resisting moisture. The following is about the composition of the best quality:—

Water . . .	4	parts by weight.
Glue . . .	2	"
Phosphorus . .	1½ to 2	"
Chlorate of potash .	4 to 5	"
Powdered glass .	3 to 4	"

In Germany the proportion of phosphorus used is much larger, and gum is used instead of glue, together with

nitrate or peroxide of lead, and no chlorate of potash. In consequence of the presence of so much phosphorus and the absence of chlorate of potash, the German matches light quietly, with a mild, lambent flame, and are injured quickly in a damp place by the oxidation of the phosphorus and the production of phosphoric acid, which attracts moisture. One of their mixtures, given by Böttger, is composed as follows:—

Phosphorus . .	4	parts by weight.
Nitrate of potash .	10	"
Fine glue . . .	6	"
Red ochre . . .	5	"
Smalt	2	"

In every case the glue is first broken into small pieces and soaked in cold water until quite soft, then added to the requisite quantity of water and heat applied by means of a water-bath until it is quite fluid, and at a temperature of 200° to 212° F. The vessel is then removed from the fire, the phosphorus gradually added, and the mixture agitated briskly and continually with a stirrer, having wooden pegs or bristles projecting at its lower end. The chlorate of potash, colouring, and other matters, all in a state of very fine powder, are then added, one at a time, to prevent risk of accident, and the stirring continued until the mixture is comparatively cool. By this means the minute particles of phosphorus are kept from uniting into large globules, and the heavier matters are prevented from sinking to the bottom.

In the process of tipping the ends of the matches (termed in the manufactory "dipping"), a quantity of the composition is spread, by means of a large palette-knife, upon the horizontal surface of a smooth stone or plate of iron, heated by means of steam beneath. The mixture becomes much more fluid by the heat, and is spread to a width and length of about ten inches by twenty, and a uniform depth of about one-eighth of an inch by means of a horizontal gauge. The "dipper" then takes a frame filled with splints, as already described, strikes the ends of the splints upon a bare part of the stone until the whole of those ends exhibit a flat, uniform surface. He then dips those ends, either once, twice, or more times, into the mixture, until they have all received a proper amount, and hands the frame to a boy, who places it upon a rack in a current of air, in order that the composition may cool and harden. Meanwhile the dipper proceeds with other frames in like manner, and replenishes the layer of composition occasionally. After a number of frames have been thus dipped, and the composition upon the splints in them has partly hardened, they are all taken, one by one, in the original order, and the other ends of the splints tipped in a similar manner. With six dipping-stones, half-a-dozen workmen have dipped as many as twenty millions of matches in a single day. This method of dipping is termed "frame-dip"; but there is another process called "bundle-dip," which consists in taking a bundle of the sulphured splints, twisting it in such a manner that the ends of the splints spread out like a brush and separate from each other; they are then tipped with composition like the others. In this process the *ends* only of the splints acquire a coating of the mixture, whereas in frame dipping the composition extends a short distance up the *sides* of the splints and is more firmly attached. Bundle-dipped matches are inferior to frame-dipped ones.

In the manufacture as now being described it will be observed that each splint is 4½ inches long, coated with composition at both ends, and will therefore form two

matches by being cut asunder in the middle; but this is not always the mode of proceeding; in some manufactories the splints are cut in two before being dipped, and are tipped at one end only.

As soon as the composition is partly dry, and of such a degree of hardness as to bear the next operation without igniting and without rubbing off, the splints (those coated at both ends) are quickly removed from the frames into parallel heaps, and the frames set at liberty. A man now takes a heap of fifty of the splints, places them between two vertical supports in a horizontal channel or trough of wood, passes a string with a weight over them to prevent their falling about, and cuts them through at their middle by means of a cork-cutter's knife, moving upon a hinge like a chaff-cutter's scythe. He then places them in heaps of 100 matches each, ready for boxing. The little boxes are now filled by women and children, who perform the operation with exceeding rapidity, and before the matches are dry, in order to avoid accidents. Each match passes through the hands of about seventeen persons, chiefly children, in its process of manufacture.

Trays of sawdust are placed near the various workmen and operators to plunge the matches into in case they ignite. Vessels of water are also freely at hand in case of accident. Also a quantity of sand is strewed in a layer of several inches deep beneath the racks upon which the frames of matches are set to harden, so that in case of a frame of matches falling, the flames may be quickly extinguished. A lotion, composed of olive oil and lime water, is exceedingly useful to apply to the skin in cases of burning by phosphorus.

(To be continued.)

Working in Aluminum.

WE extract the following valuable article from the *Ironmonger*. The information is obtained from Messrs. Bell and Brothers, Newcastle-on-Tyne, manufacturers of aluminum.

The peculiar properties of this substance having been so little understood, has hitherto hindered its general employment, but now that it is sold in a pure state at as low a rate as 50s. per pound avoirdupois, it is likely to be much more frequently used.

Aluminum is a metal of fine white colour, slightly inclining to blue, especially after being well hammered when cold.

Aluminum, like silver, is susceptible of a very fine "matting," which is not affected by exposure to the air, or by any of the impurities usually present in the atmosphere of towns. To obtain this matting, the aluminum objects (being previously washed in benzole or essence of turpentine) must be plunged into a weak solution of caustic soda, thoroughly well washed, and exposed to the action of strong nitric acid. When the desired matting has been obtained, it must be well washed again, and dried in sawdust.

Aluminum is easily polished or burnished. To do this, it is necessary to use a mixture of equal parts of rum and olive oil, as an intermediate substance between it and the polishing stone or powder used. The polishing stone is steeped in this mixture, and will then burnish aluminum in the same manner as gold and silver is burnished, care being taken not to press too heavily upon the burnishing instrument.

Aluminum can be beaten out, either hot or cold, to the same extent and as perfectly as gold or silver; and

it is susceptible of being rolled in much the same way as either of the above metals. Leaves as thin as those used for gilding and silvering can be made of aluminum. Covered ingot moulds of iron answer best for receiving aluminum intended to be used in the rolling mill. Aluminum quickly loses its temper, and therefore requires frequent reheating. The temperature of this reheating is a dull red heat, and when the plates become very thin, this demands the greatest attention.

Aluminum is easily drawn into wire. For this, the ingots are run into an open mould, so as to form a kind of quadrangular shape of a little less than half-an-inch section, which is then beaten upon the edges by the hammer very regularly; the operation of drawing out is then commenced on a horizontal bench, by very gradually reducing the diameter of the metal intended to be drawn into wire, and by frequent reheating, and then the ordinary process of wire-drawing can be proceeded with. When the threads are required extremely fine—as, for example, for the manufacture of lace—the heating becomes a very delicate operation, on account of the fineness of the threads and the fusibility of the metal. The heat of the current of air issuing from the top of the glass chimney of an Argand lamp will suffice for the heating.

The elasticity of aluminum is very much the same as that of silver, and its tenacity also about the same. The moment after it has been melted aluminum possesses about the hardness of pure silver; when it is hammered out, it almost resembles that of soft iron; it becomes elastic, acquiring, at the same time, considerable rigidity, and emits the sound of steel when suffered to fall on a hard body.

A property which aluminum manifests in a high degree is that of excessive sonorousness. This property has already rendered it of service in the construction of several musical instruments.

Aluminum is much lighter than ordinary metals. Its density is 2.56, a quarter that of silver, and about a third that of iron. By the action of the hammer, the density of aluminum increases sensibly, so as to become equal to 2.67.

Aluminum melts at a higher temperature than zinc, and a lower one than silver; to melt it, an ordinary earthenware crucible must be employed, without the addition of any sort of flux.

Its low point of fusion, along with its slowness of heating, require that for melting it a less intense fire should be used, but applied for a longer time than in melting silver.

It is easily melted in an open crucible, which facilitates the removal of the dust and other impurities which appear on the surface of the metal; and for the purpose of stirring the entire mass, a clean iron spatula is used.

Aluminum is easily run into metallic moulds; and, still better, for objects of a complicated form, into moulds of dry porous sand, formed so as to allow an easy passage for the air expelled by the metal, which is viscous when melted. It ought to contain a greater number of passage holes, and should be so managed as to run it in one long and perfectly cylindrical gut. When heated to a red heat, it ought to be poured out with tolerable rapidity. A small portion of the fused metal should be caused to run into the gut itself when full, to compensate for the contraction of the substance of the metal at the moment of solidification.

By following all these precautions, castings of the highest degree of fineness may be obtained; but, at the

same time, to succeed perfectly, an especial acquaintance with the subject is needed.

In the production of work where the use of the lathe becomes necessary, any scratching or tearing of the metal by the tool is avoided by covering the surface to which the tool is applied with a varnish composed of stearic acid and essence of turpentine.

When aluminum is soiled by greasy matters, it can be cleaned by benzole; if it be soiled by dust only, india-rubber, or very weak soap and water may be used.

The pieces of aluminum intended to be soldered must be prepared in the same manner as objects are treated for soldering with tin, viz., by a "tinning;" but it must be remembered that it is indispensable that this tinning must take place with the solder itself. The pieces to be soldered, thus tinned beforehand, are afterwards joined together, and exposed to the flame either of a gas blowpipe, or any of the ordinary sources of heat used in such cases. In order to unite the solderings, small tools of aluminum are used. These tools are used as little soldering instruments, and they facilitate at the same time the fusion of the solder, and its adhesion to the previously prepared aluminum.

The use of tools of copper or brass used when soldering gold and silver, must be strictly avoided, as they would form coloured alloys with the aluminum and the solder. It is of the greatest importance never to use any flux to cause the solder to melt, as all those at present known attack aluminum, and prevent the adhesion of the pieces to be soldered. The use of the little tools of aluminum is an art which the workmen must acquire by practice; in fact, at the moment of fusion the solderings must have the friction applied, as they melt suddenly in a complete manner. In soldering aluminum, it is well to have both hands free, and to use only the foot for the blowing apparatus.

Solders of different compositions and degrees of fusibility have been employed in soldering aluminum. The following are those which have been generally used, ranged according to their order of fusibility:—

	I.	II.	III.	IV.	V.
Zinc . . .	80	85	88	90	94
Copper . . .	8	6	5	4	2
Aluminum . .	12	9	7	6	4

No. 4 is the one usually preferred, particularly for soldering smaller objects.

In order to make the solder, the copper is first melted, the necessary aluminum is added, and stirred by means of an iron spatula, unpolished, as it comes from the blacksmith, adding also a little tallow; the zinc is then added, avoiding too much heat, as this last metal is easily oxidized, and is very volatile.

NOTICES OF BOOKS.

Water: its Impurities and Purification. By F. HAHN DANCHELL. London: H. Renshaw, 356, Strand, W.C.

UNDER the above title a little work has been published which will be found of considerable use to those of our readers who, without going to the expense or trouble of obtaining a complete qualitative or quantitative analysis of a water, may still desire to know by a few simple tests what injurious substances are likely to be present, and how they are to be got rid of. The work is intended to accompany a simple water-testing apparatus, which consists of a selection of tubes and bottles of various appropriate tests of definite strength, so that by adding to

successive tubes full of water a certain number of drops of each test in rotation, the presence or absence of ammonia, decomposed organic matter, lead, carbonate of lime, sulphate of lime, sulphuric acid, and iron, may be ascertained. The author states in his preface:—"To the head of every family it presents a ready means of ascertaining the purity of that substance which is the most extensively used, and the most absolutely essential of all those required for the support of our existence; to the keeper of horses and cattle it will, I believe, point out many of the hitherto concealed sources of the mysterious epidemics and diseases from which such severe losses have arisen; the agriculturist, by its means, will be enabled to determine the probable advantages to be derived from the use of any particular water for irrigation; the manufacturer may ascertain the nature and suitableness of the water supplied for his use; whilst to emigrants, about to choose their future home, it will have an especial value, as indicating the localities which, if health be valued, are to be chosen or avoided. And although my little apparatus has no pretensions to supply the place of, or dispense with the necessity for, an accurate qualitative and quantitative chemical analysis, I yet venture to think it will be found of use by medical and scientific men, who may have occasion to ascertain, in a ready way, whether any of the more usual impurities are present or not in water. The tests being all made dependent upon colour, and the mode of operating being merely the addition of a few drops of the test fluids to water, no chemical knowledge is required for their application. The tests are all sufficiently delicate and accurate for practical purposes, and the apparatus is extremely portable, and conveniently arranged. I have by means of these tests placed it in the power of the public to ascertain for themselves whether the representations made by myself and other filter makers are well founded or not. By means of these tests, not only can the condition of the natural water be ascertained, but the state of purity in which it is left after passing through the filtering medium may be ascertained with just as great certainty and ease."

The importance of a plentiful supply of pure water cannot be overrated. For domestic purposes, such as cooking and washing, and for manufacturing purposes, such as distilling, brewing, tanning, dyeing, starch making, paper making, and chemical works, the main use of water is as a solvent. Anything, therefore, which detracts from its solvent powers, must more or less unfit it for the purposes above referred to. It is well known that hard water requires more soap to wash with—that it necessitates the use of more tea and coffee to make our everyday beverages—more meat to produce soups and gravies—more malt to give us spirits and beer—more bark for tanning extract—and more chemicals for producing the various extracts—than would be required were perfectly pure, or at least soft water, used. The actual loss from these sources must be something enormous, amounting to many million pounds sterling per annum.

For many manufacturing purposes, such as bleaching, paper making, &c., it is also necessary that the water used should be perfectly free from colour; consequently, where traces of iron or decomposed organic matter are present, they must either be removed or some other source of supply sought out. Great expense is often gone to in sinking Artesian wells, or conveying pure water from a long distance, where no real necessity for such outlay exists. And for the domestic and manufacturing purposes enumerated, it may safely be affirmed, that no natural water is so pure as not to be capable of great improvement by a judicious system of filtration and purification.

After some clear remarks upon the different kinds of impurities present in water and the various means adopted for their separation, the author continues:—

"A water purifying apparatus, then, should be able to act upon the impurities held in solution as well as those

in mechanical suspension. It should be composed of materials incapable of giving any taint to the water passing through it. Its action should be rapid and effective. Its operation attended with the least possible trouble, or necessity for attention on the part of servants. It should be capable of retaining its purifying properties for a lengthened period. It should be so arranged that the filtering medium may be easily got at for cleansing or renewal. And, finally, the price should be such as to place it within the reach of all classes in the community."

It is scarcely necessary for us to state that this is followed by an assurance that the filters made according to Mr. Danchell's plan more than fulfil all the above conditions. Whether this be the case or not, we have had no opportunity of judging, but it is only fair to say that the material upon which he mainly relies as a purifying agent is about the best that could be employed, and that the style in which the little work is written shows that the author thoroughly understands the subject of water purification.

NOTICES OF PATENTS.

Manufacture of Gas. JOHN LESLIE. (Two Patents.)

THESE two patents are of great commercial importance. The first patent has for its object improvements in the manufacture of gas. Heretofore in manufacturing gas from coal and other bituminous mineral substances it has been usual to subject them to the process of destructive distillation, and then to purify the gas obtained. These improvements in the manufacture of gas for the purposes of illumination from Parrot coal, Cannel coal, Boghead coal, and other mineral bituminous matters capable of affording paraffine, consist in subjecting such mineral bituminous matters to distillation at low temperatures, in order to obtain the products distilled over in a condensed liquid form, and then to subject such liquids to processes of purification, in order to fix or remove the ammonia, sulphur, and other impurities, and then to subject the purified liquids to destructive distillation, by which very pure gas is obtained.

In the manufacture of gas from coal by destructive distillation as at present practised in gas works, it is necessary to employ very extensive, and at the same time, very expensive machinery and processes between the retorts where the gas is distilled off and the gasometers wherein the gas is stored for distribution, and the necessity for the use of such machinery and processes arises from the high temperatures employed in distilling over every vaporizable constituent of the coals employed, by which not only is the illuminating gas distilled off, but also very inferior and deteriorating gases, with products of sulphur and other impure matters, which are prejudicial not only in respect to the illuminating powers of the combined gases, but they are also injurious in other respects. And it is for the purposes of purifying the gases from the sulphur and other impure products that gas works have heretofore found it necessary to employ such extensive and expensive machinery and processes. In addition to the above objections, large quantities of tar, as well as of very offensive products, result from the present system of manufacturing gas from coal at gas works, resulting in a great waste in regard to the quantity of good and pure illuminating gas a given weight of coal is capable of affording. According to this invention illuminating gas may be manufactured from coal and other bituminous mineral in such manner as to dispense with the use of the machinery and processes now necessary in gas works for purifying the gases after they leave the retorts, and before they arrive at the gasometers. It

consists in so arranging gas works as to employ in the manufacture of gas the hydrocarbon products of coal or other bituminous minerals obtained by distilling such substances at a low temperature, whereby the patentee is enabled to dispense with the machinery and processes used for purifying illuminating gas obtained by the existing process of destructive distillation of the bituminous mineral; for these purposes Cannel coal, parrot coal, Boghead coal, and other coal, and other mineral bituminous matters, are distilled at a low temperature, in such manner as to obtain the products in a condensed form in place of in the state of gases; then, when necessary, the resulting fluids are purified, and then such fluids are subjected to the action of heat in a finely divided state in retorts or vessels, to convert them into gas, which is conveyed into gasometers such as heretofore used at gas works, in order that the same may be distributed therefrom, as heretofore practised.

Mr. Leslie prefers to use a cylindrical retort heated externally by a fire, such retort being caused constantly to revolve slowly. Into this the coal or bituminous mineral is introduced, broken up into small pieces, and the products evolved pass off to the condensing apparatus, which is constantly kept cool by water, and the condensed hydrocarbon products are received into a suitable receiver or vessel. Care is to be taken to keep down the heat of the retort, in order to prevent the production of gas or vapours which will not condense, the object being to obtain only fluid hydrocarbons by the first process of distillation.

When using the better classes of Cannel coal, the hydrocarbons obtained may at once be employed for the manufacture of gas, but when using hydrocarbons (obtained from less pure coal or other bituminous mineral) which have nitrogenous and sulphur compounds combined therewith, these are purified in the following manner:—To remove the nitrogen the crude hydrocarbons are washed with dilute acid; dilute sulphuric acid will answer the purpose, but dilute muriatic acid is preferable prepared by adding five gallons of water to one gallon of the concentrated muriatic acid of commerce; the hydrocarbons are then agitated violently with this diluted acid, using 15 gallons of the diluted acid to every ton of the oil. The mixed hydrocarbons and acid is then allowed to stand for twelve hours, at a temperature of 90° to 100° Fahrenheit, and then the layer of acid liquor which will have separated must be drawn off. To free the hydrocarbons from sulphur compounds, the patentee uses at the rate of 1 lb. caustic soda with 1 gallon of water, and from 14 to 30 gallons of such solution will be found sufficient to purify a ton of the hydrocarbon fluid; for this purpose the solution is well stirred into the hydrocarbons, and then allowed to settle for some hours, when the purified hydrocarbons may be readily drawn off from the impurities.

In order to convert the hydrocarbon liquid into illuminating gas, it is caused to drop into a retort or vessel heated to a good red heat, and the gas is conveyed from the retort into gasometers of the ordinary construction, from which the illuminating gas is supplied to the gas mains, as gas has heretofore been supplied from gasometers.

By these improvements not only will gas works be rendered less objectionable in any neighbourhood, but the gas obtained will be more pure, and the cost of production will be greatly reduced.

The second patent has principally for its object the purification of gas after it is manufactured. For these purposes, in distilling coal or other substance, the gas, in place of being conducted off from the upper part of the retort, and thence into the hydraulic main, is caused to descend from the retort at the lower part thereof into a chamber, and thence the gas is conducted off by a pipe at the upper part of the chamber to purifiers. Several

retorts may be connected with the same chamber, in which case they each have a slide or other valve to shut the entrance into the chamber. And in purifying gas, a solution of a salt of copper, preferring the sulphate, is employed to saturate wood shavings or other porous material, through and amongst which the gas is caused to pass. The purifying matters thus employed are from time to time subjected to the passage of atmospheric air amongst them to re-prepare them for the further purification of gas therewith.

By the kindness of the Patentee we have been enabled to witness some of the remarkable results which are obtained by these processes. A weighed quantity ($2\frac{1}{2}$ lbs.) of Boghead coal was placed in a retort, which was kept slowly revolving over a fire, the temperature never approaching a red heat, and, indeed, scarcely reaching that of melting lead. The slow rotation of the retort preventing any one portion of the coal from becoming hotter than the rest, the volatile constituents were all evolved in the liquid form, no gas whatever being produced. In a short time the $2\frac{1}{2}$ lbs. of coal had yielded in the receiver $1\frac{1}{2}$ pints of hydrocarbon fluid, leaving $\frac{1}{2}$ lb. of coke in the retort. As soon as the liquid had ceased to come over, it was carried to an iron retort which was kept at a red heat by means of a furnace, the arrangement being similar to that employed in gas-works. The $1\frac{1}{2}$ pints of hydrocarbon fluid was then allowed to drop through a funnel tube into this red-hot retort, when the gas-holder which was in connexion with it instantly began to rise, and within a minute and a-half 25 cubic feet of gas had come into the holder. The next day the luminosity of the gas, which we had seen prepared, was ascertained by means of a photometer. When burning at the rate of four feet per hour, it equalled twenty sperm candles.

The remarkable character of these results becomes more apparent if we calculate by the ton instead of the pound. A yield of $1\frac{1}{2}$ pints for every $2\frac{1}{2}$ lbs. of coal is equivalent to 168 gallons per ton of 2240 lbs. Now 168 gallons is almost exactly one cubic yard, and calculating each gallon to produce, almost instantaneously, 128 cubic feet of gas, we have thus 21,504 cubic feet of gas from 168 gallons, the material for the production of which only occupying one cubic yard of space.

Besides the greatly-increased yield obtained by this process, there are other advantages which recommend it to the serious consideration of Gas Companies. By its means all the refuse coal which is now completely wasted at the pit's mouth may be distilled into oil at the collieries. This fluid may be further purified from sulphur and other deleterious substances on the spot where it is made, whence it could be carried up to London, and converted into gas in the space of a few minutes. The advantages of this would be: the coal being used at the pit's mouth would cost a mere trifle; all the troublesome work of distillation and purification, with its concomitant evils of poisoning the neighbourhood by the offensive odour, could be performed where labour was cheap, and ground plentiful, instead of as at present in the heart of London; the expense of carriage of material to London would be considerably reduced, as only the real gas-making constituent of the coal would be transported; and lastly the complicated machinery of plant and hands, with the sickening odour with which it is always surrounded, would be in great measure done away with, no purifying apparatus being needed, and the mechanical labour of converting any quantity of the hydrocarbon fluid into gas, being reduced to the capacity of "a man and a boy."

Pigments for Coating Surfaces. G. HALLETT and J. STENHOUSE.

This invention consists in the employment of the oxides of antimony, either native or artificially prepared, as

pigments suitable for out-door application, and to be used in much the same manner as white lead. For the preparation of their material the patentees employ at once the native oxide, or that kind of mineral in which a certain quantity of the sulphide of antimony is associated with the oxide. These are crushed to a tolerably fine powder, and purified from the gangue or matrix by mechanical sorting and washing. The heavy ore is dried and subjected to the process of calcination or roasting in a reverberatory furnace with a plentiful supply of air, during which treatment all the sulphur is expelled, and the residual metal converted for the most part into antimonious acid. The calcined product requires to be very finely ground, and mixed with any suitable oil or varnish to form the paint.

In the preparation of antimonial oxides from ores or metallic regulus there is a tendency for the product to become slightly discoloured by the presence, in small quantities, of other metallic oxides (e.g., those of lead, copper, and iron), traces of either of which common impurities operate powerfully in darkening the shade of colour possessed by the pure oxide of antimony. On this account the antimony paint would not be so suitable as zinc or white lead for application whenever purity of tint is desired; but would, nevertheless, seem well adapted for the preparation of stone colour. In all cases where the painted surface would afterwards be liable to the action of an atmosphere impregnated with sulphuretted hydrogen the antimonial stone colour would, in comparison with lead, have the advantage. Although possessing more body than zinc paint, it does not appear to be, in this respect, equal to white lead.

An Impermeable Protective Coating for Leather.

C. STEVENS. A Communication. (Not proceeded with.)

This proposition relates to the employment of gutta-percha and india-rubber, severally dissolved in bisulphide of carbon, for the purpose of coating leather, and thereby rendering it less liable to become deteriorated by the action of the acids usually present in common blacking. Saturated solutions are preferred; that containing india-rubber is recommended to be first applied.

The advantage of employing the bisulphide of carbon in preference to other cheaper and more agreeable solvents for making the varnishes is not very manifest, unless on the assumption that the agent described in the specification must appear to possess the charm of novelty.

CORRESPONDENCE.

Bisulphide of Carbon in Gas.

To the Editor of the CHEMICAL NEWS.

SIR,—My Report on "Bisulphide of Carbon in Gas" has been honoured with such marked notice in the last two numbers of your Journal, that I am obliged to request the favour of the insertion of this letter.

Any writer of an ordinary pamphlet must expect to have his composition criticised, and must submit to be abused or complimented, as the case may be. I therefore ought to be highly flattered on seeing that so much notice has been taken of the Report in question; and I could well afford to let your articles pass unnoticed if my silence would not be taken as a confirmation of the statements made in your pages, and if the articles had been merely simple criticisms. But when I find that experiments have been made, the result of which have been placed in juxtaposition to mine, evidently for the sole purpose of confuting them, I feel justified, before entering into the merits or demerits of either, to ask through you the name of the author of the articles.

I shall be very pleased to accept the challenge implied, but before doing so certainly may expect in fairness my request acceded to, especially as my experiments were vouched for by the attachment of my signature.

I therefore again request to know who is the "we" who has made the "independent examination" from which such wonderful numbers have been deduced, and who seems to possess the peculiar facility of casting insinuations repugnant to the minds of scientific and honourable men.

I am, &c.

FRED. VERSMANN, F.C.S.

Chemical Laboratory, 7, Bury Court, St. Mary Axe, July, 1861.

[The Editor's name is printed on each number of the *CHEMICAL NEWS*, and he takes the responsibility of all Editorial articles. We cannot see that Mr. Versmann has anything to complain of. Far from wishing to cast insinuations to which he, as a scientific and honourable man, could object, we took particular pains in each notice to assure our readers that we did not intend to question the perfect accuracy of Mr. Versmann's results.—Ed. C. N.]

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Explosion in Making Phosphoric Acid.—Elsner relates (*Chem. tech. Mittheilungen*, 1859-60, s. 121) that some time since as a chemist was making phosphoric acid, by acting on phosphorus with nitric acid in a retort, a tremendous explosion took place when the mixture was heated, which not only shattered the retort but blew out all the windows, and even shook the walls of the laboratory. The cause of the explosion he supposes to be the formation of phosphuretted hydrogen from the sudden decomposition of vapour of phosphorous acid into that gas and phosphoric acid. Elsner recommends that the phosphorus should be treated with nitric acid in a large dish, so that the vapour of phosphorus acid may have free escape.

Behaviour of Sulphurous to Iodic Acid.—Dr. H. Kaemmerer describes (*Journal für prakt. Chemie*, Bd. lxxxiii. s. 72) an hitherto unobserved reaction of sulphurous on iodic acid. He took finely powdered anhydrous iodic acid and acted on it for some time by dry sulphurous acid. When the action had continued a quarter or half-an-hour, decomposition commenced, and a small quantity of iodine was set free, which escaped from the exit tube with the sulphurous acid. Immediately afterwards the remaining iodic acid suddenly became of a bright yellow colour, having formed, says the author, the compound $5\text{IO}_5 + \text{SO}_2$. This body must be quickly placed in a perfectly dry bottle, as it soon decomposes in the air.

If the action of the SO_2 be continued longer, rapid decomposition ensues, much iodine separates, and the mass collects together in small lumps, which adhere to the tube. From these free iodine and sulphuric acid may be washed out by alcohol and water, and then there will be left a new compound of iodine and oxygen I_2O_{13} , with a small percentage of SO_2 , which it is difficult to remove.

The *sulpho-iodic acid anhydride* $5\text{IO}_5 + \text{SO}_2$ has a beautiful bright yellow colour, a granular appearance, and the remarkable property of decomposing in contact with water, iodine being separated. In absolute alcohol and in ether it simply splits into IO_5 and SO_2 , forming in an alcoholic solution of potash iodate and sulphate of potash.

The new compound I_2O_{13} is a brown yellow powder, which does not change in the air, but always takes up some water. It dissolves in dilute nitric acid with the separation of iodine; fuming nitric acid oxidises it to IO_5 ; water at the ordinary temperature decomposes it slowly; boiling water splits it up immediately into I and

IO_5 . Heated alone to 100° it breaks up in the same way: $5\text{I}_2\text{O}_{13} = 13\text{IO}_5 + 2\text{I}$.

Aqueous solutions of the alkalies decompose the new compound, forming iodates of the alkalies and setting free iodine. Alcoholic solutions have no action on the compound, nor have absolute alcohol, ether, acetic ether, or sulphide of carbon, either at the ordinary temperature or at their boiling points.

New Oxychloride of Bismuth.—M. Lebaigue has obtained a compound to which he attributes the formula $(\text{Bi}_2\text{Cl}_3)_2 \cdot 2\text{Bi}_2\text{O}_3$ (*Journal de Pharm. et de Chim.*, Tome xxxix., p. 108). He treated oxide of bismuth with sulphuric acid, heated the mass until no more acid vapour was given off, mixed the yellowish powder obtained with twice its weight of fused chloride of sodium, and placed the mixture in a clay retort set in a reverberatory furnace. After keeping the retort red hot for some time, he was surprised to find that nothing distilled. He then allowed it to cool, broke the retort, and found a fused mass, which treated with water broke up into the soluble acid chloride and insoluble sub-chloride. Mixed with the latter were some brilliant white scales, which the author carefully separated and analysed. The results led the author to believe that the scales were formed of new oxychloride of bismuth, having the formula $(\text{Bi}_2\text{Cl}_3)_2 \cdot 2\text{Bi}_2\text{O}_3$.

II. ORGANIC CHEMISTRY.

New Acid from Toluol.—Fittig gives (*Annal. der Chem. und Pharm.*, Bd. cxvii. s. 191) a preliminary notice of a new acid he has discovered in the oxidation of toluol by nitric acid. It is colourless, perfectly insoluble in cold water, and only slightly soluble in hot water. At a higher temperature it fuses and sublimes undecomposed. It appears to have the same composition as salicylic acid without being identical, for it does not give the reaction of salicylic acid with chloride of iron. The baryta salt is easily soluble in water, and crystallises from the solution in warty tufts. The analysis of the crystals gave 32.7 per cent. Ba. The formula $\text{C}_{14}\text{H}_8\text{BaO}_6$ requires 33.3 per cent. Ba.

III. ANALYTICAL CHEMISTRY.

New Test for Sulphurous Acid.—A solution of nitroprusside of sodium, added to a solution of sulphite of soda, gives a pale rose colour, which is much more distinct when an excess of sulphate of zinc has previously been added to the solution of nitroprusside; but an intense purple red is produced when a small quantity of yellow prussiate of potash is added to the mixture. This reaction may be applied to the detection of sulphurous acid in the following way:—If the substance to be tested is soluble in water, but not neutral, it is first neutralised with acetic acid or bicarbonate of soda; an excess of the latter is not objectionable, but an excess of either caustic or carbonate of soda, or of carbonate of ammonia, prevents the reaction. If any bases are present which give insoluble salts with sulphurous acid, these must be removed. Then a proportionately large quantity of solution of sulphate of zinc is mixed with a very small quantity of nitroprusside of sodium, and added to the solution to be tested. When the amount of sulphurous acid present is not too small, a rose red or dark red colour is soon produced; but if it is not very distinct, the addition of some yellow prussiate will give an intense red. In this way a most minute quantity of sulphurous acid may be detected. Hyposulphurous acid, and hyposulphites of the alkalies, under the same circumstances, produce no reaction.—C. Boedeker. *Annal. der Chem. und Pharm.* Bd. cxvii. s. 193.

IV. TECHNICAL CHEMISTRY.

Adulteration of Wax with Paraffine.—Ludolt has met with a specimen of wax adulterated with paraffine. He detected the adulteration in the following way

Dingler's Polytech. Journal, Bd. clx. s. 224) :—He heated a piece of the wax with an excess of fuming sulphuric acid. When the wax melted a strong action took place, and when the evolution of vapour ceased the heat was continued a little longer, and then the mixture was allowed to cool. After cooling, the paraffine was found as a transparent layer upon the sulphuric acid. So much sulphuric acid must be used, that when the operation is ended the black residue may be fluid, otherwise it is difficult to separate the paraffine. Very small quantities of paraffine, the author says, may be discovered in this way. English sulphuric acid is not convenient for the purpose, as it decomposes the wax but slowly.

MISCELLANEOUS.

For Regularly Cracking Glass, a very expeditious plan is to use a pearl of glass melted with some soda on a platinum wire, heated before the blowpipe; the crack having been previously started with a sharp blade of first-rate cast-steel, made diamond-hard by quickly cooling.—*Dr. L. C. Levoir.*

Coal in California.—We learn from the *Californian Mining and Scientific Press* that a meeting of the members of the California Academy of Sciences, was held in San Francisco on May 15, at which Professor Blake gave an interesting description of the coal regions of Monte Diablo, accompanied with specimens of the coal. The veins are rather thin, but the coal is good bituminous. The fossils of the region belong to the tertiary formations. Professor Whitney is of opinion that the coal was formed from accumulations carried by eddies, and deposited in still water. A considerable quantity of this coal has been taken to San Francisco, and it has tended to reduce the price of wood and the foreign coal. A plentiful supply of coal in California would tend greatly to facilitate quartz-mining, by enabling the machinery to be operated by cheap steam power. Coal will also make California a great manufacturing State.

The Healthfulness of Fresh Paint in Apartments.—M. Leclerc, a well-known house-painter in Paris, has lately made some experiments to ascertain whether emanations from certain paints containing such substances as white lead, zinc white, linseed oil, essence of turpentine, coal oil, essence of lavender, &c., are injurious to health. He caused the insides of some boxes to be painted, and within them he placed wire cages containing rabbits which were not in contact with the paint but only subject to the influence of the emanations from it. The rabbits suffered while the paint was fresh, especially when it contained coal oil, but none of them died. It is thus proved that living in apartments recently painted, and which emit the odour of the oil of turpentine is not permanently injurious to health. M. Leclerc made, also, some other experiments for the purpose of obtaining deposits of these emanations from the fresh paintings of houses. Instead of rabbits he placed plates containing a small quantity of water in these chests. After the water had evaporated from the plates, he found some remarkable crystallisations like needles, which consisted of combinations in which the oils or essences employed formed the principal part. These crystalline combinations were obtained even when linseed oil was used. M. Deville had previously obtained similar crystallisations, but under entirely different circumstances. After these experiments and their results had been explained to the Academy of Sciences, M. Chevreul added that M. Leclerc had opened a new path of discovery for scientific men, which even Chemists might follow with profit.—*Mechanics' Magazine.*

British Association for the Advancement of Science.—The thirty-first meeting of the British Association for the Advancement of Science will commence in Manchester on Wednesday, September 4, 1881, under the presidency of William Fairbairn, Esq., LL.D., F.R.S. The General Committee will meet on Wednesday, September 4, at one a.m., for the election of sectional officers and the despatch of business usually brought before that body. On this occasion there will be presented the Report of the Council, embodying their proceedings during the past year. The general committee will meet afterwards by adjournment. The first general meeting will be held on Wednesday, September 4, at eight p.m., when the President will deliver an address; the concluding meeting on Wednesday, September 11, at three p.m., when the Association will be adjourned to its next place of meeting. At two evening meetings, which will take place at eight p.m., discourses on certain branches of science will be delivered. There will also be other evening meetings, at which opportunity will be afforded for general conversation among the members. The committees of Sections will meet daily from Thursday, September 5, to Tuesday, September 10, inclusive, at ten a.m., precisely. The following are the titles of the sections to which communications may be presented :—Section A. Mathematics and Physics; Section B. Chemistry and Mineralogy, including their Applications to Agriculture and the Arts; Section C. Geology; Section D. Zoology and Botany, including Physiology, sub-section D.; Section E. Geography and Ethnology; Section F. Economic Science and Statistics; Section G. Mechanical Science. Notices or communications intended to be read to the Association, accompanied by a statement whether the author will be present or not at the meeting, may be addressed to Professor Phillips, M.A., LL.D., F.R.S., Assistant-General Secretary, University Museum, Oxford; or to R. D. Darbishire, Esq., B.A., F.G.S., Alfred Neild, Esq., Arthur Ransome, Esq., M.A., and Professor Roscoe, B.A., Local Secretaries, Manchester. Gentlemen desirous of attending the meeting will find in the reception room (the Portico, in Mosley Street) blank forms of proposal, and may make their choice of being proposed as life members, paying ten pounds as a composition, or annual subscribers, paying one pound annually and an admission fee of one pound (making together two pounds on admission); or associates for the meeting, paying one pound. Ladies may obtain tickets, through the application of a member, in the reception-room, price one pound each ticket. These tickets are transferable to other ladies only.

ANSWERS TO CORRESPONDENTS.

* All Editorial Communications are to be addressed to the Editor; and Advertisements and Business Communications to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

N. T.—Mr. Perkin's address is Greenford Green, Sudbury, N.W.

H. C. S. E.—We will propose you, if you wish to become a Member.

Titanic Acid.—*F. S.*—To detect this acid in the presence of silicic acid, treat the silicic acid with hydrofluoric acid. Fuse the residue with metallic zinc and microcosmic salt; after the combustion of the zinc and clearing of the lead, on cooling, a very distinct colouration is always produced, whereas the button of microcosmic salt and titanitic acid gives no reaction in the ordinary way. By this test (which is recommended by Riley) titanitic acid may be found in pig iron.

Specific Gravity writes as follows :—"Would it be possible in the quantitative analysis of waters, having obtained the specific gravity, to calculate thence the total amount of solid matter contained in any volume? Hence, suppose the s. g. of a sample of water to be 1.004, the weight of an Imperial gallon of such water would be $1.004 \times 70,000 = 70,280$ grs. Now, as an Imperial gallon of distilled water is 70,000 grs., is not the amount of solid matter contained in one gallon of the water under examination the difference in weight = 280 grs.? If this is the case, it saves the long and tedious operation of evaporating down to obtain the same result. And *vice versa*, knowing the weight of the total solid contents, cannot you calculate the s. g."

THE CHEMICAL NEWS.

VOL. IV. No. 89.—August 17, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Gerhardt's Equivalents and Notation.

(Continued from vol. iii. p. 373.)

FURTHER examples, illustrating the relations between the atomic weights of compounds and their specific gravities, are given in the following table:—

Name of Substance.	Formula.	Atomic weight.	Specific gravity of vapour.	Atomic volume.
Carbonic oxide . . .	CO	28	13.950	2
Carbonic anhydride . .	CO ₂	44	21.973	2
Phosgene gas . . .	COCl ₂	99	49.931	2
Bisulphide of carbon .	CS ₂	76	38.121	2
Sulphurous anhydride .	SO ₂	64	32.031	2
Sulphuric anhydride .	SO ₃	80	43.243	2
Phosphoretted hydrgn.	H ₂ P	34	16.792	2
Trichloride of arsenic	AsCl ₃	181.5	90.819	2
Ethyl-alcohol . . .	C ₂ H ₅ O	46	23.254	2
Ethyl-ether . . .	C ₂ H ₅ O	74	36.973	2
Methyl-ethyl-ether . .	C ₃ H ₇ O	60	31.005	2
Acetic acid . . .	C ₂ H ₃ O ₂	60	30.558	2
Acetic anhydride . .	C ₂ H ₃ O ₂	102	50.018	2
Acetate of ethyl . .	C ₂ H ₃ O ₂	88	44.209	2
Aldehyde . . .	C ₂ H ₃ O	44	22.082	2
Ethylene . . .	C ₂ H ₄	28	14.131	2
Marsh gas . . .	CH ₄	16	8.038	2
Chloroform . . .	CHCl ₃	119.5	60.539	2
Tetrachloride of carbon	CCl ₄	154	76.828	2
Camphor . . .	C ₁₀ H ₁₆ O	152	76.604	2
Ethylamine . . .	C ₂ H ₇ N	45	22.630	2

The relation between atomic weight and specific gravity in the state of vapour (vapour density) which we have pointed out, has been so uniformly observed, that chemists are inclined to doubt the exactitude of the formulæ of all compounds the atomic weight of which does not yield two volumes of vapour, and the determination of the vapour density of a compound has thus become one of the most important means of controlling chemical formulæ.

The discrepancy in the vapour volumes furnished by the elementary and compound atoms, the former corresponding, as has been pointed out, to one volume, while the latter represent two volumes, has led Gerhardt to the revival of a hypothesis which had been previously countenanced by several inquirers. This hypothesis assumes that the elementary atoms are incapable of existing in the free state, and that whenever elementary atoms are liberated they combine, so to say, with themselves, and separate in pairs, which pairs of atoms may be called the molecules of the elements. We thus arrive at a rigorous distinction between the terms representing the smallest quantities in which the elements enter into combination; the latter the smallest quantity of an element capable of existing in a free state.

The molecules of the elements possessing, as they do, the double weight of the atoms, correspond, with the exception of phosphorus and arsenic (see p. 702), to 2 volumes of gas or vapour.

Name of Element.	Specific gravity.	Atomic weight.	Atomic volume.	Molecular weight.	Molecular volume.
Hydrogen .	1	H 1	1	HH 2	2
Oxygen .	16	O 16	1	OO 32	2
Sulphur .	32	S 32	1	SS 64	2
Chlorine .	35.5	Cl 35.5	1	ClCl 71	2
Bromine .	80	Br 80	1	BrBr 160	2
Iodine .	127	I 127	1	II 254	2
Nitrogen .	14	N 14	1	NN 28	2
Phosphorus	62	P 31	1	PP 62	1
Arsenic .	150	As 75	1	AsAs 150	1
Mercury .	100	Hg 100	1	HgHg 200	2

It is thus seen that the molecules of the elements, like the atoms of compounds, correspond to 2 volumes of vapour, and that if we adopt for the elements the double symbols as designating the molecules, all chemical formulæ, with the exception of those of phosphorus and arsenic, represent weights of matter simple or compound, which in the state of gas or vapour occupy the same volume.

The assumption that the elementary molecules consist of two atoms, is not capable of being proved directly, but it is supported by many considerations and analogies which are chiefly taken from organic chemistry. The limits of this outline will only admit the chief argument to be mentioned here.

The researches of the last ten or twelve years have led to the preparation of a series of hydrocarbons, which are considered as the radicals of the alcohols. The radicals methyl, ethyl, propyl, butyl, amyl, and caproyl have been successively examined. The composition of the alcohols to which these radicals belong, being—

Methylic alcohol (C₁H₃) HO,
 Ethylic alcohol (C₂H₅) HO,
 Propylic alcohol (C₃H₇) HO,
 Butylic alcohol (C₄H₉) HO,
 Amylic alcohol (C₅H₁₁) HO,
 Caproylic alcohol (C₆H₁₃) HO,

nothing appeared more justifiable than the formulæ,—

C₁H₃ for methyl, C₄H₉ for butyl,
 C₂H₅ for ethyl, C₅H₁₁ for amyl,
 C₃H₇ for propyl, C₆H₁₃ for caproyl,

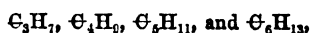
which were originally proposed for these hydrocarbons. A more minute examination of these compounds, however, very soon led chemists to double these formulæ. It is only thus that they represent quantities yielding 2 volumes of vapour, like all other compounds, and that the differences of boiling-point exhibited by these substances are made to accord with the experience on boiling-points derived from numerous other observations. In the following Table the formulæ, atomic weights,

vapour-densities, atomic volumes, and boiling-points of the six substances in question are united:—

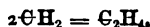
Name.	Formula.	Atomic weight.	Sp. gr. of vapour.	Atomic volume.	Boiling point.
Methyl	$\text{C}_2\text{H}_6 = (\text{CH}_3)(\text{CH}_3)$	30.15	2	Unknown.	
Ethyl	$\text{C}_4\text{H}_{10} = (\text{C}_2\text{H}_5)(\text{C}_2\text{H}_5)$	58.29	2	Unknown.	
Propyl	$\text{C}_6\text{H}_{14} = (\text{C}_3\text{H}_7)(\text{C}_3\text{H}_7)$	86.43	2	$154^\circ.4$, 68°C .	
Butyl	$\text{C}_8\text{H}_{18} = (\text{C}_4\text{H}_9)(\text{C}_4\text{H}_9)$	114.57	2	$226^\circ.4$, 108°C .	
Amyl	$\text{C}_{10}\text{H}_{22} = (\text{C}_5\text{H}_{11})(\text{C}_5\text{H}_{11})$	142.71	2	$311^\circ.0$, 155°C .	
Caproyl	$\text{C}_{12}\text{H}_{26} = (\text{C}_6\text{H}_{13})(\text{C}_6\text{H}_{13})$	170.85	2	$395^\circ.6$, 202°C .	

The difference between the boiling-points of propyl and butyl is $226^\circ.4 - 154^\circ.4 = 72^\circ.0$ ($108^\circ - 68^\circ = 40^\circ \text{C}$); that between the boiling-points of butyl and amyl, $311^\circ - 226^\circ.4 = 84^\circ.6$ ($155^\circ - 108^\circ = 47^\circ \text{C}$); and lastly, the difference between the boiling-points of amyl and caproyl, $395^\circ.6 - 311^\circ = 84^\circ.6$ ($202^\circ - 155^\circ = 47^\circ \text{C}$).

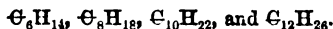
According to Kopp's observations, an increment of $\text{C}_2\text{H}_2 = \text{CH}_2$ in composition raises the boiling-point $36^\circ.3 = 19^\circ.9 \text{C}$. (see p. 499.) The boiling-points of propyl, butyl, amyl, and caproyl unmistakeably show that these bodies differ in composition not by CH_2 as implied in the formulæ



but by

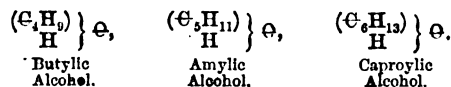
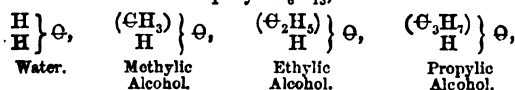


as implied by the formulæ,—

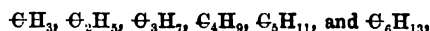


If methylic, ethylic, propylic, butylic, amylic, and caproylic alcohols be viewed as water in which one atom of hydrogen is replaced respectively by the compound atoms

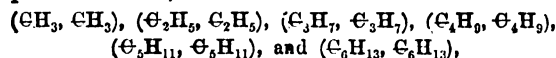
methyl C_2H_5 , ethyl C_3H_7 , butyl C_4H_9 , amyl C_5H_{11} , and caproyl C_6H_{13} ,



it becomes probable, from the boiling-point relations alluded to, that the compound atoms



in the moment of their liberation, combine with themselves, giving rise to what we may now call the compound molecules,



The methyl-, ethyl-, propyl-, &c., atoms, such as exist in the alcohols, correspond to the atom of hydrogen (to $\text{H} = 1$) such as we assume it to exist in combination. The methyl-, ethyl-, propyl-, &c., molecules which exist in the free state, consisting, as they do, of two atoms of these radicals, correspond to the molecule of hydrogen, which consists itself of two atoms of hydrogen.

(To be continued.)

*On Liquid Diffusion Applied to Analysis,*¹
by T. GRAHAM, Esq., F.R.S., Master of the Mint.

THE unequal diffusibility of different substances in water appears to present means of separation not unlike those long derived from unequal volatility. For, as regards diffusion, there exists a "volatile," and also a "fixed" class of substances; and these distinctions appear to correspond with differences in molecular constitution of a fundamental nature. Much value is attached to diffusion, as affording the means of bringing out clearly, and subjecting to numerical expression, the distinctive properties of what appear to be two great divisions of chemical substances.

The first, or *diffusive* class of substances, are marked by their tendency to crystallise, either alone or in combination with water. When in a state of solution they are held by the solvent with a certain force, so as to effect the volatility of water by their presence. The solution is generally free from viscosity, and is always sapid. Their reactions are energetic and quickly effected. This is the class of *crystalloids*.

The other class, of low diffusibility, may be named *colloids*, as they appear to be typified by animal gelatine. They have little if any tendency to crystallise, and they affect a vitreous structure. The planes of the crystal with its hardness and brittleness are replaced in the colloid by rounded outlines with more or less softness and toughness of texture. Water of crystallisation is represented by water of gelatination. Colloids are held in solution by a feeble power, and have little effect on the volatility of the solvent. They are also precipitated from their solution by the addition of crystalloids. The solution of colloids has always a certain degree of viscosity or gumminess when concentrated. They appear to be insipid, or wholly tasteless, unless when they undergo decomposition upon the palate and give rise to sapid crystalloids. Their solid hydrates are gelatinous bodies. They are united to water with a force of low intensity; and such is the character of the combinations in general between a colloid and a crystalloid, even although the latter may be a powerful re-agent in its own class, such as an acid or an alkali. In their chemical reactions, the crystalloidal appears the energetic form, and the colloidal the inert form of matter. The combining equivalent of the colloid appears always to be high, and it has a heavy molecule. Among the colloids rank hydrated silicic acid, and a number of soluble hydrated metallic peroxides, of which little has hitherto been known; also starch, the vegetable gums and dextrin, caramel, tannin, albumen, and vegetable and animal extractive matters. The peculiar structure and chemical indifference of colloids appear to adapt them for the animal organisation, of which they become the plastic elements.

Although the two classes are widely separated in their properties, a complete parallelism appears to hold between them. Their existence in Nature appears to call for a corresponding division of chemistry into a crystalloid and a colloid department.

Although chemically inert in the ordinary sense, colloids possess a comparative activity of their own, arising out of their physical properties. While the rigidity of the crystalline structure shuts out external impressions, the softness of the gelatinous colloid partakes of fluidity, and enables the colloid to become a medium for liquid diffusion, like water itself. The same

¹ Read before the Royal Society, June 6th, 1861.

penetrability appears to take the form of a capacity for cementation in such colloids as can exist at a high temperature. Hence a wide sensibility on the part of colloids to external agents. Another eminently characteristic quality of colloids is their mutability. Their existence is a continued metastasis. A colloid may be compared in this respect to water while existing liquid at a temperature below its usual freezing point, or to a supersaturated saline solution. The solution of hydrated silicic acid, for instance, is easily obtained in a state of purity, but cannot be preserved. It may remain fluid for days or weeks in a sealed tube, but is sure to gelatinise at last. Nor does the change of this colloid appear to stop at that point; for the mineral forms of silicic acid deposited from water, such as flint, are found to have passed, during the geological ages of their existence, from the vitreous or colloidal into the crystalline condition (H. Rose). The colloidal is, in fact, a dynamical state of matter; the crystalloidal being the statical condition. The colloid possesses *energia*. It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality. To the gradual manner also in which colloidal changes take place—for they always demand time as an element—may the chronic nature and periodicity of vital phenomena be ultimately referred.

For the separation of unequally diffusive crystalloids from each other, jar-diffusion was had recourse to. The mixed solution was conveyed by means of a pipette to the bottom of a column of water contained in a cylindrical glass jar. A kind of cohabitation takes place, a portion of the most diffusive substance rising and separating from the less diffusive substances, more and more completely as it ascends.

The separation of a crystalloid from a colloid is more properly effected by a combination of diffusion with the action of a septum composed of an insoluble colloidal material. Animal membrane will serve for the latter purpose, or a film of gelatinous starch, hydrated gelatin itself, albumen, or animal mucus. But by much the most effective septum used was paper, as it is metamorphosed by sulphuric acid (Gaine). It is now supplied by Messrs. De La Rue, and has become familiar under the name of "vegetable parchment," or "parchment paper." From sheet gutta percha a flat hoop is formed, eight or ten inches in diameter by three inches in depth, and one side is covered by a disc of parchment paper, so as to form a vessel like a sieve. A mixed solution, which may be supposed to contain sugar and gum, is placed upon the septum to a depth of half-an-inch, and the instrument then floated upon a considerable volume of water contained in a basin. Three-fourths of the sugar diffuses out in twenty-four hours, and so free from gum as to be scarcely affected by subacetate of lead, and to crystallise on the evaporation of the external water by the heat of a water-bath.

The unequal action of the septum, which causes the separation described, appears to depend upon this:—The crystalloid sugar is capable of taking water from the hydrated colloidal septum, and thus obtains a medium for diffusion; but the colloid gum has little or no power to separate the combined water of the same septum, and does not, therefore, open the door for its escape by diffusion, as the sugar does. This separating action of the colloidal septum is spoken of as *dialysis*.

Dialysis was applied to the preparation of various colloids. The mixed solution obtained by pouring silicate of soda into water acidulated with hydrochloric acid, was placed upon a parchment paper dialyser and allowed

to diffuse into water, the latter being occasionally changed. After the lapse of five days, seven-eighths of the original silicic acid was found to remain liquid upon the septum, and to be so free from hydrochloric acid and chloride of sodium as not to give a precipitate with acid nitrate of silver. The true hydrated alumina, and also Mr. Crum's metalumina, were obtained soluble by dialysing solutions of these oxides in the chloride and acetate of the same metal. So also the hydrated peroxide of iron, in addition to the hydrated metaperoxide of iron of M. Péan de Saint-Gilles, and the soluble hydrated chromic oxide. The varieties of Prussian blue are obtained soluble by dialysing their solution in oxalate of ammonia, the latter salt diffusing away. Stannic and titanous acids appear as insoluble gelatinous hydrates.

A solution of gum arabic (gummate of lime), dialysed after an addition of hydrochloric acid, gave at once the pure gummic acid of Frémy. Soluble albumen is obtained in a state of purity by dialysing that substance with an addition of acetic acid.

Caramel of sugar, purified by repeated precipitation by alcohol and afterwards by dialysis, contains more carbon than any of the caramellic bodies of Gélis; it forms a tremulous jelly when concentrated, and appears decidedly colloidal. Caramel, like all other colloids, has a soluble and an insoluble modification. The latter has its solubility restored by the action of alkali followed by that of acetic acid and subsequent dialysis.

Dialysis proves highly useful in separating arsenious acid and metallic poisons from organic fluids. Defibrinated blood, milk, and other organic fluids charged with a few milligrammes of arsenious acid, and placed upon the dialyser were found to impart the greater proportion of the arsenious acid to the external water in the course of twenty-four hours. The diffusate was so free from organic matter, that the metal could be readily precipitated by sulphuretted hydrogen, and the quantity weighed.

Ice, at or near its melting point, appears to be a colloidal substance, and exhibits a resemblance to a firm jelly in elasticity, the tendency to rend, and to redintegrate on contact.

The consideration of the properties of gelatinous colloids appears to show that osmose is principally an affair of the dehydration of the gelatinous septum under influences having a catalytic character, and that the phenomenon is independent of diffusion. The colloidal septum is capable of hydrating itself to a higher degree in contact with pure water than in contact with a saline solution. Colloidal septa, swollen in consequence of contact with dilute acid or alkali, appear to acquire increased sensibility to osmose, in consequence of their unusually high degree of hydration.

On Didymium, Lanthanum, Cerite, and Lanthano Cerite, by R. HEMMANN.

(Continued from page 73.)

Sulphide of Didymium is formed by heating the oxide in the vapour of bisulphide of carbon. It is a light, greenish-brown powder, evolving sulphuretted hydrogen with dilute acids, and, when heated, smouldering to a mixture of oxide and basic sulphate.

Oxy-sulphide of Didymium is obtained by fusing oxide of didymium and sulphide of sodium. When the mass is treated with water, Di_2OS remains as a gray residue, which dissolves in hydrochloric acid with evolution of HS .

Hydrated Oxide of Didymium.—Solutions of didymium salts give, with hydrate of potassium or sodium, a slimy, transparent, bluish rose coloured precipitate, which dries up to flesh-red pieces of hydrated oxide of didymium, which, however, cannot be obtained by this process free from carbonic acid. Ammonia throws down from solutions of didymium basic salts, which also appear as bulky, transparent precipitates, of a bluish-rose colour.

Chloride of Didymium.—If a solution of the oxide in hydrochloric acid be mixed with a sufficient quantity of sal ammoniac, a double salt is produced, which may be evaporated to dryness without decomposition. If this double salt be heated in a covered platinum crucible to redness, the sal ammoniac passes off, and chloride of didymium remains behind. It forms a rose-red mass, of radiated crystalline structure, and consists of DiCl :—

Di	57.205	1.	593.50	57.24
Cl	42.795	1.	443.28	42.76
	100.000		1036.78	100.00

When chloride of didymium, thus prepared, is dissolved in water, some oxychloride of didymium usually remains behind, in the form of rosy-red, pearly scales. Chloride of didymium deliquesces in moist air, and dissolves easily in alcohol.

Hydrated Chloride of Didymium.—Oxide and peroxide of didymium dissolve easily in hydrochloric acid, forming a rose-coloured fluid, which, when evaporated to a syrupy consistence, solidifies to a granular mass of crystals. According to Marignac, these crystals are monoklinœdric, and consist of the combination $\infty \text{P.OP}$ ($\text{P} \infty$). $\infty \text{P } 78^\circ$; $\text{OP} : \infty \text{P } 92^\circ$; ($\text{P} \infty$) : ($\text{P} \infty$) on $\text{OP } 67^\circ$. The composition is $\text{DiCl} + 4\text{HO}$.

Oxychloride of Didymium.—When hydrated chloride of didymium is heated, there passes off, besides water, much hydrochloric acid. There is thus formed the oxychloride as a white powder, insoluble in water and slowly and with difficulty soluble in hydrochloric acid.

Double Chloride of Didymium and Mercury. obtained by evaporating the chlorides of didymium and mercury, forms rose-red cubes. It is easily soluble in water, but not deliquescent. Its composition is $\text{DiCl} + 3\text{HgCl} + 8\text{HO}$ (Marignac).

Bromate of Didymium forms light, rose-red, permanent hexagonal prisms. Combination : $\infty \text{P.P.P}$; $\text{P } 147^\circ 58'$ (terminal edges), 67° (lateral edges). Composition, $\text{DiO.BrO}_3 + 6\text{HO}$.

Fluoride of Didymium.—Fluoride of sodium produces a precipitate in solutions of didymium salts, only slightly soluble in hydrochloric acid.

Carbonate of Didymium, obtained by precipitating didymium salts with bicarbonate of ammonia forms a light rose-red, which, when dried *in vacuo*, consists of $\text{DiOCO}_2 + 2\text{HO}$. Dried at 100° , it has the composition $2\text{DiOCO}_2 + \text{HO}$ (Marignac).

Nitrate of Didymium.—Oxide and peroxide of didymium dissolve easily in nitric acid, forming a rose-red liquid. By evaporation, the syrupy solution acquires an amethystine colour, and solidifies, after standing some time over sulphuric acid, to a crystalline mass, which deliquesces in moist air and dissolves easily in alcohol. When heated in the air it is converted into peroxide of didymium.

Arsenate of Didymium.—Arsenic acid gives with didymium salts a pulverulent precipitate only when

boiled. Arseniate of potassium, on the contrary, produces, even in the cold, a slimy precipitate. Both precipitates, when dried *in vacuo*, consist of 3DiO , 2AsO_5 , $+ 2\text{HO}$ (Marignac).

Phosphate of Didymium, $3\text{DiO}_2\text{PO}_5 + 5\text{HO}$. If a cold solution of sulphate of didymium be mixed with its equivalent of phosphoric acid,—viz., one atom phosphoric acid to three atoms oxide of didymium, two-thirds of the didymium are thrown down as phosphate of didymium, while one-third remains dissolved in the free acid. The phosphate of didymium so prepared forms a flocculent precipitate, which, dried at the temperature of the air, possesses the above composition. We obtain in fact,—

DiO . . .	53.88	3 =	208.5	53.82
PO ₅ . . .	46.12	2 =	178.6	46.18
	100.00		386.1	100.00
3DiO, 2PO ₅	87.19	1 =	386.1	87.30
HO . . .	12.81	5 =	562.5	12.70
	100.00		4427.6	100.00

The phosphate of didymium which remains dissolved in the free acid is thrown down by ammonia as a transparent, pasty precipitate, which dries up at the temperature of the room to transparent pieces of a beautiful rose-red colour, and with conchoidal fracture. By precipitating a hot solution of sulphate of didymium with phosphoric acid, Marignac obtained a pulverulent, white phosphate of didymium, of the composition 3DiO , PO_5 , $+ 2\text{HO}$.

Sulphite of Didymium, $\text{DiOSO}_3 + 2\text{HO}$ (dry) (Marignac). Oxide of didymium dissolves in aqueous sulphurous acid, forming a rose-red liquid, from which, on the application of heat, a voluminous precipitate separates, but which dissolves again on cooling, when the fluid still contains an excess of sulphurous acid.

Basic Sulphate of Didymium, obtained by precipitating sulphate of didymium with excess of ammonia, forms a light-bluish, rose-red, transparent precipitate, which dries up at the temperature of the air to pieces, with an earthy fracture and a rose-red colour, $3\text{DiO} + \text{SO}_3 + 8\text{HO}$.

Sulphate of Didymium.—Oxide and peroxide of didymium dissolve easily in sulphuric acid, forming a rose-red liquid. By evaporation, whether spontaneously or by the aid of heat, it forms dark, rose-red, granular crystals of hydrated sulphate of didymium. According to Marignac, the form of these crystals is monoklinœdric. The composition of this salt was found to be the same by the author as that determined by Marignac. It expresses, in fact, the unusual formula $3(\text{DiOSO}_3) + 8\text{HO}$.

Oxalate of Didymium.—Oxalic acid and oxalate of ammonia produce in solutions of didymium salts at first a caseous precipitate, which, however, soon sinks together to a crystalline, light, rose-red powder. Dried at 15° , the compound consists of $\text{DiOC}_2\text{O}_3 + 4\text{HO}$. Dried at 100° , it consists of $\text{DiOC}_2\text{O}_3 + \text{HO}$.

(To be continued.)

On the Production of the Ethyl Bases,¹ by M. CAREY LEA, Philadelphia.

I MENTIONED in a former number of this Journal that while engaged in the examination of the action of am-

¹ American Journal of Science and Arts.

ammonia on certain oxy-ethers, I had met with the experiments of Juncadella and De Clermont, and finding that the production of ethylamine by these reactions had been already indicated, I discontinued my investigations. Subsequently, however, having occasion to prepare a considerable quantity of ethylamine for other examinations, I determined to ascertain whether the action of nitrate of ethyl on ammonia could not be made use of as a convenient process. Juncadella had already proposed to mix nitrate of ethyl with three or four parts of alcohol, saturate the mixture with dry ammoniacal gas, and heat for two days to 212° in the water bath. This was a rather troublesome process, and a few experiments led me to the following very simple method, which I publish as having given me satisfactory results.

Nitrate of ethyl is mixed with an equal volume of strong alcohol. To this mixture is added a quantity of ordinary strong liquid ammonia, equal in bulk to that of the mixture. The nitrate of ethyl is thereby precipitated, but is nevertheless easily acted upon by the ammoniacal liquid. Three hours in the water-bath at 212° is generally sufficient to complete the reaction. As the process goes on, the nitrate of ethyl becomes brownish, gradually diminishes, and finally disappears. The resulting solution does not contain salts of ammonia and ethylamine only, as has been stated, but a large quantity of diethylamine and triethylamine.

As the nitrate of ethyl can be prepared in large quantity with great facility by a modification of M. Millon's process, I am inclined to think that this process will be found valuable for the preparation of the ethyl bases. It is necessary that the tubes should be strong, and not more than two-thirds full. Bottles hermetically sealed are not to be recommended unless extremely strong. In one experiment I operated with four eight-ounce bottles, all of which stood the test, but on repeating the process three of the four exploded at once, and the fourth subsequently.

The presence of alcohol is far from being essential. Nitrate of ethyl is readily decomposed at 212° by aqueous ammonia, and if the ammonia is in considerable excess, the decomposition is complete in three hours. If the excess is only slight, the time required for decomposition is greatly increased. In one experiment where 29 parts of nitrate of ethyl were heated with 57 of liquid ammonia, the decomposition was not complete till at the end of 15 hours. The use of alcohol, and the proportions above given, are what was found to give the most satisfactory result.

TECHNICAL CHEMISTRY.

On the Origin and Progress of the Phosphorus and Match Manufactures, by G. GORE.

(Continued from page 79.)

THE latest improvement of note in the manufacture of matches is that of M. Lundstrom, of Jonkoping, in Sweden. It consists in dividing the ingredients of the match mixture into two separate compositions, one being placed upon the ends of the splints as usual, and the other, which contains the phosphorus, being spread in a thin layer upon the end or lid of the box. The patentee uses the following mixture for the splints:—

Chlorate of potash	6 parts.
Sulphide of antimony	2 to 3 "
Glue	1 "

and the following for the friction surface, which has been previously made rough by a coating of glue and sand:—

Amorphous phosphorus	10 parts.
Sulphide of antimony, or peroxide of manganese	8 "
Glue	3 to 6 "

By this method the danger of fires arising from ignition of the matches by friction is avoided (because the matches will ignite by rubbing upon no other surface besides the prepared one); and that of poisoning is totally prevented by using the red or innocuous variety of phosphorus.

There is scarcely any manufacture of which the statistics appear more fabulous than that of match-making. To begin with England, in which the manufacture is comparatively small. The firm of Messrs. Dixon, Newton Heath, employ about 300 workpeople on the premises, and upwards of 100 at their own homes. Their stock of timber is generally of the value of about 8000*l.* or 10,000*l.* They consume 1 ton of sulphur per week, in the sulphuring of splints; and in twelve months they use between 4 and 5 tons of chlorate of potash, and 12 tons of glue. They produce about 43,000,000 of matches every week, or 2,160,000,000 matches yearly; reckoning the length of a match at $2\frac{1}{2}$ inches, the total length of these would far exceed the circumference of the earth. In London a single saw-mill cuts up 400 large timbers annually to make splints for matches. It has been estimated that from 12,000 to 15,000 gross of boxes of matches are produced weekly in London, indicating a production of 5,000,000,000 of matches yearly in that city. And another calculation has been made that the whole length of waxed cotton wicks consumed by one London manufacturer in the production of "Vestas" would be sufficient to reach from England to America and back again. One pound of phosphorus is sufficient for tipping 1,000,000 of German matches, or for 600,000 English ones.

The yearly consumption of phosphorus in the manufacture of matches in this country has been estimated at 6 tons, which, at a price of 2*s.* 6*d.* a pound, amounts to a value of 1680*l.*; and of chlorate of potash, 26 tons. A single merchant of London imports 8000 cwt. of cases of matches yearly. In 1854, 20,000*l.* worth of matches came into this country from the city of Hamburgh alone. We import at the present time at least 1500 tons of matches a-year from Sweden only. It has been estimated that 60,000*l.* worth of matches are imported yearly into Great Britain,—equal to 200,000,000 of matches daily; and that our daily consumption approaches the enormous number of 250,000,000, or more than 8 matches each day for every individual in the kingdom. In Belgium, the consumption of matches has been calculated to be 9 matches per day for every living person.

In France, the manufacture of matches is very extensive. Paris alone employs 1000 operatives, and produces 990,000,000 of boxes, worth in total 7200*l.* yearly. In Sweden there are 6 or more manufactories of matches; in one of them, in 1848, there was employed about 400 workpeople, and produced between 8,000,000 and 10,000,000 of matches daily.

But the greatest seat of match-making is located in Austria, where there are numerous manufactories which produce many millions per day of phosphorus matches: for instance, those of M. Pollak, at Vienna, and M. Fürth, in Bohemia, consume together about 20 tons of phosphorus annually, and give employment to about

6000 persons; and as one pound of phosphorus suffices for upwards of 1,000,000 of their matches, those two makers alone produce the amazing number of 44,800,000,000 of matches yearly. In the year 1849, there was exported from the port of Trieste alone nearly 200 tons of lucifer matches, and the whole produce of matches in Austria in that year was estimated at 2500 tons.

The cost at which both boxes and matches are produced is equally startling. M. Fürth "sells his cheapest boxes at one penny per dozen, each box containing 80 matches." Peter Harrass, of Suhl, in Prussia, sells his plain boxes at twopence per 100, and 1400 splints for one farthing. And De Majo, of Moravia, "sells a case of 50 boxes, each containing 100 lucifers, for fourpence."

Notwithstanding the great and universal advantages conferred upon mankind by the introduction of phosphorus into the manufacture of matches, it is not without some serious drawbacks. It produces a most painful disease, which attacks the jaw-bones of the workpeople, and causes them to decay, until, in some cases, they are entirely destroyed. This disease only attacks the operatives who are exposed to vapours arising from the match composition,—chiefly those who dip the splints, also those who are occupied in drying, cutting, and boxing the dipped matches. The workmen employed in other parts of the manufacture are wholly unaffected. The disease has been fearful in Germany and France, where so much phosphorus is used in the match composition, so much so as to excite the attention of the Governments of those countries; but it has not appeared to so great an extent here. It may be considerably, if not entirely prevented, by particular attention to cleanliness and ventilation, which has been already proved in the establishment of Mr. Hynam, of London; the most effectual remedy, however, would consist in the use of amorphous phosphorus instead of the ordinary variety. It is to be remarked, that in the manufacture of the phosphorus itself, the experience of many years has shown an almost complete immunity from any symptom of the disease; this may be partly attributed to the fact that the substance is kept covered with water from its first production to the vessels in which it is finally packed for sale.

Another great disadvantage is, that ordinary phosphorus is highly poisonous, and several instances are upon record where children have been killed through sucking the composition from the ends of matches. It might be also used for intentional poisoning, especially as phosphorus exists naturally to a large amount in the tissues of the human body; and phosphorus introduced into the stomach becomes gradually converted into phosphoric acid, which cannot readily be distinguished from that naturally present. The amorphous variety could not produce poisoning either by accident or intention, for Dr. De Vry, of Rotterdam, has administered it in various doses to animals without producing any ill effects.

Conflagrations have also frequently resulted from ordinary phosphorus matches, which could not possibly have occurred with the matches made with amorphous phosphorus under the patent of M. Lundstrom, already mentioned. In the records of fire insurance, we find that calamitous fires have been produced by cats knocking down boxes of matches, and by rats gnawing the ends of matches.

Let us endeavour to draw a useful lesson from the facts we have recorded,—not to despise or think lightly of the abstract facts or "small beginnings" of philoso-

phers, but rather to foster them, with a hope that, sooner or later, they may become of value to ourselves or our successors. Who would have thought that the curious, stinking, unctuous, luminous wax of Master Brandt would ever have become so important and universal an agent of civilization? That great modern innovator, the scientific faculty of man, has, by the certain process of experiment and induction, so immensely improved the means of producing phosphorus, and so marvellously adapted its properties to the uses and requirements of domestic and general life, that it has extended the civilising and ameliorating influence of the lucifer match to the uttermost ends of the earth. The matches of London, Manchester, and Vienna, are to be seen in the wigwam of the far-west Indian, the bush dwelling of the Australian, the habitation of the Mussulman, the frozen hut of the Greenlander, the hovel of the peasant, and the palace of the Emperor. Everywhere, as commerce extends its boundaries into newly-discovered regions, it is one of the first articles of civilisation which is introduced.

On the Preparation of Rosolic Acid and of Rosolates,
by M. JOUÏN.

M. RUNGE, in 1834, having remarked that in certain portions of a gas-making apparatus, the lime found therein became red, endeavoured to separate this coloured matter, and gave to it the name of rosolic acid, but without having well defined it.

In 1854, Mr. McDougall, a Manchester manufacturer, having observed that the lime used in the preparation of disinfecting coal-tar became, after a certain time, more or less red, took some of this substance to Dr. Smith, a chemist in the same town. After more attentive study than M. Runge had bestowed upon it, Dr. Smith succeeded in separating from it a solid brown substance, becoming yellow when dissolved in alcohol, and red on the addition to it of an alkali or of lime. He retained the name of rosolic acid given by Runge, for the first of these bodies, the red colouring matter being then considered as a rosolate.

Wishing to obtain a certain quantity of this acid for analyses, Dr. Smith sought for the most expeditious method,—the process for separating rosolate of lime being not only tedious but yielding small results; for the red lime mentioned contains only a few grammes of rosolate to several kilogrammes of the staple matter.

Supposing that rosolic acid was a modification of phenic acid, existing in the products used for the preparation of coal-tar, Dr. Smith passed phenic acid in vapour over lime in a red-hot tube, and found that the lime became red-hot only after a certain time, and he obtained only a small quantity of rosolate of lime. Then he tried the following process, which he ultimately preferred, as being the most simple. He heated a mixture of phenic acid, soda, and peroxide of manganese, and again obtained, and in larger quantities, the red matter, rosolate of soda.

Thus the question rested, when, having occasion to study phenic acid, I indirectly found Dr. Smith's rosolic acid, without then being aware of his researches; and I found that rosolic acid and rosolates could be much more easily obtained if, in the process given by the Manchester chemist, peroxide of mercury were substituted for peroxide of manganese.

In short, with manganese are obtained, besides rosolate of soda, manganates of soda, which decompose when

it is attempted to separate them for the purpose of purifying the colouring matter, forming oxides of manganese which must be separated, and thus necessitating a lengthened purification, while if oxide of mercury is substituted for the oxide of manganese in the mixture of carbolic acid and soda, we very rapidly obtain (ten minutes suffices), and at a temperature below 150° , the transformation of carbolic into rosolic acid, and the formation of a rosolate of soda of a beautiful red. This salt then appears as an excessively viscous liquid, solidifying almost entirely when cooled, and simply needing decanting to purify it, which frees it from the reduced mercury which remains at the bottom of the vessel.

This colouring matter, obtainable of various tints according to the quantity of oxidising matter employed, has not yet been used for dyeing, on account of its instability under the action of acids, which by neutralising the alkali gives to rosolic acid its arnoffo colour; but I trust that trials now in operation will result in utilising it for dyeing.

A fact, which I re-mention here, relating to phenic acid, is, that if heated with bichloride of mercury this acid will also yield rosolic acid, at the same time producing hydrochloric acid; and, with a distilling apparatus, rosolic acid can be collected in the retort, while the excess of phenic acid, saturated with hydrochloric acid, condenses in the receiver and fuming excessively in the air.

Rosolic acid proceeds from oxidised carbolic acid; for if the latter is represented by the formula $C_{12}H_6O_2$, that of the former becomes $C_{24}H_{12}O_6$. This formula is given as the result of three analyses.

On Certain Copper and Zinc Alloys, by M. E. KOPP.

AN alloy, patented by M. Gedge (*Dingl. Polyt. Journ.*, vol. clviii. p. 273), remarkable for its malleability—for when hot it can be forged, stamped, rolled, and drawn with the greatest ease—gives, on analysis, the following numbers:—

Copper	.	.	.	60.00
Zinc	.	.	.	38.15
Iron	.	.	.	1.50
				99.65

The proportion of zinc can be increased to 44 per cent. This alloy, which is recommended for sheathing vessels, seems identical with Aich's alloy (*Technologiste*, May, 1861, p. 429), to which the following composition is assigned:—

Copper	.	.	.	60.00
Zinc	.	.	.	38.20
Iron	.	.	.	1.80
				100.00

The colour of this alloy is rather darker than brass; its fracture is paler, bending at a red heat, and of fine grain. It takes a high polish. Its density is 8.37 when annealed, and when hardened 8.40. It is extremely ductile, though not so flexible as brass. Experiments made in the Austrian marine arsenals show that the Aich's metal possesses a high degree of tenacity; that it maintains its properties at red heat, provided the temperature is not raised to a clear red heat; that it can be puddled, hammered, and worked like the best forged iron; that when cold it can be considerably bent without cracking or breaking; that its absolute and relative resistance approximates to that of iron of good quality.

It is very probable that these alloys are nothing more than malleable brass or Chantz metal, concerning which M. Storer published a most interesting paper in the *Repertoire de Chimie Appl.*, 1860, p. 213. It is very likely that the iron is useful only in diminishing the nett cost of these alloys.—*Repertoire de Chimie*.

On the Manufacture of Iron,¹ by E. RILEY, F.C.S., late of the Dowlais Iron Works.

THE author commenced by stating that, having considered the various ores and coals used in the manufacture of iron and the working of the blast-furnace, he should proceed to the pig iron, and the different processes it underwent in its conversion into merchant bars and rails.

The various qualities of pig iron were considered: they were known in commerce by the Nos. 1 to 6, Nos. 1 to 3 being foundry iron, and Nos. 4 to 6 grey forge iron; besides these there was also mottled and white pig. No. 1 iron was only produced when the furnace was in the best working order, and required a heavy burden of coal and limestone; it was, consequently, the most expensive iron to produce; any little irregularity in the blast furnace soon reduced the iron to the lower numbers, at the same time altering the cinder, which was very grey when No. 1 iron was produced.

On referring to the analysis of various qualities of pig iron (from Nos. 1 to 6) it will be seen that the percentage of silicium is directly as the quality of the pig, being the highest in No. 1, and lowest in No. 6; whereas the sulphur is inversely as the quality of the pig, being the lowest in No. 1, and highest in No. 6. The phosphorus is a constant quantity. The samples of pig analysed were made from the same materials. The carbon does not differ very materially in quantity in the grey iron, but as you get on to mottled and white the carbon then becomes either partially or wholly combined with the iron, whereas in very grey iron the carbon all exists as graphite.

When the sulphur is present to any extent in an iron ore it is difficult to obtain grey iron, and when it is in large quantity it becomes almost impossible, unless it be mixed largely with other ores, the effect of sulphur on grey iron being to liberate the carbon and reduce the quality of the iron. Experimentally, if sulphide of iron be mixed with grey iron, graphite is separated, and if the sulphide of iron is used in any quantity the iron becomes white; thus it is that finery and forge cinders always make, when used alone, a white iron, from the high per-centage of sulphide of iron they contain.

In considering the quality of grey pig iron for foundry purposes, it must be admitted that the chemical composition of the pig is not necessarily an indication of its strength; and it is always necessary that its physical condition should be considered, although, as a rule, which generally holds good, you will find that pig iron which contains a high per-centage of silicon is weak, and more especially so when, besides this silicon, the iron contains a high per-centage of phosphorus, which is generally the case in iron made from the oolitic ores. The sulphur cannot be present to any extent in very grey iron; it never exceeds, and is generally below, .05 per cent. in No. 1 and No. 2 foundry pig. Pig iron, after being re-melted, is generally increased in strength. This is due more to an alteration in its physical condition than

¹ Abstract of a Paper read before the Society of Engineers.

to any chemical change, and is probably due to the iron becoming more homogeneous and uniform throughout. It has been found practically that a very siliceous iron, by being re-melted, was greatly increased in strength, without in any way altering its chemical composition.

It would naturally be asked, has the chemical composition of pig anything to do with its strength, and is not its physical condition the main point? The author considered that the chemical composition was of great importance; at the same time, he admitted that the composition of pig iron was not well understood, more especially as to the way in which the various elements existed and were combined. He also thought that some elements existing in pig had hitherto not been detected; he had himself, in several instances, detected titanium, which, he believed, had not been found before. Again, the elements in pig iron—more especially carbon and silicon—were capable of existing in very different states, known to chemists as allotropic conditions (or the same element existing in a different physical state, and not unfrequently playing a different part in combination). Thus, we have carbon as the diamond, graphite, and charcoal; and we have yet to learn, not only in what condition the two elements above-named exist in various qualities of iron, but also, from the great similarity in many of the properties of them, whether they may not, in fact, replace, to some extent, each other; and thus, possibly, you may have silicon playing the part, and, in fact, become equivalent to, carbon in certain cases.

It has recently been shown, by Professor Brodie, that carbon has, or ought to have, three different atomic weights, and you may have carbon existing in its three states in the various qualities of pig, altering it physically, and differing only chemically, in the aggregation of its atoms, the actual amount of carbon being identical.

Undoubtedly as our knowledge and experience increases we shall be able to give more satisfactory reasons than we can at present, as we are only able to draw general conclusions, which, I believe, as a rule are correct.

Pig iron from various localities was then considered, the Welsh being generally noted for its strength. It was not so fluid as the Scotch and Staffordshire, which was more adapted for light castings. The general effect of silicon and phosphorus was to make iron more fluid, but to diminish its strength. The hot blast iron was also considered more adapted for some castings than others.

Various processes were noticed for toughening pig iron, which generally consisted in reducing the impurities by a process of refining or oxidation, or mixing wrought iron or metal with the pig.

The treatment of the pig in the refinery and the construction of the latter, were next considered. Formerly the pigs were re-melted in the finery, but in many works the iron was run direct from the blast-furnace into it. White pig was much more easily blown than grey pig; the former quality was generally used in Wales for rails and bars, and it is only when the very best quality of iron is made that grey pig is used.

The finery has, however, in many works been superseded, and the pig is taken directly to the forge and puddled. When pig is used in the forge, it is called boiling; when metal puddling, in some works it is still found advantageous to use the finery partially, and it is only when the pig is of good quality that it can be done away with altogether.

The author then referred to diagrams of the puddling-furnace, and noticed the great improvement that had

been effected by the use of cast-iron bottoms. They were generally one and a-half to two inches thick, and rested on bars so as to allow the air to keep them cool. In boiling pig, the action of the iron and cinder (which was much more fluid than in puddling) was very great on the sides or bushes of the furnace, and great difficulty was experienced in keeping them intact. Limestone had been found to stand better than anything else, although it made the iron red-hot, and was objectionable on this account. Red ore had been used, and a mixture of red ore, hammer slag, and lime with more or less success; also spathose iron ore had been found to stand very well, and in America steatite, or soapstone, had been used successfully. Some works had a mixture of their own, which was kept secret, but generally consisted of red ore hammer slag and lime, in various proportions. The maintenance of the bushes of the puddling-furnace was of great importance in an iron works. In Staffordshire, calcined tap cinder was used for the bottoms of puddling-furnaces.

The various qualities of pig, and their working in the forge, were next considered; in Wales, where white pig iron was used, there were two, perhaps three, varieties, which worked very differently in the forge. First, you have what is technically called "strong iron." This iron is more or less siliceous, and cast solid with a convex face when cast in sand or ashes; with a concave face, when cast in chills (cast iron moulds). It is very sonorous, and rings like a bell when struck, and breaks with a perfectly solid white and crystalline fracture. This is the best description of white forge iron, and will make bars or rails of excellent quality.

The next description of white forge iron is when the pig contains a high per-centage of sulphur and phosphorus; the iron is then thick, and runs into the chills or sand with a very rough face, covered with warty excrescences, strikes dead, like lead, when struck, is frequently not solid, and is of a dark white colour; this iron is known as "weak iron," and is always produced from finery and forge cinders, when they are used in any quantity on the blast-furnace.

When very siliceous iron ores are used, the tendency is to get a siliceous pig. Finery and forge cinders do not, as might be anticipated, make a siliceous iron, except when they have been calcined and rendered infusible.

In some cases you have pig iron siliceous, and weak at the same time, from the presence of sulphur and phosphorus. The pig is then of the worst possible kind.

Siliceous pig iron is much more difficult to refine or puddle than weak pig. The author mentioned a case where white pig iron, containing $3\frac{1}{2}$ per cent. of silicon, was blown for seven hours in a finery, and was not then metal; this pig, when puddled, worked to a yield of 27 cwt. of pig to a ton of puddled bar, and after two heats the men would not work any more. It was found in practice advantageous to mix the strong pig with the weak in the forge. The author considered that silicon, to a certain extent, in forged pig was advantageous. He found that the best white forged pig contained from 1 to $1\frac{1}{2}$ per cent.; also pig made wholly from Welsh mine contained the same amount. This per-centage of silicon gave what was known as strength to the iron, but when the silicon exceeded the above-named proportion, then the iron became difficult to work; the effect of the silicon was to retard the puddling process, and thus it gave time for the impurities to be more completely separated.

The skill of the puddler was of much importance, and the author warned all engineers not to rely on single

experiments made on the yield of pig iron. The only way to get at the true result was to take the working of a whole forge.

The average quantity of coal used to make a ton of puddle bar in Wales was 17 cwt., in some cases less, and the quantity of pig from 22 to 23 cwt.

The next point considered was the mechanical treatment of the iron. This the author considered a very important one, and quite irrespective of its chemical composition. In most works in Wales the squeezer is used, and in some works a patent squeezer, consisting of three grooved eccentric rollers. The latter was considered preferable to the former. The other mechanical means of compressing the iron were the old tilt hammer and the steam hammer. Formerly the old tilt hammer was the only means of compression used, but since inferior iron has been made by driving the furnaces and using impure ores, the iron would not stand the old tilt, and the squeezer was introduced, by the use of which almost anything can be shingled into a bloom and passed through the rolls, whereas, with the old tilt hammer, the iron from the first has to bear the full weight of the blow. This does not apply to the steam hammer, as with it the blow can be regulated to any nicety.

Lately, engineers, in their specifications, have stipulated that the iron shall be hammered and not squeezed, and the plan has been adopted of doubling the puddle balls by slightly hammering them, placing them one on the other, and welding them into a slab for the head of the rail; this makes the rail head crystalline, and not fibrous, as it is only once submitted to the drawing action of the rolls.

The iron, after squeezing or hammering, is passed through the rolls, and rolled into No. 1 iron or puddle bar, which is cut by shears into the requisite lengths for the subsequent pile.

The different methods of making the various piles for rails, &c., were entered into, the great object in piling being to get the pile as solid as possible and free from interstices. The piles were frequently made of puddle bar, of different widths, piled alternately so as to break the joints, and also, in some cases, the puddle bar was cut in short lengths equal to the width of the pile, and each layer piled at right angles to the previous one. This had a tendency to make the iron less fibrous.

The various sections of rails in use were then alluded to. The double-headed rail generally was made with slabs at the top and bottom of the rail. For the best quality of rails they were about two inches in thickness, and the rails were double heated. Some engineers had their rails made of three hammered slabs, one piled on the other.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Saturday, March 23, 1861.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. EDWARD FRANKLAND, F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

LECTURE X.

It is now nearly a century ago since Scheele, the great German chemist, mixed binoxide of manganese with hydrochloric acid. He noticed that a gaseous body was disengaged possessing a greenish yellow colour, and

exhibiting very remarkable properties. He investigated it to a certain extent, and the discovery of this remarkable and new element soon attracted the attention of other chemists, especially of several chemists in France, who continued the investigation of the body. The substance was produced, as I have said, by the action of binoxide of manganese—a body, therefore, containing oxygen, and we may say oxygen in excess—upon hydrochloric acid—a compound which at that time was termed “muriatic acid,” sometimes, also, “marine acid,” as being an acid obtained from sea-salt. On account of this origin of the new substance which Scheele had discovered—namely, its origin in muriatic acid and an oxidising body like binoxide of manganese—it received the name of “oxymuriatic acid.” The chemists of that day thought it was produced by the union of oxygen with muriatic acid. This body being discovered about the time that Davy was beginning to prosecute his interesting chemical researches, very soon attracted his attention, and from experiments which he made upon this body discovered by Scheele he soon found that the view of it then held by chemists was perfectly untenable; that, in fact, it had as good a claim to be considered an elementary substance as oxygen, hydrogen, nitrogen, carbon, or other elements known at that time. He proposed for it the name “chlorine,” from a Greek word signifying greenish colour, that being the colour of the gas. Here is some of it in this vessel. This view of Davy, or, we should say, this fact, which Davy proved by experiment, that the body could not be decomposed—that constituting our definition of an element—was not generally received, and Davy's view of chlorine was for a long time contended against. I remember myself the time when it was not considered particularly unorthodox for chemists still to look upon this substance as an oxide of muriatic acid.

This element, chlorine, which is in many respects a very interesting counterpart of oxygen, although differing in other respects widely from that element, may be obtained by several processes, but may be best procured by the one to which I shall confine myself. This process consists, in fact, in the identical operation by which Scheele first prepared this gas. It consists in placing the binoxide of manganese, containing one equivalent of manganese and two equivalents of oxygen, in contact with hydrochloric acid which is prepared largely for manufacturing purposes, and which is therefore readily obtainable. The solution of that acid is the liquid which we have in this bottle. If we mix together these two substances and apply heat to them, this gas, chlorine, is evolved at once in large quantities; and the mode in which it is evolved, and which was first rendered clear by Davy, will be made evident to you by these cubes. Here is our muriatic or hydrochloric acid; we will take two atoms of it; we shall then consequently have two equivalents of hydrogen and two equivalents of chlorine. That is one of the substances. The other substance we use is the binoxide of manganese, containing one equivalent of the element manganese and two equivalents of oxygen. These bodies placed in contact with each other act in this way: the two equivalents of oxygen combined with the manganese take out from the hydrochloric acid the two equivalents of hydrogen contained in those two atoms of the acid. They come out, therefore, thus [pairing off the cubes representing the hydrogen with the cubes representing the oxygen]. There is one atom of the new compound,—there is the other. Now, what is this that we bring out? Hydrogen and oxygen in equal proportions—equal equivalents: this is, obviously, water. We thus have two equivalents of water separated by this reaction; and one equivalent of manganese and two equivalents of chlorine are left. These two equivalents of chlorine cannot remain combined with the manganese, if the temperature be slightly elevated, that is to about 80 or 90 degrees Fah.; and the result

of the decomposition which may be regarded as taking place simultaneously with the separation of these two atoms of water is, that one of the equivalents of chlorine combines with one equivalent of manganese, forming the metallic salt, chloride of manganese, which remains dissolved in the water; and then we have one equivalent of free chlorine left. This was the re-action which Davy demonstrated to take place in this process of mixing hydrochloric acid with binoxide of manganese, and exposing the mixture to a gentle heat.

Now, this element—chlorine—is remarkable, in the first place, for the great intensity of its affinities. It is difficult to say whether oxygen or chlorine possesses the most powerful affinity for other substances. There is a sort of contention for the mastery between these two elements. Sometimes we have chlorine expelling oxygen, from its combinations, and we have sometimes oxygen expelling chlorine. We may say that the affinities of the two bodies are very nearly equal, but there is just this peculiarity about chlorine, that its affinities are generally capable of being exerted at ordinary temperatures. We have found that oxygen requires usually the application of an elevated temperature to bring its affinities into play. When we wish oxygen to combine with hydrogen, for instance, we have to apply heat to the mixture in order to bring about that combination; but chlorine, on the other hand, is capable, in a great majority of cases, of combining with other substances at ordinary temperatures.

On account, first of all, of its very powerful affinities, and, secondly, of its property of combining with other bodies at ordinary temperatures, chlorine never exists free in Nature; we never can find a trace of chlorine in a free state naturally. It exists, in fact, almost entirely in one form of combination, namely, in this form—a form in which all of us know it very well—as common salt, or chloride of sodium, of which this is a native specimen [exhibiting a piece of rock salt]. The great bulk of this material exists chiefly, as you are aware, in the waters of the ocean. Considerable portions of it have, however, been deposited in the bowels of the earth by the evaporation of inland seas, possibly, or by some other geological process. We make borings, and send down water which absorbs the salt, then pump up the solution and evaporate it, and thus we obtain our supply of chloride of sodium. This natural compound of chlorine contains one equivalent of sodium and one of chlorine. In addition to this compound, we meet with it, though in much smaller quantities, united with potassium, with calcium, with mercury, and occasionally, though very rarely, with silver. The three chief compounds of chlorine which we meet with are those with sodium, potassium, and calcium; and if you look towards our list of elements you will find that these bodies belong to the metals, or to the most positive of our positive elements, whilst chlorine is one of the most negative of our negative elements. The consequence is, that the chlorine which exists in this chloride of sodium is retained there by a very strong affinity, and this metal most effectually neutralises the negative energy of the chlorine. I will not prepare any of this gas before you, because its preparation is always attended with the escape of some of it into the room, and being an exceedingly irritating gas, it would be very unpleasant to prepare it here. I have, however, several jars of it already prepared, and in collecting it it is necessary either to receive it over warm water, or to collect it by the process of displacement. We cannot collect it over mercury, because it immediately combines with the mercury at ordinary temperatures. There again you see it differs from oxygen, which does not combine with mercury at ordinary temperatures. Cold water also absorbs from two to three times its own volume of this gas, and, therefore, there is a considerable loss if we collect it over cold water.

Chlorine possesses the following properties:—In addition to the colour, to which I have already directed your atten-

tion, it has a powerful and exceedingly irritating odour. I dare say most of you know this odour. It is the odour that you perceive to issue from that disinfecting substance called chloride of lime, when it is exposed to the air. In that case you have it in a very mild form indeed. The quantity which is given out to the air from the chloride of lime is very slight, but if you attempt to inhale it in the concentrated form, the irritation to the air-passages of the lungs is perfectly unbearable, and in a concentrated form a couple of cubic inches is quite sufficient to deprive a man of life, if it gain access in its undiluted form to his lungs. The specific gravity of this gas is about two and a-half times that of the atmosphere, and therefore we can collect it in such tall vessels as these without much difficulty. It is one of the gases, also capable of being liquified. It requires the pressure of only four atmospheres to liquify it, and then it condenses into a yellowish, mobile liquid, the properties of which were first investigated by Mr. Faraday. It is, as I have said, distinguished for its intense negative energy, and for its power of combining with positive elements with great energy. But it is very peculiar—in its taste, I was almost going to say—but at all events in its selection of elements to combine with, it differs considerably from oxygen.

In the first place, as we have already noticed, its affinities are capable of being exerted at ordinary temperatures. Here we have a jar of it, and into this jar I will dust a little powdered metal, and in order that you may not be annoyed with the odour of the gas more than can possibly be avoided, we will perform the experiment under the ventilator. Here I have in this little gauze bag some metallic antimony, reduced to a fine powder, and when we shake it over the mouth of the jar, and cause the powdered metal to fall into the chlorine, you see, the moment it comes in contact with the gas, it ignites, and the two combine, forming chloride of antimony. We may repeat this experiment with powdered arsenic, and several other metals. We may also ignite sodium or potassium in this gas, for those metals are capable of burning in chlorine with very great brilliancy; but I think we had better dispense with any further experiments of this sort, as they might give us too large a quantity of chlorine in the atmosphere. Some of the non-metallic positive bodies—phosphorus, for instance—are also peculiar for possessing powerful affinities for chlorine, and if we introduce a piece of phosphorus into a jar of this chlorine gas, it at once takes fire, and continues its combustion. It is not necessary to ignite it before introducing it into the gas, as in the case of oxygen.

In making this experiment with phosphorus, I wish you also to notice some facts with regard to the light of the combustion, for it is a very instructive experiment. In burning in chlorine gas, phosphorus produces a volatile substance, the chloride of phosphorus, furnishing no solid particles from which the light can radiate, as it does in ordinary gas flames, or flames generally luminous. Now, I will first plunge the phosphorus into the jar of chlorine; it will ignite spontaneously, and you will see that it will burn with a very dim light, scarcely perceptible, I should imagine, at a distance. I will then bring the phosphorus out into the atmosphere, and then it will burn at the expense of oxygen, and form a solid, non-volatile body—phosphoric acid and the phosphorus on emerging from the chlorine, will emit a very considerable amount of light. But the amount of light thus emitted, depends very much upon the temperature of the combustion, and, therefore, if we transfer our phosphorus from the atmosphere into a jar of oxygen, we shall then see the difference of luminosity which occurs in that case simply by an increase in the temperature, because in the air we have the same product, namely, phosphoric acid formed in the same quantity; but in the jar of oxygen you have not the effect of atmospheric nitrogen to diminish the heat and consequently the light evolved by the flame.

There is our piece of phosphorus, and it at once ignites when it comes into the chlorine, and you see with what a very minute amount of light it burns there. Now, we will bring it into the air, and you see the very much greater luminous effect it produces here; and lastly we will plunge it into the oxygen, when the light becomes almost too intense for the eye to bear. The only difference between the combustion in air and in oxygen being the great elevation of temperature which takes place when combustion occurs in the latter.

Chlorine possesses some peculiarities in its tastes, so to speak, for combination. It unites with hydrogen and with the metals, with very great energy, but it is incapable of uniting directly with carbon. At any temperature to which we like to expose the two elements, they will not unite. Carbon, you will recollect, burns in oxygen at an elevated temperature with very great energy indeed, and we have some forms of carbon which take fire in atmospheric air without any application of heat whatever, but you cannot by any such direct process induce carbon to combine with chlorine.

This renders the action of chlorine on any ordinary combustible very peculiar. If we plunge a taper, for instance, into a jar of this gas, the taper will continue to burn for a few moments, but it is very soon extinguished. The taper at present burns with its ordinary luminous flame. Now we will plunge it into this jar of chlorine, and you see what an immense amount of smoke it produces, and how dimly it burns, and I think we shall find presently that it will be extinguished. It is now extinguished. It is incapable of continuing its combustion in the chlorine for this reason—the chlorine is only capable of uniting with the hydrogen of the combustible, and it separates the carbon of the wax taper. The smoke that was given off from the flame was due to the separation from the wick of the particles of carbon which are totally consumed while the taper is burning at the expense of oxygen. These particles of carbon gradually choke up the pores of the wick, and prevent the ascent of the wax to the summit of the wick where it would be consumed, and therefore the taper after a short time is extinguished. A gas light is capable of burning in chlorine for any length of time. It burns with a smoky flame, but it contains so much more hydrogen than the candle-flame, whilst it has no wick to become choked, that it is capable of continuing its combustion. There is another experiment illustrative of the same thing. Camphire is essentially a compound of hydrogen and carbon, like the wax of the taper, and if we spread it over a large surface of paper we see very strikingly the relative affinities of the chlorine for the hydrogen and for the carbon. We shall find, I think, if our turpentine is sufficiently pure, that it will be spontaneously ignited by the chlorine; the chlorine will attack the hydrogen of the turpentine, and the whole of the carbon will be deposited in a dense cloud of smoke as in the case of the combustion of the taper. I will pour a few drops of this camphire upon paper and then plunge it into this jar; you see we have it spontaneously ignited and the carbon separated from it as a dense fuliginous cloud, which in fact has covered the interior of the vessel with a mass of carbon, rendering it perfectly black. This is one of the peculiarities, then, of chlorine—its powerful affinity for hydrogen and its almost total want of affinity for carbon. It is true that we can get compounds of carbon and chlorine, but we can only get them by indirect means. This is effected by first combining carbon with some other element, and then removing that element by means of chlorine; we cannot make them unite directly in this process of combustion.

Another of the peculiarities of chlorine, serving to distinguish it from oxygen, is the effect of light upon its affinities; in fact light is to chlorine, in a great measure, what heat is to oxygen. We have seen that the application of heat to mixtures of oxygen with combustible

elements, as hydrogen, for instance, is capable of causing those elements to combine. In the same way the application of light in some cases to mixtures of chlorine with similar elements causes them to combine. I will endeavour to render this visible by an experiment which I intend to make here: it is an experiment which requires a good many precautions to make it succeed, and as I have not had an opportunity of trying it before the lecture, I cannot promise you success. What I am going to do is this,—I am going to mix together hydrogen with chlorine in one of our collodion balloons. These two gases are produced by the electrolysis of muriatic acid, which is torn asunder in the same way as water, and we have its two elements, chlorine and hydrogen, evolved in equal volumes. If we then connect our balloon here with the exit tube of this decomposing apparatus, we shall be able, I trust, in this way, to fill it with the mixed gases. [The collodion balloon was attached to the decomposing apparatus and the room was darkened.] We shall soon have filled that balloon with the mixed gases; and you see we are doing it with the exclusion of daylight, because that would cause the gases to ignite or combine, and defeat our experiment. We have here two parabolic reflectors—copper reflectors, silvered. We shall produce the electric light in the focus of the lower one, and as soon as our balloon gets near the upper one, the effect of the lower one will be to collect and gather up all the rays given off from the coal points which we shall here ignite, and to send them as a solid cylinder of rays, so to speak, against the upper reflector, and the upper reflector will receive this cylinder of rays, and will concentrate them to a point at some five or six inches lower than the level of the upper reflector; and thus we shall have our balloon exposed to this intense light so concentrated. We will just try our mirrors, and ascertain if we are likely to get a sufficient amount of light upon the balloon to effect the experiment. Whilst the balloon is filling, I will show you the corresponding experiment with the mixed oxygen and hydrogen gases which we will cause to explode by heat, just as we hope to make the mixed hydrogen and chlorine explode by light. For this purpose we will inflate another balloon with these mixed gases. [A collodion balloon similar to the one being inflated with the hydrogen and chlorine was attached to the vessel containing the mixed hydrogen and oxygen.] Now, you see, our second balloon is inflated, and I think it will possess a sufficient ascending power to reach the top of the theatre, and we will set light to it by means of a little bit of thread impregnated with nitre, which will act as a fuse; and when that fuse burns so as to come in contact with the collodion balloon, it will ignite the envelope, and will communicate a sufficient amount of heat to the mixed gases to cause the mixture to explode. [The balloon ascended to the top of the theatre, and a light having been previously applied to the thread, the latter burnt away until the fire reached the balloon and exploded the gases.] There, you see, is the result; the two gases ignite; and now I hope to produce a similar effect with the chlorine and the hydrogen by the agency of light. I think we may, perhaps, have a sufficient quantity here for our purpose. We will at once bring our balloon up to its proper position, and you will see the result. [The balloon was brought into focus, and immediately exploded.] You see, our two gases by the action of light produce just the same result as we produced in the other case by the action of heat; the moment the two coal-points were separated that concentrated light fell on the balloon, and produced this effect, which is not due to heat, as you see the envelope of the balloon is still unconsumed. Thus light is to chlorine to a certain extent what heat is to oxygen.

We have the same intense affinity of chlorine for hydrogen coming strikingly into play also in the processes of bleaching and disinfecting by chlorine, for chlorine is one

of our most powerful disinfectants. I will now endeavour to show you the mode of bleaching by means of this agent, chlorine. For this purpose we will use a solution of the gas. Here we have a cylinder full of a decoction of cochineal, which possesses intense colour. To this we will add some of the chlorine-water, and you see where it descends how the colour disappears. I do not know whether we have added quite sufficient, but probably we have enough to destroy the whole of the colour contained in that cylinder. You see the colour is gone under the action of the chlorine. Now, the action of the chlorine in destroying colour is the same as in the disinfection of noxious compounds. It consists in the removing, more or less completely—in the one case from the coloured compound, and in the other case from the noxious compounds which we wish to render innocuous—the hydrogen which those compounds contain. Let me try to illustrate this by some of our blocks. We will collect together here some atoms of chlorine; and here we will form a hypothetical miasmatic atom if you please which shall consist of nitrogen, carbon, hydrogen, and oxygen. This we may regard, then, as a hypothetical miasmatic atom, containing carbon, nitrogen, hydrogen, and oxygen. Such an atom is gradually undergoing oxidation; the hydrogen is gradually removed by atmospheric oxygen and replaced by the latter element. So long as this goes on, so long does this atom retain its noxious property. Before the process begins, it is innocuous; when it is completed, it is again innocuous. It is only while it is going on that noxious effects are to be apprehended. Now, the action of chlorine upon such a substance is to complete at once this process of removing the hydrogen from the atom. I will show the mode in which the hydrogen is removed. Two atoms of chlorine are required for the removal of each atom of hydrogen from this infectious matter. It is done in this way. One of these atoms of chlorine takes out an atom of hydrogen, brings it out in the form of hydrochloric acid, and the other atom of chlorine goes into the place of the hydrogen; so that you see now we have a body containing one equivalent less of hydrogen than we had before, and that hydrogen is replaced by chlorine. If this operation be repeated, we shall then remove the whole of the hydrogen from this noxious substance, and we shall then have replaced the whole of this by chlorine. The substance is no longer spontaneously oxidisable, and is deprived of its power of burning or slowly oxidising in atmospheric air. Its hydrogen is completely removed by the chlorine, and its noxious qualities are destroyed, it being no longer capable of undergoing oxidation, or of producing noxious effects.

The action of light upon chlorine, in producing compounds of that element, leads us naturally to speak, in the first place, of the action of light generally upon chemical affinity. This influence is far more restricted than the influence of heat upon the chemical force; nevertheless, the cases in which it occurs are of very great importance, and very considerable interest. For instance, that operation to which I alluded in the last Lecture,—the operation by which plants are capable of removing from the air carbonic acid thrown into the atmosphere by combustion and respiration, and returning to the air the oxygen contained in that carbonic acid,—can only take place in the presence of light; it cannot be carried on without luminous influence. This, then, is an instance of a grand modification of chemical affinity which is taking place all around us, and on which, in fact, life almost entirely depends. Of this process we may say we know simply nothing, for we have not the slightest idea how, by the action of light, the leaves of plants are capable of decomposing carbonic acid; we cannot imitate it in any of our processes; we can only effect it by the agency of our most powerful affinities. In addition to this grand operation of light in modifying chemical affinity, we have several others which are of considerable importance. We have this

effect upon the affinities of chlorine, bromine, iodine, and silver; these are the chief elements, in fact, on the affinities of which light is capable of exerting an influence; and if I may be permitted at all to generalise upon this influence, I should say that in the case of chlorine the affinities of that element are almost invariably increased by the action of light. The affinity of bromine is little affected; that of iodine and of silver is greatly depressed under the influence of light, so that a compound which iodine would be capable of forming in the dark it would not be capable of forming in the light; whilst a substance which chlorine would be incapable of forming in the dark it could produce under the influence of light. Now, the different rays of which white light is composed, do not all of them exercise this influence upon affinity, it is only one class, or one set, of those rays, the most refrangible of them, those that are capable of being most turned out of their course by passing from one medium to another, it is only these most refrangible rays that are capable of exerting this influence. I will endeavour to show you upon this screen the rays to which I refer, and we will endeavour then to get some chemical effect from those rays. [The room was darkened, and the prismatic spectrum thrown upon the screen.] There you have what is already well known to you, namely, the spectrum of the electric light—a beautiful series of colours, beginning at one extremity with red, then orange, then a band of yellow, then of green, then a band of blue, and finally, a band of indigo, but it is only the rays at this blue extremity of the spectrum that exert this chemical action; in fact, we may say that the rays at the other extremity rather neutralize what has been done by the rays at the blue extremity. These so-called "chemical rays," are invisible to the human eye; possibly, they cannot penetrate to the retina; at all events they are incapable of giving us the sensation of light when they reach it, supposing they do arrive there. Now, I will endeavour to make these rays visible to you by a process for which we are indebted to Professor Stokes. I have here a piece of uranium glass. You see this glass becomes luminous in the invisible rays of the spectrum beyond the blue. A piece of paper covered with sulphate of quinine will give us a somewhat similar result. That paper is illuminated in the dark portion of the spectrum, and these are the very rays which affect chemical affinity to the greatest extent. We will now interrupt this spectrum, or intercept it on its way to the screen by a photographic plate, by means of which, I think I shall be able to obtain an image of the spectrum, and to show you the way in which these rays modify chemical affinity. We will first get our image upon a sheet of ground glass, and in such a position as will enable us to receive it upon a glass plate prepared with collodion. There we have a tolerably sharp representation of the orange, the yellow, the green, the blue, and the indigo; and now the chemical effect will only begin at the line where you see the luminous spectrum ends, while the other or visible part will produce little or no chemical effect in the operation we are now going to perform. Here I have a sheet of glass upon which I will spread a film of iodised collodion—collodion containing iodide of potassium, and we will now immerse this plate in a solution of nitrate of silver, so as to precipitate by the ordinary photographic process, in fact—the so-called collodion process—a film of iodide of silver upon the surface of the plate. That will be precipitated in one or two moments, and then we will expose that film to the action of the spectrum, which we have before obtained on the ground glass. We will remove the ground glass, and substitute for it our prepared plate. Here, you see, we have a yellow film of iodide of silver produced upon that plate. It is necessary first to secure the complete evaporation of the ether in which the collodion was dissolved; that, I think, we have now sufficiently effected, and you see we have a yellow film here. I do not want to expose it too much

even to the light of this gas, but some of you may, perhaps, wonder why I am operating here with this comparatively brilliant light. The fact is, that this gas-light contains very few of those rays which I rendered visible to you on the spectrum, and, consequently, it is incapable of producing any effect on this plate, and I may work with this gas flame for a short time, at all events, with impunity. Now we will expose the plate to the spectrum. I will make a mark here where the luminous spectrum goes to. It is a very rough mark, but it will do for our purpose. I have just marked the extent upon the plate to which the luminous part of the image of the spectrum comes, so that you may be able to judge when we throw the image of this plate upon the screen, after we have brought out our effect, whether what I have said be correct or not, as to these chemical rays existing upon the outside of the spectrum. The spectrum has now acted sufficiently, and I pour upon this plate what photographers term a "developing solution," which is a solution of pyrogallie acid. This enables the surface of the iodide of silver, where it has been exposed to the light, to decompose the nitrate of silver in the presence of this pyrogallie acid, and to produce an image on the plate. You see we have here an image of our spectrum. Now, I will remove the sensitive coating from the plate by means of a solution of cyanide of potassium, so as to make it more transparent in those portions which have not been acted upon by the light. Having thus got our effect, let us examine in what direction that effect has been produced, whether it has been produced, or much of it produced, by those luminous rays, or whether we have been indebted chiefly to the non-luminous portion of the spectrum. I will try to throw an image of our result upon the screen by means of this lens; and now we will interpose our plate, and there is the result we have got. You see a great chemical effect has been produced on the right hand of that little notch which I cut with a strip of wood on the plate while the photographic process was going on. That line marked the extent of the visible spectrum. To the right all is invisible on the plate, and in that part we had the visible spectrum on the screen. You see to what extent in that direction the chemical effect has gone, and how very much more the rays have operated to the right than to the left of it.

I shall now be able only to glance very briefly indeed at the compounds of chlorine. We have the names and composition of these compounds upon these diagrams:—

Compounds of Chlorine.

I. With Hydrogen.

Hydrochloric Acid . . . HCl

II. With Metals.

Protochlorides . . . MCl
Sesqui-chlorides . . . M₂Cl₃
Bichlorides . . . MCl₂
Terchlorides . . . MCl₃

III. With Oxygen.

Hypochlorous Acid . . . ClO
Chlorous Acid . . . ClO₂
Peroxide of Chlorine . . . ClO₄
Chloric Acid . . . ClO₅
Perchloric Acid . . . ClO₇

Chlorine forms only one compound with hydrogen—hydrochloric acid,—from which Scheele first obtained this element. This compound, as I remarked before, was at that time called muriatic acid. With metals it forms four principal kinds of compounds, namely, first the compound in which one equivalent of metal is united with one equivalent of chlorine, these are called protochlorides; secondly, sesquichlorides, or bodies containing three equivalents of chlorine to two of metal; thirdly, bi-chlorides, or bodies containing one equivalent of metal and two of chlorine; and, lastly, ter-chlorides, or bodies containing one equivalent

of metal and three of chlorine. Then we come to the compounds of chlorine with oxygen. These are exceedingly curious bodies, and if time had permitted, I would have shown you specimens of one or two of them and their reactions; but it is impossible that we can do that now. It is very singular that these two elements, which are so much alike in their chemical characters, should combine at all. They are the two most negative elements with which we are acquainted, and yet you see they form this numerous class of compounds. It may, in fact, always be regarded as a rule in chemistry, that, if two elements will unite, then the nearer their affinities, or, rather, the nearer their chemical qualities come together, the more numerous will their compounds be, but, at the same time, such compounds will be very unstable. The two elements now in question form some of the most dangerously explosive compounds with which chemists are acquainted, and it is very difficult to prepare some of them, even in small quantities, without fear of explosion. On the other hand, the compounds of chlorine with metals, and with the positive elements, are characterized by great stability, and if we have in the compounds one equivalent of metal united with one equivalent of chlorine, they are invariably all of a neutral character.

Some of the general considerations regarding this subject I have marked among the following memoranda, to which I can now do no more than refer you:—

1. Chlorine always occurs in Nature combined with positive elements, and chiefly with sodium, as common salt.
2. Chlorine is distinguished for its intense negative energy, and for its capability of exerting its affinity generally at ordinary temperatures.
3. It possesses a very feeble affinity for carbon.
4. Light frequently affects the affinities of chlorine, in the same manner as heat influences those of oxygen.
5. Chlorine exerts great bleaching and disinfecting powers. Its affinity for hydrogen endows chlorine with both these powers.
6. Oxygen, hydrogen, nitrogen, carbon, and chlorine may be considered as the chemical representatives of all the other elements; thus sulphur, selenium, and tellurium are closely allied with oxygen in their chemical habits. Most of the metals bear a strong chemical resemblance to hydrogen. Phosphorus, arsenic, and antimony are closely analogous to nitrogen. Boron and silicon possess considerable likeness to carbon; whilst bromine, iodine, and fluorine are the chemical counterparts of chlorine.

In conclusion, I have very briefly to direct your attention to the remaining elements which belong essentially to inorganic chemistry, and I trust that none of my audience have expected that I intended in these Lectures to give the history of the whole of these elements; it would have been utterly impossible in such a Course of Lectures. I have endeavoured rather to take representative elements, and make these illustrate the action of the chemical force—representatives symbolising the action of corresponding elements in a vast number of instances.

I have treated of oxygen, hydrogen, nitrogen, carbon, and chlorine, as representatives of five great classes of elements. Under these five elements you may group together all the other elements, and you may regard the reactions which we have seen to ensue with these elements as going through their respective groups, to a very great extent at all events. There are, of course, peculiarities in the case of each element; but the general characters of the reactions of these representative elements may be regarded as going through the entire of the five classes at the head of which they are placed.

Vulcanised India-rubber which is worn out, until it has become as brown as the pure caoutchouc, still contains an appreciable quantity of sulphur.—*Dr. L. C. Levoir.*

NOTICES OF PATENTS.

An Improved Grease for Lubricating the Frictional Surfaces of Machinery. C. N. LEZOR. Paris.

THIS lubricating composition is prepared from oil or fatty matters mixed with small quantities of the alkalies, potash and soda, in aqueous solution. The inventor recommends that the materials should be employed in the following proportions:—Of any serviceable description of lubricating oil take 33 parts, tallow 25 parts, caustic soda or potash mixed in equal proportion 2½ parts, dissolved in about 40 parts of water. These ingredients require to be well incorporated by kneading, when they will furnish a very white soft grease, suitable for lubricating the bearings and other moving parts of machinery without having any tendency to corrode the metallic surfaces.

The mixture of materials here specified would result in the preparation of a very crude and partially saponified soft soap. Some alkali would doubtless remain for a considerable time in the free state, and, although this may not exert any deleterious action upon surfaces of iron, there would be every probability of its acting injuriously upon brass, bronze, and some of the softer alloys occasionally used for the bearings of machinery. The want of definite instructions regarding the kind of lubricating oil to be employed with the tallow renders it impossible to define more exactly the chemical nature of the ultimate product. Whether olive oil be used, or any one of the class of mineral products obtained in the distillation of shale or coal, such as paraffin oil, either would equally answer to the term "lubricant;" but inasmuch as these oils differ widely in their behaviour with caustic alkalies, the first only being capable of saponification, the lubricating material would possess different properties accordingly as one oil or the other happen to have been employed in its preparation. Both soft and hard soap dissolved in water, and the same incorporated with various kinds of grease, have received in this country a wide application for lubricating purposes.

MISCELLANEOUS.

Stoppers of Caustic Solution Bottles.—It is well known that the stoppers of these bottles rapidly become incrustated; greasing is of little use, moreover, we have to consider that we may thus introduce fatty acids into the solution; now paraffine answers well: first, because caustic alkali does not act upon it, and secondly, because it perfectly lubricates the surfaces in contact.—*Polyt. Notizblatt.*

A Sensitive Reagent for Sulphur.—By M. Schlossberger.—A solution of molybdate of ammonia in hydrochloric acid diluted with water, possesses the property of colouring blue, if traces of sulphur are present. By this means the presence of sulphur even in a single hair is easily recognisable.—*Polyt. Notizblatt.*

Nitrate of Soda.—Nitrate of Soda in the neighbourhood of the River San Francisco, Bahia, Brazil:—I have the honour to acknowledge the receipt of your lordship's despatch of January 7 last, instructing me to report, after a careful inquiry, on the localities in which nitrate of soda has been found in this province, its state of purity, the cost of extracting and refining it, and the expense of transport to a shipping port. In reply I beg to state that ever since the mention made in my Commercial Report for the year 1856 of the discovery of this important manure in this province I have received undoubted information of its existence in several other localities, and in consequence lost no time in renewing my exertions, with the assistance of a few friends having connexions in the interior, to obtain every information possible thereon. The

information received from several districts is such as to satisfy my most sanguine expectations, especially in the north-east of this province, in the neighbourhood of the River St. Francisco, at a place called "Salitre." In the words of a most distinguished and talented native of the town of Joazeiro, through a valley of sixteen to twenty leagues long, one mass of nitrate of soda is found; in some places on the surface, at others a few feet under; and it contains sufficient manure for exportation for years to come. On the opposite side of the above-named river, near the town of Pilao-Arcado, other deposits exist in equal quantities. The projected Bahia railroad to the town of Joazeiro must pass through the first named district, but those magnificent deposits will only become available when that railroad reaches the St. Francisco. In the interest of the Company and of commerce it is ardently to be desired that its works should be commenced and prosecuted with vigour, as the distance is only 240 miles from the city.—*Extract from the Report of H. B. M. Consul.*

New Process for Preparing Nitrate of Silver.—By M. Greiner.—Dissolve impure silver in nitric acid, and when the liquid is almost neutral and cold, add a solution of sulphate of soda to precipitate the sulphate of silver, which is to be washed in distilled water; after this digest it with heat, with a proper proportion of nitrate of baryta, thus forming sulphate of baryta, to be separated by filtering, and nitrate of silver which can then be crystallised. It remains to be seen whether the product is pure; the facility with which nitrate of silver combines with alkaline nitrates, makes it likely that it would also have a certain affinity for nitrate of baryta.—*Journal de Pharmacie et de Chimie.*

ANSWERS TO CORRESPONDENTS.

* All Editorial Communications are to be addressed to the Editor; and Advertisements and Business Communications to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

C. Bades.—The fault is not in the cyanide solution, but in the way the iron has been cleaned. After it was removed from the "pickle" it should have been soaked in strong soda, and after being washed, instantly dipped into the cyanide solution. When the copper is properly precipitated it should be again well washed. You can get all the apparatus necessary for the electric light and experiments with vacuum tubes at Mr. Ladd's, Beak Street, Regent Street.

D. ONYON.—The subject is rather an intricate one. If we meet with any good information, it shall at once be published. We are obliged for the information and will endeavour to learn further particulars.

Riflemen's Belts.—To polish enamelled leather, take half a pint of the best cream, a quarter of a pint of linseed oil, make them each lukewarm, and then mix them well together. Having previously cleaned the leather, rub it over with a sponge dipped in the mixture; then rub it with a soft dry cloth until a brilliant polish is produced.

A Reader.—We have seen nitro-benzol prepared almost colourless by distilling it by means of steam at a temperature below its boiling point. Distillation in the ordinary way is apt to injure the delicacy of its odour.

Ice.—A correspondent writes:—"A friend of mine in the West Indies having lately seen a paragraph, evidently taken from a Paris paper, of a French gentleman who experimented before the Society of Arts with two quicksilver batteries, and having put one of them in the fire, after pouring in ammoniac, made ice. My correspondent having bought the utensils, is unable to work them, and has therefore requested me to ask your advice and assistance."

Chemistry of Brewing.—Preventing Beer from turning Sour.—We have the following from reliable authority:—"A French chemist has discovered a chemical compound, by which, upon the addition of a quantity of it, according to the quantity of beer operated upon, it prevents the beer passing into the acetous fermentation, and keeps it in a perfectly sound condition for an almost indefinite space of time, no matter the quality of the materials used in its manufacture; but as beer, &c., are excisable articles and the penalties heavy if found adding any substance otherwise than allowed, the discoverer goes to the Board of Inland Revenue, states the discovery he has made, wishes to know if any brewers using it would be prosecuted. The Board reply by saying it must receive a searching analysis by their chemists, to see whether it would be injurious to the consumers. In a subsequent communication from the Board, a reply is given that it possesses no injurious properties, and he is allowed permission to use it. He is about getting a patent for it, as to have used it without the sanction of the Board, prior to obtaining a patent, would have subjected him to heavy penalties."

THE CHEMICAL NEWS.

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SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Chemical Researches on the Assay of Silver by the Moist Way, by G. J. MULDER, Math. Mag. Phil. Nat. Medicina, Artis Obstetrica et Pharmaceutica Doctor, Professor of Chemistry at the University of Utrecht, Grand Cross of the Order of the Netherlands Lion; of the Luxembourg Order of the Couronne de Chêne; of the Swedish Orders of Wasa and Polar Star, &c., &c.

IN the latter part of the reign of King Charles X., the celebrated Gay Lussac made a proposition to introduce a different method for the estimation of silver. It had been proved that the method by cupellation gave, even in the hands of very able practical men, not always the same and satisfactory results; and further, it had also been proved, that this method was liable to faults against which it seemed there were no means of guarding. The new method adopted was the volumetrical one. A weighed quantity of the silver to be assayed for its real value is dissolved in nitric acid; another weighed portion of perfectly pure silver is also dissolved in the same acid. A solution of common salt in water is made so as to be of known strength, and so much of this solution of common salt in water is added to both solutions of silver in nitric acid, as is necessary to precipitate all the silver in each of the solutions.

Since common salt undergoes double decomposition in contact with nitrate of silver in such a manner, that nitrate of soda and insoluble chloride of silver are formed, and since common salt precipitates silver always in equivalent proportions, the quantity of the solution of common salt which has been used, can serve as a measure for the quantity of silver in the solution of silver which has been subjected to analysis. In the same way, the solution of pure silver gives the value of the measure, with which the silver to be analysed is to be compared. If, for instance, one gramme of the pure silver had been weighed, and also one gramme of the silver to be analysed, and if of the very same solution of common salt 1000 parts by liquid measure had been necessary to precipitate all the pure silver, and 900 parts only of that solution had been applied to precipitate perfectly the silver subjected to analysis, then the value of the silver to be analysed should be to the value of the pure silver as 900 to 1000. This most excellent method was not only highly satisfactory to its inventor Gay Lussac, but proved to be the same to every one who became acquainted with it. It was very soon adopted by every one who appreciated correctness, and it soon superseded the method by cupellation. Gay Lussac wrote a memoir, in which he gave a description

of his method as it is yet employed, under the title, "Instruction sur l'Essai des Matières d'Argent par la voie Humide; Paris: 1832;" which memoir was translated into German by Liebig, under the title, "Vollständiger Unterricht über das Verfahren, Silber auf nassem Wege zu Probiren; Braunschweig: 1833." It is a most extraordinary proof of the perfectness of this method, which is called the assay by the moist way, that after so many years it has not been changed. It still remains as a monument erected by the practical genius of Gay Lussac.

It may give rise to some astonishment that this method has not been repeatedly subjected to scientific researches, and the results published, were it only for the reason that this method, after having been invented, has come into general practice. In the first place, it must be stated, that the science of Chemistry has been greatly advanced, and although, on the other hand, Gay Lussac himself has published his method, perfectly fit for general use, a great many scientific points relating to it have neither been explained nor touched by him. No doubt the skilful men who in their practice have nearly daily to do with the so-called method by the moist way, have made a valuable collection of many points relating to this method, but none of these has been published; in every manual the general foundations of the practice of this method have been given, but nothing more. Notwithstanding this, it is in this, as in so many other parts of science, there will always be found something valuable when a thoroughly deep investigation is made, and the facts viewed under a somewhat changed light.

I therefore hope, that no one will charge me with temerity, when I, when other more capable men in the use of the method by the moist way keep silent, undertake to investigate that method chemically, and to explain the particulars to be observed in it, and to try according to the best of my capability to review this method by the moist way in the light of the modern science of Chemistry.

I am aware that I am hardly competent to treat the subject according to its value, all be it that my honoured friend, Mr. Van Riemsdyk,² has shown me his method of operating. Part of the experiments mentioned hereafter have been done by me, with the assistance of Mr. Van Vlaanderen.

Although I do not intend to make remarks, neither to give explanation, about all things relating to the method, I have thought it expedient to review it both in a practical and scientific point of view.

As far as the practical application is concerned, I wish to give all that has appeared to me necessary to increased exactness, as well as facility, I have altered the apparatus according to my own experience, and I will

¹ Translated from the original Dutch by A. Adriaani, Math. Mag. Phil. Nat. Medicus Doctor, Fellow of the Royal Netherlands Institution of Engineers, &c., &c.

² A. W. G. Van Riemsdyk, Esq., is a Member of the Supreme Council of the Mint of the Kingdom of the Netherlands,—by the bye, one of the most extensive mint works of Europe, established at Utrecht.

give the reasons for the changes made. I shall be able to prove that my apparatus give correct results, and their simplicity is such that they may be readily constructed from the materials found in every well-arranged chemical laboratory.

The only instrument which must be procured from elsewhere is a balance, and it is just this instrument which is an impediment to reaching absolute accuracy, even when one is in the possession of a balance which is sensible to the one-twentieth part of a milligramme. As far as the method itself is concerned, it is able without any difficulty to estimate the silver to one-twentieth of a milligramme for one gramme of silver.

I divide my research into two parts,—*The Chemical Investigation*, and *The Practical Method*,—and may state at once that by what is called normal solution of common salt, I mean such a solution of that salt in water, that 100 cubic centimetres of it contain at a temperature of 15° C., 0.54141 gramme salt, a quantity just sufficient to precipitate a solution of 1 gramme of pure silver dissolved in nitric acid. By decimal salt solution I mean one part of the same liquid diluted with nine parts of water. I must also mention that I understand by decimal silver solution a solution of 1 gramme of perfectly pure silver in nitric acid diluted with water to 1 litre, also at a temperature of 15°.

Decimal solution of salt and decimal solution of silver have been applied to the same volume, both being of equal strength. 1 cubic centimetre decimal solution of salt precipitates 1 milligramme of silver, and 1 milligramme of pure silver is contained in 1 cubic centimetre of the decimal silver solution.

I. The Chemical Investigation.

Solid Substances in Liquids.—If we wish to become acquainted with the method by which the quantity of silver is estimated, when a solution of silver in nitric acid is precipitated by a solution of common salt in water, it is necessary, in the first place, that we should make a thorough investigation of that which goes on when dissimilar substances are present together in a liquid.

It is easy to say from chloride of sodium there is formed chloride of silver, while at the same moment nitrate of soda is reproduced from nitrate of silver. The chloride of silver being insoluble is precipitated, while nitrate of soda remains in solution in the liquid. It is scarcely necessary to prove that things are not going on in this case in such a simple way; or to say that such though it is the chief action which takes place, is not the only one which is called into play here.

Beside the action of the real chemical affinity, there are in the action which solids and liquids mutually produce, consecutive actions of the so-called physical influences, which here, as in many other instances, play so prominent a part, that even Berthollet felt inclined to give them a first place while engaged in publishing his celebrated work, "*Essai de Statique Chimique*, 1803."

These so-called physical influences are certainly to be taken notice of, even although we know them only imperfectly, and although they are neither well classified nor expressed in laws.

Should we take them for these reasons, as if they were not existing, or should we cast them aside as if they had only a minute influence?

Even he who only makes use of chemistry as a branch of applied science is driven by the necessity of the greatest possible correctness to value every influence,

though he does not care about the name, whether it be chemical or physical; in the same way he who is a scientific chemist by profession makes use of many parts of the chemistry applied to arts in order to obtain more information, for in applied science an important fact is often hidden from which pure science can derive profit; profit, however, not so much in the sense of making money as in the sense for the promotion of true genuine knowledge.

If a more correct idea be formed of what happens when a solution of silver is precipitated by a solution of common salt, then it will be seen that it gives rise to many peculiarities worthy to be studied more closely; and, even although they cannot yet be explained in all parts, they merit a place along with that of the peculiarities of many other substances in action. It may be, therefore, not altogether unfit to begin our investigation by bringing these under a general point of view, or to show at least the relation which exists between them, and in doing so we are sure to obtain more space for the right representation of the phenomena presented by the precipitation of a silver solution by a solution of common salt. In the first place, then, I will take under consideration the position in which solids are placed when in solution in liquids, dissimilar solid substances of course, which, generally speaking, must be considered to exercise a chemical action upon each other; which, however, are either totally or partly prevented from satisfying their chemical action by the interference of so-called physical influence.

There exist a great many instances in which it can be proved, that the fact of a substance *a*, being in solution, can exercise a great influence upon the mutual affinities of two others, *b* and *c*, which are held in solution by the very same liquid. Vauquelin (*Ann. de Chimie*, tome xii.) gives a very simple instance of such an occurrence. He prepares a saturated solution of a salt *a* in water, and afterwards dissolves in the same solution a salt *b*, after which a fresh quantity of the salt *a* can be dissolved by the previously saturated solution of it. The degree of solubility of the salt *a* first depended upon the action which took place between *a* and water; but by the presence of *b*, *a* is again disposed to enter into the solution to a greater extent than before.

It is supposed in this case that the salts do not act chemically upon each other; hence it follows that two salts which have not a chemical action upon each other, if in solution together in the same liquid, have the power of increasing their mutual solubility, and *a*, as well as *b*, endeavour to do so. Like as there is no limit for the physical division of solids, so there is no solid also which may be called perfectly insoluble, provided it admits of very great divisibility. What we are accustomed to call insoluble in chemistry is not really so. The solubility of sulphate of baryta in water, of oxide of tin (stannic acid), in dilute nitric acid, of carbonate of lime in water free from carbonic acid, can be readily proved. Since water possesses the property of keeping in suspension, or even in solution, a great many minutely-divided substances, and since, according to the instance just mentioned, as given by Vauquelin, the solubility of a soluble salt can be greatly increased by the solution of another quite different salt in the same liquid, then the case may often occur that a so-called insoluble substance in water is rendered perceptibly soluble by the presence of another soluble substance, or in case it would have been otherwise precipitated, can be kept in solution. In such cases the chemical affinity gets modified simply by solution, for the substance which we are acquainted with

as being insoluble remains in solution, and presents itself to us in a quite different aspect from that by which it is ordinarily characterised.

Among the instances of the so-called physical influence upon chemical affinity, I may bring here under notice the mutual action of sulphate of soda and chloride of magnesium: both being dissolved in water it is quite dependent upon the temperature in what form they crystallise. In the cold, sulphate of soda crystallises out, chloride of magnesium remaining in solution; at a higher temperature sulphate of magnesium and common salt are formed, and at the boiling point, again, another change takes place,—separation, viz., of a double salt of sulphate of magnesium and sulphate of soda. It is in this way that Balard obtains sulphate of soda from the brine which yielded common salt, merely by means of cooling. (*Regnault, Cours Élément. de Chimie*, p. 577.)

(To be continued.)

*On the Estimation of Tin by Standard Solutions,
by M. SCHUBERT-KESTNER, of Thann.*

THE estimation of tin by standard solutions depends on the transformation of protochloride of tin into bichloride. MM. Streng and Mohr¹ have employed bichromate of potassa, solution of iodine, and permanganate of potassa as oxidising agents. The result of M. Mohr's experiments is that whichever of these bodies is employed the proportion found varies with the quantities of water used, that upon taking a certain quantity of chemically pure tin, the trials made with permanganate of potassa, or any other oxydiser, will not give the weight of tin employed, but always a less weight, a circumstance which has induced him to admit for these estimates an empirical atomic weight for tin (65), differing greatly from the actual weight (59). Even this number is allowable only when operating constantly with quantities of water equal to those which served to determine it.

M. Lenssen² estimates tin by the iodine solution, but he operates in a liquid containing double tartrate of potassa and soda, and bicarbonate of soda in excess. The results M. Lenssen obtained by this method are satisfactory, by using the atomic weight of tin generally adopted (59). We shall see further on why M. Lenssen's results agree.

M. Stromeyer³ having recently occupied himself with the same subject, has succeeded in solving the difficulty. The solution of protochloride of tin is carefully introduced into an excess of sesquichloride of iron. The salt of iron becomes reduced to a minimum according to the following equation:—



It is then estimated by permanganate, as if it were a salt of protoxide of iron. The results M. Stromeyer obtains in this way are very exact. The author adds that such a method of estimating is applicable only in the absence of copper or iron, as these two metals decompose permanganate of potassa, as well as the tin; but it may be of great use in the estimation of commercial salts of tin.

I have endeavoured to determine the causes of the variations observed when oxidising a solution of protochloride of tin, proximately by permanganate of potassa,

and whether, as M. Mohr supposes, these anomalies arise only from the presence of oxygen dissolved in the water. To make these experiments agree better, the solution of protochloride of tin mixed with the proper quantity of water has been instantaneously oxidised by permanganate, so as not to take time into consideration. The irregularities occurring during this operation are shown in the following table:—

c. c.	of Sn Cl ₃ , without	water, required	Permanganate.
2	and 10 c. c. water	"	34'5
2	" " 20	"	34'5
2	" " 50	"	34'5
2	" " 100	"	34'0
2	" " 200	"	33'0
2	" " 300	"	33'0
2	" " 400	"	31'0
2	" " 500	"	31'0
2	" " 600	"	29'0
2	" " 700	"	30'0
2	" " 800	"	28'5
2	" " 900	"	28'0
2	" " 1,000	"	28'0
2	" " 1,200	"	25'0
2	" " 1,400	"	23'0
2	" " 1,600	"	22'5
2	" " 2,000	"	17'0
2	" " 3,000	"	8'0
2	" " 4,000	"	10'0
2	" " 5,000	"	17'0
2	" " 6,000	"	18'0
(A) 2	" " 8,000	"	24'0
2	" " 10,000	"	23'0
2	" " 14,000	"	25'0
2	" " 24,000	"	32'5
(B) 2	" " 24,000	"	32'0

Leaving A and B to rest for ten minutes,—

(A) has required 14'0 of permanganate.
(B) " 25'0 "

The result of these experiments is that the more the solutions are diluted, the less permanganate is necessary until the proportion becomes about 10,000 parts of water to one of tin (100 cubic centimètres of the oxidising liquid corresponding to 1 gramme of tin). From this point the quantities of permanganate necessary are again augmented, and when the dilution is brought to $\frac{1}{24000}$ th as much permanganate is needed as when it was only at $\frac{1}{2000}$ th. The oxygen of the water then seems to have very little action on concentrated solutions, while on diluted solutions its action is very marked. At a certain degree of dilution, however, these phenomena are reversed, and instead of continuing to augment, the sensitiveness of the liquid for oxygen diminishes more and more, becoming gradually reduced to what it was in the concentrated solutions. In the preceding experiments no account has been taken of time; the oxidation always took place immediately after the protochloride was mixed with water. By allowing the very diluted protochloride to remain several minutes before adding the permanganate, a much greater absorption of the oxygen of water takes place, as shown by the Experiments A and B.

If the oxygen of water is the cause of these variations, they ought not to take place when boiled water freed from air is used; and in fact they do not then take place. Water was boiled, and the temperature then lowered to 80°, the protochloride was then introduced and the estimation proceeded with. The following results show that in operating thus the standard does not vary, whatever quantities of water are used.

¹ Mohr: *Traité d'Analyses*, pp. 297 and 349.

² *Annalen der Chemie und Pharmacie*, vol. cxlv., p. 114.

³ *Annalen der Chemie und Pharmacie*, vol. cxvii., p. 261.

Protochloride employed.	Quantities of Water.	Standards found.	
		With Cold Water.	With Boiled Water.
2	100	26	26
2	200	23	26
2	300	22	26
2	400	21	26
2	800	19	26
2	900	17	26
2	1,000	16	26
2	1,200	13.5	26

For studying the action of free oxygen on diluted and concentrated solutions of tin, I passed a current of this gas into various solutions of protochloride of tin. For this purpose two flasks, communicating by a tube, are filled, one with a solution of 2 cubic centimetres of protochloride of tin in 100 cubic centimetres of water, and another with the same quantity of protochloride in 1000 cubic centimetres of water. The oxygen current, forced to traverse the contents of the first flask before arriving at the contents of the second, during an hour, the liquids in the two flasks filtered and tested by permanganate of potass, yielded the following results:—

	c. c.	c. c.
1st Flask, containing 2 Sn Cl in 100 water, required 25.5 permanganate.		
2nd Flask	1000	9.2
2 c. c. Sn Cl, in 1000 of water, without oxygen current, 16.0		"

The first flask contained the strongest solution, and received the oxygen first, and absorbed only an amount of oxygen represented by 0.5 of permanganate; while the metal solution of the second flask absorbed a quantity represented by 16 — 9.2 = 6.8 of permanganate. The same experiment repeated by employing only 50 cubic centimetres of water in the first flask, and by continuing the oxygen current for two hours and a-half gave the following numbers:—

1st Flask, containing	50 c. c. of water,	26 permanganate.
2nd Flask,	" " " " 1000	" " " " 3.2

In this case the concentrated solution of protochloride of tin did not absorb oxygen, while the diluted solution absorbed a quantity equivalent to 16 — 3.2 = 12.8 of permanganate, that is to say, nearly half the oxygen necessary to oxidise the protochloride completely.

M. Mohr's experiments prove¹ that the oxygen of water has not the slightest action upon salts of protoxide of iron, whilst under the same conditions the protoxide itself oxidises immediately. M. Mohr has taken advantage of this property to estimate by means of permanganate the oxygen dissolved in water. It is interesting then to observe whether there exists between protochloride and protoxide of tin a difference of affinity for oxygen, analogous to that distinguishing the salts of iron of the oxide. This analogy, in fact, exists, only the reactions are inverse; that is to say, protoxide of tin manifests no affinity for free oxygen, while, as we have seen above, the protochloride combines more or less rapidly with this gas dissolved in water.

(To be continued.)

On Obtaining Thorina and Tantallic Acid in Crystals, by MM. A. E. NORDENSKJOELD and J. J. CHYDENIUS.¹

THE thorina employed was extracted from orangite by treating that mineral by hydrochloric acid, separating the silica, precipitating by ammonia, redissolving the precipitate in hydrochloric acid, neutralising by ammonia.

and precipitating the thorina by sulphate of potassa, as double sulphate of potassa and thorina. This salt washed with a solution of sulphate of potassa was dissolved in water, and the thorina precipitated by ammonia. The previously washed thorina was again dissolved in nitric acid; then, for the last time, precipitated by ammonia, washed, and calcined. It had a slight yellowish colour. Density 9.08.

In one experiment thorina was crystallised by exposure to the temperature of a porcelain furnace in platinum capsules, with borax, in the glass formed by which the crystals were found. In another experiment a like result was obtained, with boracic acid, by Ebelmen's method. Thorina formed small brown or white prismatic crystals, sometimes ending in a base, sometimes in a quadrangular pyramid, the faces of which are sometimes hollow like those of sea salt. The authors refer these forms to the type of a prism with square base. The angle of the octohedron face on the adjacent face of the prism would be 102° 30', which greatly resembles that of an octohedron of oxide of tin, rutile, and zircon. Thorina would then be isomorphous with these three oxides, and would then have the formula ThO₂, instead of ThO or Th₂O₃.

The density of crystallised thorina is 9.20. Tantallic acid extracted from tantalite (hjelmite) of Nya Kärarivet, and freed from tungstic acid and oxide of tin by fusion with a mixture of sulphur and carbonate of soda, was mixed with four times its weight of salt of phosphorus. After exposure to the heat of a porcelain furnace, and being treated by hydrochloric acid, it yielded a gelatinous mass on the addition of water.—*Poggendorff's Annalen.*

Notes on the Extent to which Mercury Volatilises along with the Vapour of Water at 100° C., by J. W. MALLET.

IN Berzelius' *Traité de Chimie* it is stated that Stromeyer drew attention to the fact of the evaporation of mercury in considerable quantity at 60° to 80° C. with the vapour of water, the more volatile substance carrying with it the less volatile, as in the case of a solution of boracic acid when heated.

This fact does not seem to have been very generally noticed by the compilers of chemical text-books in treating of the history of mercury, though it was always stated that the metal is capable of volatilising to a very slight extent, even when alone, at the common temperature of the atmosphere. Some doubt, too, would seem to have been thrown upon Stromeyer's observation by the experiments made, under peculiar conditions, by Fresenius, and reported by him in the appendix to his *Treatise on Quantitative Analysis*. It was found that 6.4402 grm. of mercury, covered with a considerable quantity of water, and heated to the boiling point of the latter for a quarter of an hour, lost but .0004 grm., while exposure to the air at summer heat for six days produced a further loss of .0005 grm.

I have lately observed the tolerably rapid evaporation of the metal with steam, produced not from a mass of water covering the quicksilver, but from a porous and highly absorptive clay which was dry to the touch, but gave off some eight or ten per cent. of water when heated to 100° C. A specimen of this soil had been in contact with mercury, and several grms. of the metal in small globules had become mixed up with the mass. It was placed in the common copper box with double sides which serves as a steam-bath, and exposed to the temp-

¹ *Traité d'Analyses par Liqueurs Titrées*, p. 260.

² *Poggendorff's Annalen der Physik und Chemie*, vol. cx., p. 642. 1860 No. 8.

ature of boiling water. In half-an-hour I was surprised to see that the little piece of glass tube through which the heated air and vapour from the inside of the steam-bath escapes was coated with a bright specular deposit of metallic mercury. This was brushed off, and the tube replaced, when again it became in a short time similarly coated. The steam bath was kept in constant use in drying this and other specimens of soil for twenty or thirty hours, and in this time between four and five grains of mercury was collected from the glass tube. Doubtless the condensation of the mercury vapour on the latter must have been far from complete—a large proportion of mercury probably escaped into the atmosphere. At any rate it seems clear that the metal can volatilise in very considerable amount when surrounded by vapour of water at $100^{\circ}\text{C}.$, and not at the same time pressed upon or affected by the cohesion of a mass of liquid water. Hence an obvious necessity for thoroughly effective condensation when mercury is to be determined in its compounds by ignition with soda-lime.—*American Journal of Science.*

On the Composition and Properties of Permanganic Acid, by M. H. ASCHOFF.¹

MR. PHIPSON'S assertions relative to the non-existence of permanganic acid, already refuted by M. Machuca, are further contradicted by those of M. Aschoff. Having occasion to occupy himself with these inquiries, M. Aschoff has entirely confirmed M. Mitscherlich's long-known views relative to the composition of permanganate of potash. He also proved that violet permanganate becomes changed to green manganate with disengagement of one atom of oxygen, and that the green solution again becomes violet by the action of carbonic acid with a deposit of peroxide of manganese, according to the equation $3(\text{K.O.MnO}_3) + 2\text{CO}_2 = \text{K.O.Mn}_2\text{O}_3 + \text{MnO}_2 + 2(\text{K.O.CO}_2).$

The author afterwards occupied himself with preparing anhydrous permanganic acid. He finds that this preparation can be made without danger, even when operating with 20 grammes of permanganate, by taking care to cool the sulphuric acid (1.845 density) with a refrigerating mixture, and by gradually introducing the permanganate, which must be free from all chlorinated combinations.

The dissolved permanganic acid colours the acid dark green, forming at the same time some oily drops, which fall to the bottom of the vessel, and which, after a time, or rather when the mixture is heated, disengage oxygen. Manganate of potash behaves in an analogous way. There is always a deposit of peroxide of manganese, the manganic acid immediately decomposing into peroxide and permanganic acid.

Permanganic acid forms a dark brown-red liquid, fluid at 20° , very unstable, attracting humidity and decomposing with disengagement of oxygen. It gives no trace of vapour in an exhausted receiver, and when heated to above 65° it detonates violently with flame, and is transformed into sesquioxide of manganese. Its composition answers exactly to the formula Mn_2O_5 .

When permanganates or manganates contain chlorides the phenomena are different, and the yellow vapours obtained by M. Dumas² are produced by adding sea salt

to a mixture of permanganate of potash and sulphuric acid.

M. Aschoff has not succeeded in condensing a sufficient quantity of these vapours for analysis; but he has collected and studied some small drops analogous to anhydrous permanganic acid, which are also isolated in this experiment, and the composition of which answers to the formula Mn_2ClO_6 . This body may be a new combination or a mixture of anhydrous permanganic acid and a chlorinated combination of manganese.

TECHNICAL CHEMISTRY.

On the Manufacture of Iron,¹ by E. RILEY, F.C.S., late of the Dowlais Iron Works.

(Continued from page 98.)

In rolling the flange, or American rail, a particular description of cold short iron was made for the flanges to stand the strain that there necessarily must be from the rails being rolled edgewise, and the flanges not travelling at the same speed (from the less periphery of the rolls) it was necessary that the flanged portion of the rail should slip; and partially on this account, and also on account of the flange becoming colder than the mass of the rail, it was difficult to obtain a perfect flange, unless a peculiar description of iron was used. This iron was made generally with forge cinders and Welsh ironstone, without any red ore. In the bridge rail, which was rolled flat, head up and down, the iron was only likely to rip by becoming cold, and was not submitted to so great a strain as with the flange rail. For common American rails the slabs for the heads were frequently not made more than three-quarters of an inch thick, and the rails were only single-heated.

The way in which a rail has been piled, and the thickness of the top slab for the head, may be easily determined by filing a section of the rail smooth, and acting on the surface with dilute mineral acid, which dissolves the iron and leaves the cinder in the welds or joints intact.

Rails being submitted to a different strain to other descriptions of iron, it was soon found that fibrous iron, such as is used for making cable bolt was not adapted for rails, as the iron was too soft, and strips or ribbons soon began to pare off the rails. Various means have been adopted to make iron crystalline and hard. As a rule, the more you roll iron the more fibrous it becomes, up to a certain point, and for this reason many engineers have the slabs for the heads of the rails hammered, so that they may only be submitted to the drawing action of the rolls once. Since the introduction of the oolitic iron ores it has been found that, by smelting them with a mixture of a little cinder and clay ironstone, that a peculiar description of cold short crystallised iron is produced, and this iron has been very largely used for the manufacture of the heads of rails. The peculiar description of this iron is due to the phosphorus which the oolitic ores contain. Phosphorus has the property, when alloyed with iron, of making it work soft and cold short.

Rails made with this iron have been very favourably reported on by engineers. The iron, although hard, is very weak, and the author's opinion was that hard iron might be made with equal advantage by the use of manganese, without, at the same time, reducing the strength, as you do when phosphorus is present.

Some foreign engineers have even gone to the length

¹ Monatsbericht der K. Preuss Akademie der Wissenschaften zu Berlin. Poggendorff's Annalen der Physik und Chemie, vol. cxi.

² Annales de Chimie et de Physique, Second Series, vol. xxvi. p. 81.

of making it a clause in their specifications for rails that they shall break when a given weight is let fall upon the rail resting on bearings at a specified distance apart.

Single-heated rails require about 12 cwt. of coal to the ton, on an average, and double-heated about 16 cwt.

The author, after apologising for the very brief way in which he had considered the various processes in the manufacture of iron, next proceeded to consider some of the recent improvements that had been suggested and carried out for the manufacture of iron and steel.

The most important, perhaps, was the Bessemer process, which, when first put forth by Mr. Bessemer, in 1856, caused quite a sensation in the trade. The author was, at that time, at the Dowlais Works, and made a series of experiments on the process, which were detailed. The results of the experiments were, that only from the very best descriptions of foundry iron could wrought iron be produced which had at all the character of ordinary bar iron, and even this was of a peculiar crystalline structure, and had the appearance of burnt iron. When the ordinary forge pig, used for making bars and rails, was operated on, nothing could be done with it at all. The iron, after blowing and casting, when submitted to the action of the squeezer, crumbled all to powder and dust; and it was only by using very great care that a piece was made into a bloom and passed through the rolls, the bar was as brittle as glass, and broke, when struck, in all directions, longitudinally and transversely. The same results as the above were obtained by experiments at various other works, and only when such pig as the best Blaenavon Foundry, foreign pig, or pig made from spathose iron ore was used, that any good results could be obtained.

The author investigated the cause of the failure of the process, and on analysing samples of the pig, and the iron after blowing, he found that the whole of the carbon and silicon had been separated absolutely, as not a trace could be found; whereas the sulphur was only slightly diminished in quantity, and the phosphorus, if anything, increased. From the above it was evident that the non-separation of the latter elements was the cause of the failure, in a great measure, with ordinary white forge pig.

Mr. Bessemer instituted a series of experiments on his process, and erected works at Sheffield to carry it out practically. He soon found that he had been in error in using some of the best description and highest-priced irons, as there were others in the market which were far cheaper and much more adapted for his process. On analysis it was found that the grey pig made in Cumberland and Lancashire from the red hematites was nearly free from sulphur and phosphorus, containing not more than .05 per cent. of the former, and .10 per cent. of the latter. This iron is very siliceous, but by blowing the whole of this is separated; by blowing this iron, and adding after the blowing some metallic manganese, or an alloy of iron and manganese, iron or steel of any quality could be made. The author, before considering the condition in which he considered the Bessemer iron to be, detailed more experiments he had made on wrought iron in a fused state.

The results of the experiments (which had been frequently repeated) were that if the very best fibrous cable bolt iron was melted in a clay crucible (so as to be kept away from carbon), a crystalline fused button was obtained, which had somewhat the appearance of antimony. This iron, when worked by a smith, was very redshort, and after welding it broke all to pieces as the heat came down, although at a low temperature it

worked tolerably well. It was, in fact, in the same condition as the Bessemer iron, and was in this state useless. On adding to the pieces of iron (from 1 to 1½ lb. were used in each pot), a very small amount of metallic manganese, from 1½ to 3 or 4 per cent. in various experiments, it was found that the lower quantity of manganese restored all the original properties to the iron, and that anything could be done with it, and it would work at all temperatures. The higher proportions of metallic manganese gave the iron a steely character, and in fact converted it into a low steel. Experiments were also detailed on the reduction of wrought iron in a fused state direct from the best clay ironstone (by leaving an excess of oxide of iron in the cinder); the iron thus produced was useless, as it would not work at a high temperature, but worked moderately well at a low heat.

The author considered that a small amount of carbon was essential to wrought iron, and by the process of melting, or the Bessemer process, this small amount of carbon was removed. By the introduction of metallic manganese, the carbon was taken by the iron, and the manganese would become oxidised, and unite with the silicon or other impurities in the iron. It was well known that pig iron, made from the spathose iron ore, not only contained a high per-centage of manganese, but also a higher per-centage of carbon than any other description of pig. This iron was generally white, the author never having seen it better than mottled, and he believed great difficulty would be experienced in getting it grey. Manganese certainly has the property of fixing carbon in iron in some way not very well understood, and when this metal is alloyed with iron he considered that the manganese would be completely separated before the carbon was touched, and in its separation the oxide of manganese, being an excellent flux, it would have a tendency to separate the impurities in the iron.

The reduction of manganese from its ores, and the difficulties attending it, were mentioned in illustration of the properties of this metal, and the author considered that the application and use of manganese in the manufacture of iron was most important.

The author then referred to drawings of the vessel Mr. Bessemer used for his process. One of the great difficulties was in getting the tuyeres to stand, as the fluxing action of the oxide of iron and the temperature in the blowing vessel was very great. In several instances a whole fire-brick had been fluxed away in twenty minutes. By using powdered sandstone, known as "ganister," the fluxing action was not so great, and a further improvement had been made by introducing the tuyere from the top, and making it altogether independent of the vessel, so that it could at any time be withdrawn and replaced by a new one.

Various other processes for the manufacture of steel were commented on, amongst others Uchatius', Mushet's, and puddled steel. The author considered that puddled steel was liable to be wanting in uniformity.

Much had been written on the composition of steel, and various theories had been propounded as to its composition, and the elements requisite for the production of the best varieties of steel. M. Frémy, had published a very extensive series of experiments, and he was of opinion that iron in its three states—cast iron, steel, and wrought iron—is due not only to the different proportions of carbon they contain, but also to the presence of nitrogen. M. Mushet, again, has started a theory that titanium is essential in the manufacture of steel.

It was a well-known fact that cyanides have the property of converting iron into steel, and after the very

elaborate investigation of M. Frémy, in which he showed that nitrogen assisted very materially the formation of steel, the author was of opinion that nitrogen might act in a somewhat similar manner to manganese—viz., as a carrier of carbon, but he was hardly disposed to think that nitrogen was a constituent part of steel. The formation of steel with pure carbon and iron was discussed, and it was thought that further investigation was necessary to prove if nitrogen was or was not a constituent part of the best varieties of steel.

With regard to titanium, this metal had a great affinity for nitrogen and carbon; and if nitrogen is requisite in the manufacture of steel, it is possible it might act as a carrier of nitrogen and carbon. A large sample of titanium (in a pure state), obtained from the hearth of an old blast-furnace, was exhibited.

The author concluded by pointing out the difficulties that there were in accounting for the different varieties of iron which were due, in some cases, to physical, and in others to chemical differences, in their constitution, and at times, perhaps, both combined. It was only by observation and carefully conducted experiments that the true causes could be determined. He was ready to admit that our knowledge was limited on the subject, and he must plead this as an apology for the imperfect way in which he had treated it.

On White Gunpowder, by F. HUDSON, Esq.

HAVING lately prepared different samples of white gunpowder (according to the receipt of Dr. J. J. Pohl, given in the CHEMICAL NEWS, July 6) for some military engineering experiments, I have tried the process of separately grinding the materials, viz. chlorate of potash, ferrocyanide of potassium, and cane sugar, and then mixing them; also grinding them together with a little water added, and then dried at a temperature of about 150°. I find that those samples which were prepared moist and then dried are more easily exploded than those prepared by the dry process. In fact, one sample exploded in an open porcelain dish by simple friction with a spatula with which one of my assistants was crushing some of the larger pieces. Through the explosion he was laid up for several weeks and nearly lost his eyesight. No samples prepared dry are as explosive as those prepared moist, the addition of water causing a more perfect mixing of the particles of its chemical constituents than can be effected by the dry grinding process. This accounts for the greater danger attending the use of white gunpowder prepared in the moist way.

A cannon loaded with the white powder goes off on the application of a few drops of sulphuric acid (equally as well as with a light applied) to its touch-hole.

This property of the gunpowder may possibly be applied to some advantage in the construction and preparation of bomb shells for long ranges. The shells would not explode (if filled with the white powder and containing a glass vessel with sulphuric acid) until they struck the object. No useless explosion of the shell could take place in the air, as is too often the case with the ordinary fusée shell.

Its expansive or explosive force is also twice that of common gunpowder. In all experiments performed with this white gunpowder care must be taken not to compress it too violently; otherwise accidents may frequently occur. A blow with a hammer upon stone with some of the powder upon it explodes all samples that I have prepared.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Two Lectures on Musical Acoustics, and on the Physiological and Psychological Causes of Musical Harmony and Discord, by Professor HERMANN L. F. HELMHOLTZ, of Heidelberg.

LECTURE II.

I HAVE had the honour of bringing before you, in my first Lecture, the theory of the composition of waves of sound; at first, the composition of such waves as were of independent origin, and where the composition was a real composition of different systems of waves. I have also explained to you that a wave of any arbitrary form can be considered as a composition of a number of waves of simple form—of waves in which the motion of every particle corresponded to the motion of a pendulum, and calling [these vibrations simple vibrations, we may call such waves simple waves. Therefore, a wave of any other form can be considered as a composition of a number of simple waves. It is shown by the French mathematician that this is always possible, and that any form of wave can be composed by simple waves into one, so that the intensity and the phase of every small wave is known, if the form of a complex wave is at first given. Now, this composition of a complex vibration by simple vibrations is only an imaginary composition. We may consider it in the same way as we may consider every quantity, every number, as a sum of parts. Further, we find that this composition, which is a sort of composition of simple waves, is not only an imaginary composition, but that the human ear makes the same analysis of complex waves as if they were really composed by simple waves. My last experiment was on the monochord, where I tried to show you that, in the sound of the string, if I move the whole string, there, the simple sounds are contained which must be composed in order to get the whole motion of the string.

Now, you will usually find it very difficult to hear the harmonic sounds of the string—to hear the sounds corresponding to the simple vibrations of the string which are kept together in the whole motions. These harmonic tones have been known for a long time, but they have been commonly considered as rare and accidental phenomena of small intensity and of small importance. Philosophers believed they were of small intensity, because it was difficult to hear these tones. But this difficulty is of a very peculiar kind. We should think that nothing could be easier than to feel the sensations of our nerves; but that is indeed not so simple and not so easy as it appears. We are exercised and educated for using our senses, and even in the most common perceptions by our senses there is more exercise than we commonly are inclined to guess. We are exercised to judge by the sensations we get of the real nature and form and quality of the external objects which surround us, and we accomplish this business with the greatest accuracy—with really astonishing accuracy. But we are also in the same way exercised to abstract from all those parts of our sensations, which are necessary for the comprehension of external objects. Every one who has studied the subject of visual phenomena and other sorts of internal sensations, knows how very difficult it is to be attentive to the sensations which are excited by the interior irritations of our nerves. There are a great number of phenomena in the eyes. For instance, there are little filaments which swim about in the vitreous humour, which are only seen if the eye is diseased. And you will remember the celebrated experiment by which the French philosopher excited the curiosity of King James I. of England and his courtiers. He found out that there is a blind spot in every eye, and

that there is a part of the object of vision which is not seen at all. I shut my right eye and look straight forward with my left eye, and bring this piece of red sealing-wax in front of it. If I bring it here I see it, if I bring it there I see it, and so on, but there is a place where it is not seen. Now I suppose that everyone of you who has not made this experiment but whose attention has been drawn to it by others, will hardly believe that—you will doubt it. But such a spot is in every eye. Take another phenomenon of the same kind. Mr. Wheatstone has shown that the correct perception of the dimensions of every object before our eyes depends upon the comparison of the two different images given by our two eyes. You all know that this can be quite clearly proved by the stereoscope. Now, if you look at any solid object before you, I doubt that you will be able to perceive that the condition of the form of the object depends upon the union of the two perspective images which are given by both your eyes. Now, the same difficulty exists for the harmonic tones. If we hear the sound of a string, we always hear the fundamental tone accompanied by a number of harmonics. This whole sensation composed of the fundamental and the harmonics we consider as the sensual sign for the sound of a string. We perceive by this complicated sound that the string is tuned. If there is another combination of harmonic sounds, we guess that there is another instrument sounding. So that we are exercised to distinguish this complicated sensation; but we are not at all exercised to distinguish and perceive the simple elements which are contained in such a complicated sensation: therefore this difficulty of perceiving the harmonic tones is not caused by the small intensity or rareness of harmonic tones; it is not the difficulty of sensation; it is only the difficulty of perception of these tones. It was, therefore, quite wrong to suppose that these harmonic tones are rare or accidental phenomena. Although it is very difficult to hear them, and it requires much exercise, and a peculiar help that we may succeed in directing the attention to these tones. I must say I have exercised myself much in different ways to hear the harmonic tones, but I was always doubtful as to their intensity; and I could not succeed in comparing by the ear alone their intensity in different instruments. The enquiry of the unassisted ear is of great importance in these researches, because it is necessary to show that harmonic sounds are heard even by the unassisted ear. Now, I strove to find a method for hearing the harmonic sounds more easily, and for becoming sure that there was in the tone of any instrument such harmonic sounds, or that they did not exist; and at length I found a method which is quite convenient and simple. I use for that end these glass spheres; they are tuned to different pitches. Here you have a sphere which is tuned to B flat. If I bring the tuning fork before the mouth of the sphere, the air resounds and gives the sound of the tuning fork, and always with great intensity. If the note which corresponds to the pitch of this sphere is produced in the external air, then the interior air of the sphere is violently shaken and vibrated. If I place this sphere to my ear, then I hear the note of the sphere. Every tone when this note is produced in the outer air, be it a fundamental, or be it a harmonic sound, or as a part of a great mass of different sounds. So that I hear, then, this sound corresponding to the pitch of the sphere with great intensity, and all the other sounds which are produced in the same time come to my ear with much less intensity; so that it is very easy to find out if there exist any harmonic tones of a given pitch in any mass of sounds which are produced together in the outer ear. Now, by this means it was very easy to find the harmonic sounds of all musical instruments. I find that there are very few instruments entirely free from harmonic sounds. The best method for getting really the simple sounds is this which I have applied—to bring the tuning-fork before the sounding cave filled with air, which is tuned to the

same pitch. Therefore this kind of sound is really simple sound. We can produce the same kind of sound also from the stopped organ pipes. They gave simple sounds accompanied with a little noise of air. This is a sound of the same kind. The sound of the pipes is not quite so pure as the tuning fork, but it is nearly devoid of all harmonics. Now, the sounds of all other musical instruments contain harmonic sounds of more or less intensity. The most similar to the stopped pipes of the organ are the open pipes; you hear these a little more clearly. The sound of the open pipes, if they have a great section, contains only the first and second harmonics; and if they have smaller sections then the sound is more sharp and it contains more harmonics—to the fifth or sixth. You hear that these sounds are sharper; they are so because they contain more harmonics. Now, these harmonics always give consonant intervals when connected with each other: but the higher harmonics give dissonant intervals,—they are nearer; and therefore such instruments which have very high harmonic sounds sound sharp, and have something disagreeable in their quality of sound. If you will compare the sound of the stopped pipe, or the sound of the open pipe, and the sound of the reed pipe, of the same pitch, you may also perceive that the simple sound appears to be lower than the complex sound of the reed pipes. The fundamental sounds of those two pipes are really the same, but the reed pipe contains high harmonic sounds and therefore the one sound appears to be higher than the other sound, and you hear this difference very plainly. Now, it is not possible, by these means, to ascertain the intensity of harmonic sounds. This can be done for some instruments by mathematical theory. The mathematical theory of the motion of strings is able to give for different methods of exciting the string, the amplitude of the harmonic sounds. For the piano, if its common point where the string is met by the hammer, is $\frac{1}{4}$ th of its length, you have the fundamental sound and the different harmonics. The 8th harmonic is wanting, and the higher harmonics are very feeble, if the hammers are covered with leather, as it is common. Here you have the same for the guitar, if the same point of the string is excited you see the 8th is also wanting. Here also you have the harmonic sounds for the violin, the first sounds are of less intensity than those of the piano or the guitar, but the higher harmonic sounds are not wanting; and therefore the sound of the violin is sharper than the sound of the piano and guitar. A very striking and interesting example of changing the quality of the sound is given by the reed pipes, if they are connected with different tubes filled with air. The human throat contains such a reed pipe in the nerves, and the cavity of the mouth represents the tube which is connected with the reed pipe. Now the cavity of the mouth can be tuned by changing its volume and its opening to different pitches. And we find by enquiring into the quality of the sound with these spheres, that the harmonic sounds of the human voice are different for different vowels, and that different vowels are produced by giving to the cavity of the mouth a different pitch. We can do the same by the reed pipe. The theory of producing vowels in this way was first given by Mr. Wheatstone after the experiments of Mr. Wills. Here I have a common reed pipe of a very sharp sound, it is like A spoken through the nose. Now the pitch of this reed pipe is B, of two feet length, given by organ pipes. If I put upon this a glass sphere, which is tuned to the same pitch, you have a sound like oo. If I put upon it a sphere tuned an octave higher, you have o. If I put upon it a sphere two octaves higher than the first one, you have au, and so on. In this way different vowels can be produced, and are produced in the same way by the mouth. These harmonic sounds which are near to the sound of that sphere with the sounding cavity, are always produced with greater strength. That the cavity of the mouth is tuned to different pitches, can be immediately

ascertained by tuning forks. You saw that the sphere resounds for this tuning fork. If I put my mouth into the position for giving the vowel *o*, you hear the resonance of the tuning-fork. The pitch of the mouth is the deepest for *oo*, and then you can get *o*, *aw*, *ā*, *ah*, for the higher vowels, and so it goes on from *ah*, *a*, *e*, *ee*. This other tuning-fork is $\frac{1}{2}$ th higher than the first, and resounds for *ah*. If I put my mouth into the position of *o* or *oo* you hear nothing. In this way, therefore, you can ascertain the pitch of the cavity of the mouth; and it is stated by the theory of Mr. Wheatstone that those harmonics which are near to this pitch, which is constant for every vowel, and is the same for female and male voices and voices of children,—the harmonic sounds near to this pitch are of greater intensity, and the others of less intensity; and by this composition the vowels are distinguished. Now, it was a question of great importance for the physiologist to ascertain whether such sounds can be also composed by their harmonics not produced in the common way. Because, by such experiments it was possible to ascertain if only the existence and intensity of harmonic sounds is sufficient for producing a given quality of sound, if there are also other differences, if there are other influences necessary for getting the quality of the vowel sound. For this end I have composed an apparatus consisting of some tuning-forks. Every tuning-fork is connected with a sounding cavity, a green cylinder, and put between two branches of an electric magnet. The electric magnet is magnetised by an interrupted galvanic current. The galvanic current is produced by a battery; the interruption is caused by the vibrations of another tuning-fork, the ends of which are connected with the platinum wires, and dipped into mercury if the tuning-fork is swinging, so that by the vibration of this fork the current is interrupted, and we have here 240 interruptions of the current in a second of time. The first tuning-fork in the sounding cavity makes 240 vibrations in a second; the second makes twice as much, the third three times as much, and so on. So that the other tuning-forks give the harmonic tones belonging to this fundamental sound. If I let go the interrupted current through the spirals of the electromagnet, then the tuning forks are brought into vibration because at every magnetization of the iron they are attracted and then go back and then are again attracted, and so on, so that they go in continuous vibration. As long as the ends of these resounding cavities are closed, you will not hear the sound of the tuning-forks, or will hear only a very small intensity. If I open the ends here by a little key, then you will hear the sound, and in this way I can alter the intensity of the sound by opening completely or incompletely the mouth of the cavities, and so I can compose the fundamental sound with any given number of harmonic sounds. This is the fundamental sound; the higher sounds are very feeble; the sound becomes similar to *o*, and, therefore, I make *o oo*. The higher tuning-forks give *a* or *ah*, or the German pronunciation *a*, *o*, *oo*.

In this way, therefore, we can compose artificially the quality of these sounds. You will hear it better perhaps after the end of the Lecture, if you come nearer to the instruments. The sound is a little feeble for a great room, and I cannot strengthen the intensity of the current, because then the sparks become too intense, and volatilise the mercury. So, therefore, it is possible to compose in this way the different qualities of sounds, and to make them similar to the sound of the different musical instruments. The quality of the sound of the vowels is a very favourable example for that, because the vowels have only a musical sound, and other noises which commonly accompany the sounds of other instruments, as the sound of the rushing of air, or the rubbing with the bow on the string of the violin, and so on, are wanting in the vowels, and the vowels are different only by the musical part of their sound, and of course we can only imitate the musical part

of the sound by this tuning-fork. The fundamental question to decide by these means, if only the existence and intensity of the harmonics is wanted for giving equality of the sound; if perhaps also the difference of phase is different, and has influence upon the equality of sound. For showing the sense of this difference I have here the wave-machine of Mr. Wheatstone which I suppose you have often seen. You see here [pointing] a wave of long vibration, and there, one of shorter vibration, where two waves come to every one wave of the higher. The one corresponds to the fundamental sound, and the other to the octave. Then the upper curve shows the combined vibration. You see at present the hills of the inferior wave correspond to the hills and the valleys of the superior. There you will see that the phases all agree; both are coinciding in these two and they give here [pointing to the upper row] these greater heights. We can alter the figure and get different curvatures. Now, by these experiments I found out that only the intensity of the harmonics is wanted, so that all these curves we get above would correspond to the same quality of sound, and that gives no difference of quality of sound, so that only the existence of these is what we want. Therefore the quality of the musical part of the sounds is only determined by the existence of the harmonics. So much about the quality of the sound.

I come now to the influence of the harmonic sounds upon the consonants and dissonants. For finding the cause of the dissonance of two musical sounds we must consider another phenomena, the phenomena of interference of sound. Two sounds can be combined in that way,—two sounds of equal pitch can be combined in this way, that every sound gives an impulse to the air in the same direction, then the intensity of the two sounds is strengthened, they help each other, the vibration of the air becomes greater, or two sounds of the same pitch can be combined together, so that if the one gives an impulse the other goes back, and the impulses are given alternately. Here the motions of the air corresponding to these two sounds are contrary—opposite—and destroy each other. We may combine on the wave machine of Mr. Wheatstone two sounds of the same pitch. Here we have the combined wave. The hills correspond to hills, and the valleys to valleys, therefore in the combined wave you have higher hills and deeper valleys. If we alter the phase of one of these so that hill corresponds to valley, and valley to hill, then you see that the composed curve becomes a straight line. There the hills fill out the valleys of the other line which corresponds to the same point. The two sounds destroy each other, their impulses upon the air are of opposite direction and they destroy each other; so that the air which is shaken by their single sound becomes quiet if both the sounds are combined. Now if the air is quiet we hear no sound. We can produce such a combination by the sirene you saw only lately; because I can here alter the position of the holes of the upper box. I can put this box in such a direction that the holes are opened at the same time and the impulses of both boxes come to the ear in the same time or I can put them in that position that the impulses come alternately, then the sound is destroyed. It is not completely destroyed, because the motion of the air produced by the sirene is not quite simple, and therefore the form of the hills does not correspond exactly to the form of the valleys, and there remains a small intensity of sound. The fundamental sounds destroy each other; but the first harmonic sounds remain; therefore, though the sound becomes more feeble it does not quite cease, and goes up in its higher octave. We have, you hear, alternately greater and smaller intensity, coincidence and interference. Now, if I turn round the axis of the upper box, then I come at first to two points of coincidence and interference and again a point of coincidence and interference, and so we have alternately coincidence and interference. We have, then, a phenomenon which is called beating of the sound.

Now, you know that, if I turn the axis one way, the pitch is heightened, and the other way it is lowered. Therefore, you see that, by giving to the upper box such a motion that the pitch of the sound is a little altered, then we get alternately coincidence and interference, this strengthening or lessening of the sound. So that, therefore, by altering the pitch of one of these sounds by a small quantity, we get this phenomena of beatings. The phenomena of beatings can be produced on all other musical instruments. If we connect two sounding instruments which have nearly the same pitch, but not quite completely, the number of the beatings is always corresponding to the difference of the number of the vibrations. The beatings are very complete. If we have simple sounds, as, for example, by two stopped pipes—when they are of the same pitch you hear no beating, but if I alter the pitch of one, the beatings are heard very distinctly. Here the beatings are separated merely by complete intermittence of the sound. If I take two other pipes which have the harmonic sounds, then the beatings are not so complete, because, during the time where the fundamental sound ceases, the first harmonic sound is strengthened. I take two of these metal pipes, and at present you have an unison, but by altering the pitch of one, the beatings are produced. In the same way the beatings can be produced by other musical instruments. Consider what takes place in the ear during this time. If the tympanum of the ear and the air in the ear is not vibrating, if there is repose, then we still have no sensation at all—no sound. The phenomena of beating, as long as we consider only the motion of the external air, is no exception from the common rule of undisturbed composition of the different sounds. You see that this machine of Mr. Wheatstone's, which possesses different vibrations, produces interference by adding, or by summing the vibrations together, after the common law. Therefore, this repose is produced by the common rule, that vibrations are added without disturbance. But it is another thing with the sensations of the ear; the sensation of two sounds have no two different phases. If we add one sensation to a second tone, we cannot expect to get the same by this addition; we should always expect that we should have the double sensation, but that is not the case. In the sensation of the ear there really is a difference; the sensations in the ear are not always summed together by the common law, and we cannot consider the sensation of two systems of waves like the sum of the sensation of the ear. The ear is a resounding body. Perhaps there are, in the interior of the ear, little elastic bodies, which resound differently for every pitch. This drawing [pointing to a diagram] represents parts of the internal ear. It is like the shell of a snail, vaulted, hollow, just separated by a wall, which is one-half osseous, and the other membranous. The nerves represented by the white stripes come to the osseous part, and then to the membranaceous part; there we find little elastic arches, called commonly the organs of concha. They are little elastic arches, of a very small size, which are connected with the nervous pipes. These little elastic arches spring if the membrane is shaken by the vibration of the air; and if we suppose that these elastic arches are tuned every one for a different pitch, then it would follow that, by every external sound, only one, or very few of these elastic bodies are put into vibration, and that only one or some nervous fibres are excited, the others remaining quiet, so that all the sounds of different pitch are also felt by different nervous pipes. Now, if we take another sounding body, the air of this little flask, and if I bring two tuning-forks before the mouth, you will hear the beatings, which show that the air in the interior of the flask is at first vibrating and then not—alternately vibrating and not vibrating. [The Professor excited the tuning forks, and held them before the mouth of the flask, when the alternate vibrations were distinctly heard.] Here the vibrations of the air are excited by the tuning forks; in the same way the vibration of the little elastic

bodies in the air must be excited by the external sounds. If two sounds come together which give interference, then the elastic bodies must repose, and the acoustic nerve cannot be excited. In this way we may explain that we can distinguish by our sensations the sounds of different pitch—indeed the sounds of different pitch seem to be the sensations of different nerves. We may explain further, that we separate the sensation of a complex sound into its single harmonics. This would be a real explanation of this curious fact, that we separate such a sound, which is not separated by the manner in which it is produced, not separated into single parts, but really by the complex sound as the sound of a string. All those particles must be put into vibrations which correspond to the fundamental sound, or the harmonics of the external sound; and we can give in this way an explanation of this curious analysis of sounds. Now, we will consider the beatings of two similar sounds if the difference of the sound becomes greater. We have had hitherto only small differences of the sound. You have heard the beatings quite plainly. Now, I can alter the pitch of the sound so that there shall exist great difference of pitch; then the beatings become very swift. This is nearly a difference of a semitone, and you hear the beatings very plainly. If you take a single tone also, you hear the beatings much more plainly. Then the number of the beatings is double—the natural *c* and the sharp *c*, the octave higher, is twice as fast. Here the beatings become almost distinct, and you will distinguish very well how the slow beatings pass into the swift beatings—[striking a lower note]; and therefore we have here the same phenomenon, only with great velocity—[striking lower notes]. Now this, I may say, is the real cause of the dissonance. If two sounds of nearly the same pitch are combined, they are not felt. They destroy each other sometimes, and strengthen each other at other times; so that they do not flow away in a continuous manner like the single sound, but they agree with each other, and disturb each other. Therefore, combining two distinct sounds by the single tone, we have beatings. Beatings, as I have explained to you, is an interrupted sound; and we have also an interrupted sensation in the ear. An interrupted sensation in the ear is disagreeable to the ear. It is the same with all other interrupted sensations. If you take a quivering light which falls upon the eye, you will be dazzled, and you cannot bear such a light a long time. You may compare it with scratchings on the skin. That is also a discontinuous sort of exciting the nerves of the skin. This same sort of disagreeableness exists for the ear, and makes it troubled; but by the sensation of two sounds which come to each other. I have given you by this the first phenomenon of dissonance. I would like to explain to you the dissonance of the harmonics, if you will allow me. I will show you by the siren that really interrupted sound gives quite the same impression to the ear. I can put into the siren here a little reed pipe, which gives a high tone. It is put into this box and the disc is turned. The holes here in the under side of the box are opened and closed alternately, so that the sound and the current of air can go out or is closed in. This interrupted sound which is then not a beating sound—not produced by the beating of two sounds, gives quite the same impression as a really beating sound, a sensation of dissonance, such as an interrupted sound. But we have also other dissonances of two sounds, which are not very near in their pitch: the dissonances of the impure octaves, and so on. In this case we must consider that nearly all musical instruments give harmonic sounds, and even these instruments which are of the best application in music, as the piano, the violin, the guitar, and sharp organ pipes. All these have harmonics of great intensity, and these harmonic sounds must all give beatings. We may show that in the organ. Here we have the sound of a stop pipe with feeble harmonics; but I may combine it with the higher sound, and thus give another quality of

sound. I connect it with the octave as it is connected in some of the registers of the organ which are called mixtures of the organ. Now, if I combine with this pure octave an impure octave, I make this beating: you have really in this case the beatings of these two sounds, but neither of these can be the harmonic, neither sound for instance in this reed pipe. You can hear the beatings and so on. If I take the fifth, then, these sounds have harmonics. If they are the harmonic sound of the lower and the harmonic sound of the higher, they will give no beating, but if they are not quite the same, then we shall have beatings. This is upon the fifth. You hear the beatings, they come swifter and swifter, the more the sound is altered. Now, if we suppose this theory that the difference of the pitch is felt by the different excitation of the little elastic sounds which are connected here with the ends of the nerves, we can calculate, for instance, from two sounds of a violin which are brought together, the amount of roughness which is produced by the intensity of beats of the different harmonics. We know the intensity of the different harmonics, and we can calculate by theory in what instances, the different little elastic solids will swing with each of these sounds and their harmonics, and we can calculate the intensity of the roughness, the intensity of the beatings which are produced by these two sounds.

Now, suppose that this drawing [pointing] represents the interior of an octave, the harmonic sounds, and that on the violin the sound is produced representing the fundamental sound of this octave, and another sound in any given interval of this octave from the first fundamental sound is produced at the same time. Then these little hills which are drawn above the curve represent the roughness of the intensity of the beatings which are produced by different harmonics of the two sounds. This lower curve represents the beatings produced by the fundamental sound, the black one the beatings produced by the first harmonic of the fundamental; the green the beatings of the second harmonic; the yellow the beatings of the third harmonic, and so on. Now, you see that here remain some valleys between these hills. These valleys correspond to such intervals where the roughness is a minimum, and you see the roughness is a minimum at first when the two sounds are in unison. The one is the octave of the sound. Then we have a deep valley which corresponds to the best consonants of the octave and the fifth. Then we have the other valley which corresponds to the fourth, one to the major third, minor third, the major sixth, and then a very imperfect consonance, the minor sixth. And here remains a place which corresponds nearly to the consonant interval which is not used in music—the natural seventh. Therefore you see that, calculating only by theory, the roughness of the beats we get quite completely, not only the places for the consonant intervals of the scale, but even the different degrees of the consonant intervals. Here the valley of the fifth comes down to the basis; that of the fourth not quite to the basis; that of the major third is still higher, and the minor third more so, and so on. And so we get the series of these consonantal intervals.

In this way we can give a complete account of the difference of consonance of these intervals. I must remark that as we have some qualities of sound without harmonics it seems that these would give no dissonances if they are not two sounds coming together. But in this case there is another phenomenon,—sounds of combination which I cannot describe here; they cannot be the subject of a public Lecture, because they are only heard by great attention and if one is quite undisturbed. It is a phenomenon which is not easily observed. I will only remark that for such cases where the harmonics are not very intense these combination sounds can also produce beatings, and that by those beatings of the combination sounds the dissonances and the consonances are determined in these cases where the harmonics are wanting. Whilst you will hear very easily the roughness of consonance, it is very different

if the quality of the sound is altered. I will alter the pitch of the octave. In the sound with strong harmonics you hear the beatings. Here the open pipe, they are there also perceptible, but not so sharp as before. In the stopped pipe they are of less intensity and so on. Now, these stopped pipes give, indeed, dissonances, but they are so feeble that the stopped pipes in the organ cannot be used for music in common. They are too soft, and have too small an amount of energy. The dissonances give energy; if the difference between consonances and dissonances is wanting, we feel a want of energy in the music, and, therefore, the stopped pipes can only be used exceptionally. And even the organ pipes have all too small an amount of harmonic sounds, and, therefore, if the intensity and energy of the organ is required, all these mixtures are used—that is, we combine artificially with other organ pipes some organ pipes which give the harmonic sounds of this pipe, so that we get the strong intensity of harmonics.

I shall conclude this Lecture by a more general consideration of the nature of that which impresses our mind as beautiful in the fine arts. The question which is illustrated in a very striking way by that which we have said on the theory of the harmonic. That there are laws to form every sort of beauty cannot be doubted, for we cannot alter any work of art arbitrarily without destroying its beauty. It may be a picture, statuary, a work of poetry, or a piece of music, but these laws are not given by our understanding. Some of them, indeed, can be expressed in the form of practical rules; but always the highest degrees of artistic beauty are those which are understood and described in the most difficult way. Philosophers, therefore, long ago have stated that there are laws of beauty—that beauty must agree with innate laws of our mind, that it reveals these laws: to the immediate perception of the senses, and that we feel its communication without being conscious of it. We may, indeed, understand and explain some parts of that beauty, some of its most striking features, but we would not be inclined to acknowledge as beautiful, in the artistic sense, a work which we know to be the effect of mere calculation and logical conclusions. Such a work always appears poor in beauty. We must feel that there is a larger stock of truth, nature and passion, in any work of art than we can derive by the mere speculation of the understanding; then such a work of art appears to us as infinitely rich; it appears as a revelation of that original harmony, consistency, and truthfulness, which belonged to the human mind, as it came from the hands of its Creator, before it was contaminated by egotism and stunted by the constraint of every-day life. This paradox has always puzzled philosophers.

We ought to feel the latent consistency and lawfulness of beauty without becoming really conscious of it, and without understanding it. Our present subject, however, gives a fine example of this process. We have in the present case only the most simple elementary rules of harmony, we have nothing to do with the higher rules of melody and musical composition. The consideration, however, which I have brought before you applies also to these elementary cases. We have seen that harmony is different from disharmony, by the beating of harmonic sounds. Now we have the sensation of these harmonic tones in our nervous apparatus; the existence of these sounds is level, also in the common use of the ear, or the difference we perceive between sounds of different qualities. But commonly, without performing philosophical experiments, we are not conscious of the existence of the harmonic tone in the sensation of our ear, and therefore of the phenomena depending on the existence of those tones, as, for instance, their beats are felt and an alteration of the quality of the sound commonly has a sort of roughness, we perceive very well that chords agreeing with the laws of consonance and agreeable to the ear are beautiful, and

different from dissonant chords, without being able to perceive the cause of this difference. Therefore we have here such a law of sensation as I have described before; we feel that two sounds coming together either agree or disagree, but we cannot say why they agree or disagree. In this way the general rule of artistic beauty is kept also in this case, where we have to do only with the more elementary part of music; for, of course, these our explanations are not sufficient to answer for the higher beauty of music, for its power and its softness, and you may expect that a long time will pass on before we shall be able to understand their mechanism. At present I think we must be content to have thrown only a glimpse into the first elementary questions of this beautiful subject.

NOTICES OF PATENTS.

145. *Preparation of Non-conducting Substances for the Deposition thereon of Metals by Electric Action.* B. PIFFARD, Caroline Villas, Kentish Town, London.

IN our second volume, p. 323, will be found a communication from Mr. Piffard on this subject. He has, it seems, further improved upon the process there given, and now takes out a patent for it. In carrying this invention into effect, the surface to which it is desired to impart a metallic coating is washed with a solution of nitrate of silver, in such a manner that a film of the solution adheres to the whole of the surface, which is then thoroughly dried, and afterwards exposed to the action of sulphuretted hydrogen gas. The result of this process is, that a conducting surface of metallic silver will be produced, which can then be treated by any of the known methods of obtaining the electro deposition of metals. If the surface under treatment will not retain a complete film of the solution of nitrate of silver, it should be previously washed with a thin solution of gum, or other suitable substance, and then dried, before applying the solution of nitrate of silver.

This is likely to be very successful, and will be a decided improvement upon the old methods of obtaining this result by the use of black lead or metallic powders.

163. *Cast Steel.* R. MUSHET, Coleford.

THIS invention consists in adding to steel, or to any mixture of materials which, when melted, produces cast steel, a mixture of granulated pig iron, cast or refined iron, and pulverised titanium ore, titaniferous iron ore, titanio acid, or oxide of titanium, and melting the said steel-producing materials, and the said mixture of granulated iron and pulverised titanium ore together in the melting-pot, in order to alloy the titanium contained in the said ore or oxide with the cast steel melted from the mixture of steel-producing materials, operated upon so as to produce thereby an improved quality of cast steel. And, secondly, in adding to steel, or to any mixture of materials which, when melted, produce cast steel, a mixture of granular iron and deoxidised ilmenite or compound ore of titanium and iron, and melting the said steel or steel-producing materials and the mixture of ore of titanium and iron and granular iron together in a melting pot, in order to improve the quality of the cast steel obtained.

175. *Telegraphic Cables.* J. CHATBERTON, Highbury Terrace, and W. SMITH, Pownall Road, Dalston.

FOR the purposes of this invention, in order that the gutta percha insulating coatings should not be subjected to heat in the process of manufacturing a cable—as has heretofore been the case when applying hot pitch, resin,

and other matters, together with or after the fibrous materials and wire coatings have been applied round the gutta percha coated wires—the patentees apply oxides or carbonates of lead, or other preparation of that or other metal, combined with tar, oil, or varnish, together with the fibrous materials, and externally of the insulating gutta percha coatings, whether wire be employed as well as fibrous materials to give strength to the cable or not. For these purposes the fibrous material used, whether made up in the form of tape, yarn, cord, or otherwise, is coated or saturated with the paint or composition above mentioned, and by preference in the act of applying such fibrous materials and whether such fibrous materials are wound spirally around or laid longitudinally over the surfaces of the gutta percha; and it is preferred to bind an uncoated tape exterior of the saturated coatings of fibre, in order that the cable may be at once coiled.

For obvious reasons, we think the introduction of any metallic compound in an insulating coating inadmissible; especially when, as in this case, we do not see what good end is gained by the addition.

106. *Iron and Steel.* W. LONGMAID, Inver, Galway.

THIS invention has for its object the alloying of iron and steel with minute quantities of gold and platinum, thereby greatly increasing the strength and otherwise improving the quality of the metals so treated. The gold and platinum are combined when in a melted state with the iron and steel when they are in a melted state, or in the progress of their manufacture. There is no doubt that the quality of steel and iron would be improved by this addition. Faraday proved that long ago; but we fancy that steel manufacturers would look twice at a sovereign before throwing it into the melting-pot. Mr. Longmaid need not fear infringements of this patent.

197. *Glass.* W. W. DOBESON and G. WARREN, Bill Quay Bottle Works, near Gateshead.

THESE improvements consist in using air under pressure under the gratings of the furnace, by which the draught through the fuel may be insured and rendered more constant and certain, notwithstanding the changes of weather. The air is forced into a closed ash-pit under the grating or fire-bars of the furnaces by means of a fan or any other suitable blowing apparatus.

Grants of Provisional Protection for Six Months.

1646. John Cass Smart and Adam Aitchison, Scarborough, Yorkshire, "Improvements in the manufacture of charcoal."—Petition recorded June 27, 1861.

1856. William Edward Gedge, Wellington Street, Strand, London, "Improvements in the preparation and clarification of the saccharine matters obtained from beet-root, sugar-cane, Indian millet, and other sacchariferous vegetables or plants."—A communication from Louis Constant Bernard, Wasserling, Haut Rhin, France.

1858. Alfred Wood, Lewes, Sussex, "Improvements in apparatus employed for fermenting purposes in brewing beer, as also for storing beer, and for general purposes of fermentation."

1892. Charles Constant Joseph Guffroy, Lille, France, "Improvements in preparing medicinal substances and compounds from the livers of cod and other salt water fish."

Invention Protected for Six Months by the Deposit of a Complete Specification.

1947. Marc Antoine Francois Mennons, Rue de l'Echiquier, Paris, "An improved odontalgic elixir."—A communication from Jean Pierre Tajan, Paris.—Deposited and recorded August 6, 1861.

Notices to Proceed.

825. John Gay Newton Alleyne, Butterly Ironworks, Alfreton, Derbyshire, "Improvements in machinery employed in the manufacture of iron."

853. Thomas Goulston Ghislin, Hatton Garden, Holborn, London, "Preparing, applying, and adapting certain vegetable productions called *Eiklonia-buccinalis*, *Protaceae*, *Juncus Serratus*, *Juncus Trista*, and *Armyllidoea*, to further new purposes of manufacture, and certain modes to effect the same."—Petition recorded April 6, 1861.

876. Francis Taylor, Romsey, Southampton, "Improvements in apparatus for receiving, drying, and deodorising human excrement."

877. Frederick Ransome, Ipswich, "Improvements in the manufacture of artificial stone and cement or plaster, and in treating timber for the purpose of preserving the same."—Petition recorded April 9, 1861.

935. Robert Hodgson and Enoch Holden, Carlisle, "Improvements in the manufacture of soap."—Petition recorded April 16, 1861.

1012. Michael Henry, Fleet Street, London, "Improvements in apparatus for aerating liquids."—A communication from Jean Auguste Bazet, Boulevard St. Martin, Paris.—Petition recorded April 23, 1861.

1051. Ferdinand Charles Warlich, London Street, Fenchurch Street, London, "Improvements in preparing coal used in the manufacture of artificial fuel."—Petition recorded April 26, 1861.

1802. Charles Constant Joseph Guffroy, Lille, France, "Improvements in preparing medicinal substances and compounds from the livers of cod and other salt water fish."—Petition recorded July 29, 1861.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Magnetic Chrome Oxide.—Geuther describes (*Annal. der Chem. und Pharm.* Bd. cxviii. s. 61) a process for procuring the magnetic chrome oxide (first obtained in the amorphous state by Wohler) in a crystalline form. He passes the vapour of acichloride of chromium evenly, and not too quickly, through a glass tube a foot and a-half long, heated in a combustion furnace. The ignited charcoal is placed so that it does not come into contact with the tube. When the tube has cooled, the inside is found to be covered with small glistening crystals, which, by their brilliant appearance and colour approaching to violet, may be easily distinguished and separated from the black crystals of the ordinary chrome oxide formed here and there in places where the heat has been very strong. The sparkling violet crystals are strongly magnetic, and must be crystals of the magnetic oxide. Care must be taken not to raise the heat too high, but the most crystals are formed when the tube is raised to the highest temperature allowable.

Geuther found that this magnetic oxide of chromium contains more oxygen than the green oxide. That prepared by the author lost 6.55; 5.88; 5.14 per cent. of oxygen when converted into the oxide Cr_2O_3 . If therefore, it is not considered as an independent oxide, it may be regarded as a compound of 2 atoms of chrome oxide and 1 atom of chromic acid. The crystallised magnetic oxide once formed is a very stable compound; but by long continued heating it may be converted into the green oxide, the crystals retaining their original form. The same change takes place quickly when the crystals are heated in a current of hydrogen. In the amorphous state this oxide is a black powder insoluble in hydrochloric, nitric, and nitro-hydrochloric acids, or even in a mixture of fluoric and nitric acids. By boiling in a strong solution of the fixed alkalis it is slowly converted into chrome oxide and chromic acid, and quickly undergoes the same

change when fused with hydrates of the alkalis. The sp. gr. at 10° is close upon 4; it stands, therefore, in this respect between that of chromic acid 2.63, and that of the crystallised green oxide 5.21. The crystals appear to be prisms with a rhombic base. Some magnetic oxide appears to be formed when pure chromic acid is heated alone or in a current of oxygen.

Nitro-iodic Acid.—The yellow body obtained when iodide is treated with mono hydrated nitric acid, Kaemmerer regards (*Journ. für prakt. Chemie*, Bd. lxxxiii. s. 65) as nitro-iodic acid, or iodic acid in which one equivalent of oxygen is replaced by one atom of nitric oxide. The formula therefore is (IO_4NO_2) . The substance is most easily prepared by rubbing iodine in fine powder with a mixture of equal parts of ordinary fuming nitric acid and strong sulphuric acid. It is a very unstable compound. When dried over lime under a glass bell it decomposes, giving off nitric oxide gas. Water destroys it. A very small quantity brought in contact with absolute alcohol, ether or acetic ether sets fire to those bodies and gives off thick clouds of iodine vapour. Exposed to the air at a moderate temperature it quickly breaks up into I , IO_3 , and NO_2 , but at a very low temperature and in a dry atmosphere it may remain unchanged for twenty-four hours. Concentrated sulphuric acid dissolves it slowly without decomposition. It is precipitated from the solution, however, when the sides of the vessel are rubbed hard with a glass rod, or when strong nitric acid is added.

Double Hyposulphates.—Dr. Kraut describes (*Archiv. der Pharm.*, Bd. clvi. s. 129) crystals of double salt $\text{BaS}_2\text{O}_8\text{Na}_2\text{O}_8 + 4\text{Aq.}$ obtained by the spontaneous evaporation of a solution containing equivalent weights of the two salts. Crystals of the baryta salt are obtained first, then some of the soda salt, and lastly, large transparent crystals of the double salt. The crystals are permanent in the air. They have rounded angles and convex surfaces.

Another double salt $\text{Ag}_2\text{S}_2\text{O}_8\text{Na}_2\text{O}_8 + 4\text{Aq.}$ is obtained in the first crystallisation of a solution, containing equivalent weights of the two salts. The crystals appear to be isomorphous with the soda and silver salts. They effloresce over oil of vitriol, but not in the air. The author could not form double salts with the hypsulphates of many of the metals.

II. ORGANIC CHEMISTRY.

Amount of Theine in Paraguay Tea.—Rammelsberg found (*Bericht. der Akad. der Wissenschaft zu Berlin*, 1861, s. 263) that benzole easily extracted the theine from Maté, or Paraguay tea. In this way he obtained 0.44 per cent., a much larger proportion than Dr. Stenhouse extracted.

Ether Formed by Fermentation.—Leuchs says (*Chem. Centralblatt*, No. 25, 1861, s. 385) that raisins, from which the greater part of the sugar has been extracted by boiling, made into balls, laid under a glass bell, and kept at 18°C . form ether, which is also produced when cotton wool, moistened with a mixture of sugar, spirit of wine and water, is placed under the same circumstances. The author says that ether is always formed when spirit of wine, sugar and water are simultaneously present in porous bodies.

Influence of Sulphur on Fermentation.—When finely-powdered sulphur is placed in grape juice, Leuchs says the fermentation goes on as usual with the formation at the same time of a good deal of sulphuretted hydrogen. If the must is not too much diluted with water, there is a smell, and even taste resembling that of garlic or onions, but when much diluted the smell is more like that of liver of sulphur. The sugar appears to be decomposed but slowly, for the fluid remains sweet a long time. Leuchs suggests that mineral waters, containing sul-

phuretted hydrogen and carbonic acid, may be prepared by fermenting with a little sulphur. Grapes which have been dusted over with sulphur to cure the disease, the author says, give wine that smells of sulphur, but loses the smell in six months, in consequence of the oxidation of the sulphur. Powdered sulphur added to a solution of grape sugar excited no fermentation in the first eight days. Afterwards the fluid became cloudy, and sulphuretted hydrogen was evolved. The fluid passed through filtering paper with great difficulty, and gum appeared to have been formed. (*Ibid.* s. 387.)

MISCELLANEOUS.

The Urine of *Omnivora* reduces ferricyanide of potassium to ferrocyanide, which can be shown by the addition of an iron or copper salt. It is occasioned by the uric acid in solution.—Dr. L. C. Levoir.

The Bessemer Process of Making Steel.—Hematite pig-iron, smelted with coke and hot-blast, has chiefly been used. The metal is melted in a reverberatory furnace, and is then run into a founder's ladle, and from thence it is transferred to the vessel in which its conversion into steel is to be effected. It is made of stout plate iron, and lined with a powdered argillaceous stone found in this neighbourhood below the coal, and known as ganister. The converting vessel is mounted on axes, which rest on stout iron standards, and by means of a wheel and handle it may be turned into any required position. There is an opening at the top for the inlet and pouring out of the metal, and at the lowest part are inserted seven fire-clay tuyeres, each having five openings in them; these openings communicate at one end with the interior of the vessel, and at the other end with a box called the tuyere box, into which a current of air from a suitable blast engine is conveyed under a pressure of about 14lb. to the square inch, a pressure more than sufficient to prevent the fluid metal from entering the tuyeres. Before commencing the first operation, the interior of the vessel is heated by coke, a blast through the tuyeres being used to urge the fire. When sufficiently heated, the vessel is turned upside down, and all the unburned coke is shaken out. The molten pig iron is then run in from the ladle before referred to; the vessel, during the pouring in of the iron, is kept in such a position that the orifices of the tuyeres are at a higher level than the surface of the metal. When all the iron has run in, the blast is turned on, and the vessel quickly moved round. The air then rushes upwards into fluid metal from each of the thirty-five small orifices of the tuyeres, producing a most violent agitation of the whole mass. The silicium, always present in greater or less quantities in pig iron, is first attacked. It unites readily with the oxygen of the air, producing silicic acid, at the same time a small portion of the iron undergoes oxydation, hence a fluid silicate of the oxide of iron is formed, a little carbon being simultaneously eliminated. The heat is thus gradually increased until nearly the whole of the silicium is oxydised; this generally takes place in about twelve minutes from the commencement of the process. The carbon now begins to unite more freely with the oxygen of the air, producing at first a small flame, which rapidly increases, and in about three more minutes from its first appearance we have a most intense combustion going on: the metal rises higher and higher in the vessel, sometimes occupying more than double its former space. The frothy liquid now presents an enormous surface to the action of the oxygen of the air, which unites rapidly with the carbon contained in the crude iron, and produces a most intense combustion, the whole, in fact, being a perfect mixture of metal and fire. The carbon is

now eliminated so rapidly as to produce a series of harmless explosions, throwing out the fluid slags in great quantities, while the union of the gases is so perfect that a voluminous white flame rushes from the mouth of the vessel, illuminating the whole building, and indicating to the practised eye the precise condition of the metal inside. The workman may thus leave off whenever the number of minutes he has been blowing and the appearance of the flame indicate the required quality of the metal. This is the mode preferred in working the process in Sweden. But here we prefer to blow the metal until the flame suddenly drops, which it does just on the approach of the metal to the condition of malleable iron: a small quantity of charcoal pig iron, containing a known quantity of carbon, is then added, and steel is produced of any desired degree of carburization, the process having occupied about twenty-eight minutes from the commencement. The vessel is then turned, and the fluid steel is run into the casting ladle, which is provided with a plug rod covered with loam: the rod passes over the top of the ladle, and works in guides on the outside of it, so that, by means of a lever handle, the workmen may move it up and down as desired. The lower part of the plug, which occupies the interior of the ladle, has fitted to its lower end a fire-clay cone, which rests in a seating of the same material let into the bottom of the ladle, thus forming a cone valve, by means of which the fluid steel is run into different sized moulds, as may be required, the stream of fluid steel being prevented by the valve plug from flowing during the movement of the casting ladle from one mould to another. By tapping the metal from below, no scoria or other extraneous floating matters are allowed to pass into the mould.

When in a closed Funnel a jet of water enters with pressure in the direction of the tangent at the largest circumference, and coal-gas enters in the direction of the axis, the gas can be lighted amidst the jet of water which revolves downwards.—Dr. L. C. Levoir.

Copper Smelting.—*The Ontonagon* (Lake Superior) *Miner* states that the representatives of the French company have been making investigations among the mines of the Ontonagon district in relation to the establishment of smelting works, and that the result of their investigations will undoubtedly be the early erection of such works. The fuel used will be wood. In June, the Isle Royale Mine, turned out 65 tons of copper, the Franklin 80, the Pewabic 85, the Quincy 140, and the Huron, Hancock, and Portage 22 tons, making an aggregate of 396 for the month.

ANSWERS TO CORRESPONDENTS.

* All Editorial Communications are to be addressed to the EDITOR; and Advertisements and Business Communications to the PUBLISHER at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

* In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Coke.—A small square box, containing two lumps of coke, was delivered by a carrier, carriage-paid, at the Editor's private address, last week. There was no name of the sender, or anything else on it, to show whence it came; and no letter has since been received which in any way refers to it.

J. A. Tring.—The metal is deposited on the pens by the electrolytic process, and subsequently polished by friction. The most uniformly successful electrolytic solutions consist of double cyanides of the metal to be deposited, with cyanide of potassium.

Nemo.—1. Mr. Leslie's address is Conduit Street, Regent Street. 2. The first patent is tenable unless the other party has previously published the principle of it. Private knowledge of it goes for nothing.

A Poor Chemist's favourable opinion has been received by the Editor with much pleasure.

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SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Colour-tests for Strychnia and the Diagnosis of the Alkaloids, by WILLIAM A. GUY, M.B. Cantab. Fellow of the College, and Professor of Forensic Medicine, King's College, London.

(Continued from vol. iii. p. 303.)

THE last of the four questions proposed for solution in my first communication still remains to be discussed. It relates to the diagnosis of the alkaloids, and was expressed in the following terms:—Is it possible by means of the colour-tests, or by any simple modification of them, to distinguish the alkaloids from each other?

Before proceeding to examine this question I must briefly refer to what I have already stated respecting the colour-tests. These tests, as commonly understood, are compound tests, consisting first of the reaction of the alkaloid with sulphuric acid, and then of the reaction of the peroxides of lead and manganese, the bichromate of potash, the ferricyanide of potassium, and the permanganate of potash, with the mixture of the alkaloid and sulphuric acid.

The sulphuric acid produces in strychnia no change of colour. Hence the acid itself becomes a test of no mean value. It serves, as I shall point out more particularly presently, to distinguish strychnia from a considerable number of the alkaloids, and also from several substances which give with sulphuric acid alone coloured reactions, some of which reactions with the acid alone bear a certain resemblance to those of strychnia with sulphuric acid *plus* the bichromate of potash, ferricyanide of potassium, permanganate of potash, peroxide of lead, or peroxide of manganese. In order to distinguish strychnia from other alkaloids, and from the substances to which I have just referred, it is essential that the sulphuric acid should be first applied to, and well mixed with, the strychnia, and that the absence of colour having been noted, the reagents that develop the colour should be in their turn applied. In all that I have said on the subject of the colour-tests for strychnia, and in what I have yet to say, I assume this mode of procedure to be strictly observed; and I shall continue to speak of these reagents as the colour-producing tests.

I have hitherto assumed that the sulphuric acid is applied cold; but I shall presently point out that we may greatly add to the value of this constituent portion of the colour-tests by first warming and then heating the acid solution. A very slight rise of temperature serves to develop very beautiful and characteristic colours in some of the alkaloids, while it does not change the colour of strychnia. A further rise of temperature to the point of causing the mixture to give off vapour,

develops colour in strychnia also, and deepens or changes the tints already imparted to the alkaloids previously affected by warming the acid mixture.

The colour imparted to a solution of strychnia in sulphuric acid by raising it to a temperature at which it gives off vapour is not strongly marked. It is a faint yellow or light greenish-brown, differing somewhat in depth of tint with different specimens of strychnia treated in all respects exactly alike. If the acid solution is allowed to cool it no longer acts characteristically with the bichromate of potash or the other tests. Their action is wholly destroyed or greatly impaired. Hence in applying the colour-tests for strychnia, we must use concentrated pure sulphuric acid, and use it cold.

So much for the first part of the colour-tests. We add to the alkaloid under examination sulphuric acid in small quantity—pure, strong, and cold; and it undergoes no change of colour. The alkaloid, therefore, whatever it may be, belongs to the class of which strychnia is one.

To this cold acid solution of the alkaloid we add a minute fragment of bichromate of potash, ferricyanide of potassium, permanganate of potash, peroxide of lead, or peroxide of manganese. The result is the remarkable development of successive transient, or short-lived, colours already described.

The negative reaction of strong cold sulphuric acid on strychnia, followed by the effect of heat on the acid mixture, and this by the peculiar colours produced by the application of the colour-developing substances to the cold acid mixture, constitute a series of phenomena which afford fair promise of proving the means of a successful diagnosis of the alkaloids.

To this work of distinction I now address myself, and in order to prepare the way more completely for the work of tabular analysis towards which the details given in my former communications have been tending, I must again revert to the question whether the action of the colour-tests on strychnia is, or is not, characteristic, and therefore, diagnostic.

This is a question which must have suggested itself to every chemist engaged in medico-legal inquiries as of the utmost practical importance, and one chemist (Mr. Thomas E. Jenkins) having been employed to investigate a case of suspected poisoning, in which the colour-tests gave indications of the presence of strychnia, very properly put the question to the test of experiment, by applying to a variety of active principles, including most of the alkaloids, first, colourless concentrated sulphuric acid, and then a fragment of a crystal of bichromate of potash. The experiments, which were carefully performed, and appear to have been strictly comparable one with another, embraced no less than fifty alkaloids and active principles, derived from the animal as well as the vegetable kingdom, and among them, urea and uric acid, and cantharidine. More than half of these substances differed from strychnia in yielding colour when treated with sulphuric acid. The smaller half resembled strychnia in this respect. But not one of the whole

¹ Substance of part of the Croonian Lectures for 1861, delivered at the Royal College of Physicians; from the *Pharmaceutical Journal*.

fifty gave with the bichromate of potash the characteristic coloured reactions of strychnia. Feeling that my time would not be wasted in repeating his experiments, and, if possible, extending them to substances not included in his list, I possessed myself (partly by the kind assistance of Mr. Morson) of all the alkaloids and similar active principles which could be obtained, and tested them in the same way. To Mr. Jenkins' list, some of which I was not able to procure, I succeeded in adding as many as sixteen new substances. None of these gave the characteristic coloured reactions of strychnia, nor any succession of colours any way resembling them, so that I am now in a condition to assert that strychnia stands alone among sixty-six analogous compounds in the reactions which it gives with sulphuric acid followed by the bichromate of potash.

Mr. Jenkins (as will be seen in the table published by him in the *CHEMICAL NEWS* for October 6, 1860) arranges his reactions in two columns. The first column displays the effect produced by strong sulphuric acid; the second, the changes of colour caused by the addition to the acid solution of the bichromate of potash.

Now this table affords an excellent illustration of the absence of method and logical arrangement. The substances operated on evidently took their places as they chanced to come to hand. Strychnia itself has to be sought out; and substances which give coloured reactions with sulphuric acid, find themselves in contact with others that give a negative result.

And yet the table seems to court the attention of lovers of method and logical order. It is almost impossible to keep one's hands off it. At least, one would like to place the substances which are not coloured by sulphuric acid by themselves, and those that are coloured also by themselves. I have undertaken this work of tabulation, and, on inspecting the result, am seized with the same desire to resume the work of arrangement. In both groups there are substances which yield with bichromate of potash the same colours. Why should not these similar reactions be bracketed together? This, accordingly, I have carried into effect, and the result is a splitting up of the fifty substances into several groups of such moderate dimensions, that a man given to tabular analysis can scarcely resist the additional temptation to try whether some modification of the tests themselves, or some additional tests, or both together, may not lead to a successful separation and elimination of each and all the substances contained in the table.² I now proceed to lay before you in a tabular form the results of a very laborious experimental attempt to distinguish some of the principal alkaloids and active principles, whether derived from the vegetable or animal kingdom, from each other.

As the first object which I had in view was to distinguish the poisonous alkaloids and analogous active principles from each other, these substances are of course admitted into the tables; but I have added to these the active principles of many of our aperient medicines and common articles of diet; and have ex-

cluded only such of those substances as were so strongly characterised by colour or odour, as not to be properly grouped with the colourless, or faintly-coloured, and inodorous alkaloids and active principles.³ The table, divided for convenience into two, consists, as it is, of as many as thirty-five different substances, closely resembling each other in physical properties and chemical composition, and offering collectively a very difficult subject for tabular analysis. The tables, as you have them before you, are the result of a long series of experiments, and were only made to assume their present form after a great many experimental groupings and transpositions.

The first object which I had in view was to find some simple test which would divide the whole body of active principles comprised in the two tables into two principal groups. Concentrated sulphuric acid, as a constituent of the strychnia colour-tests, was obviously well adapted to this purpose. It has the effect of dissolving the alkaloids and active principles without change of colour in the case of one considerable group (Table I.), and with change of colour in the case of another considerable group (Table II.). The first group (Table I.) comprises the majority of the active poisonous principles—strychnia, brucia, morphia; atropine, picrotoxia, aconitina; and the animal principle cantharadine. With these and several other alkaloids and active principles, some of which are very harmless ones, concentrated sulphuric acid either produces no change of colour, or a faint yellow, straw, or buff tint.⁴ I speak of cold concentrated sulphuric acid, applied as in the preliminary to the colour-tests for strychnia.

The next object which I had in view was to subdivide these two large groups (Tables I. and II.) each into two or more smaller groups by some second test, which might also, perchance, produce in some members of the two groups characteristic reactions, so as to eliminate or separate them at once. For this purpose I tried the effect of warming the acid solution of the alkaloids by passing the porcelain slab through the flame of the spirit lamp, so as to raise the temperature of the liquid to a point short of that at which it gives off vapour. This subsidiary test answered its purpose completely; the group of substances in Table II. which had already given colours, more or less characteristic, with the cold acid, underwent further changes of colour more or less peculiar; while the group in Table I. which had undergone no change with the cold acid, became subdivided into two leading groups, of which one continued without colour, and the other underwent changes of colour more or less characteristic. In the instances of meconine and of aconitina, the colours developed proved to be peculiar to these substances, so as to lead at once to their elimination from the rest.

By raising still further the temperature of the acid solutions to a point at which vapour was given off, a further separation into distinct groups or classes was

³ To this statement it will be seen that there are one or two exceptions, such as naphthaline, which, though colourless, has the odour of tar.

² In the lectures given at the College of Physicians, Mr. Jenkins's original table, and the two tabular arrangements alluded to in the text, were sent round. These tables were the more freely used as illustrations of a want of logical arrangement, as the author evidently did not aim at diagnosis, but only at the distinct ascertainment of the peculiar and characteristic reactions of strychnia. The remarks in the text were not, therefore, intended in any respect as a censure of Mr. Jenkins for not adopting an arrangement which, for his purpose, was unnecessary. I may add, that his table comprises several substances which will not be found in the tables I am about to submit. I mean such substances as the kinic and kinovic acids, and such animal products as urea and uric acid.

⁴ I ought to state in this place that some of the alkaloids are very sensitive to heat, so that a very slight rise of temperature is followed by a decided development of colour. Hence it is possible that experiments made in a cold room in winter may yield results differing somewhat from the same experiments made in a warm room in the height of summer. It must also be borne in mind that sulphuric acid, if not free from nitric acid, may impart to some of the alkaloids a tint of rose, yellow, or buff, sufficiently deep to justify their removal from Table I. to Table II. It may be well, therefore, to state that the experiments on which the tables are founded were made in the winter, in a room with a fire, but in the morning, before the temperature of the room had been raised to a point approaching summer-heat; and that the acid employed was free from nitric acid.

found to take place. The six substances which stand first in order in Table I. still remained colourless, while the remainder assumed tints more or less characteristic. Esculine and atropine; picrotoxia, santonine, and narcotine; amygdaline and naphthaline, formed themselves into three distinct groups; while strychnia, brucia, and morphia gave indications of colour less distinct and characteristic.

By this triple test of sulphuric acid—cold, warmed, and heated—the active principles contained in this division (Table I.) have been either eliminated, or split up into manageable groups, awaiting the application of further tests. It seemed natural to select as the next test in order nitric acid—a known characteristic test of brucia and morphia. By applying this test to the several members of the table in succession, codia was isolated from the group of six to which it belonged by the distinct yellow colour assumed by the acid solution. Emetine was, in like manner, isolated from the other members of the same group of six, by yielding with the acid a yellow-brown colour. Atropine, again, was distinguished from esculine by giving a negative result, while esculine assumed a distinct yellow colour. In the same way narcotine was detached from its group of three, and naphthaline from its group of two. Again, morphia was distinguished by the rich orange hue given by the acid, together with the red fumes of nitrous acid; and brucia was effectually distinguished from strychnia by the intense red of the former contrasting with the faint tinge of red imparted to ordinary specimens of the latter. For the next eliminating test I have selected sulphuric acid followed by a solution of permanganate of potash, in the proportion of 10 grains of the salt to 3j. of water. The effect of this test on strychnia was to develop its characteristic colours, strongly contrasted with the altogether different colours given with brucia, and still more with the brown colour imparted to morphia, followed by the bleaching of the liquid. This same test divided the somewhat unmanageable group of four at the top of the table (cantharadine and asparagine, and caffeine and cinchonine) into two small groups of two each. In the first two the immediate effect of the permanganate was a red-brown colour; in the second, a pink. Picrotoxia and santonine gave the same colour (pink) and remained undistinguished. The last and ultimate work of elimination was effected by means of heat applied to the active principle in its solid state. Cantharadine was found to be wholly dissipated by the heat of the spirit lamp, while asparagine gave a bulky carbonaceous deposit. And caffeine was found to present the same difference when compared with cinchonine. Lastly, picrotoxia was found to be distinguished from santonine by the bulky carbonaceous residue with the first, and the scanty deposit of carbon with the second.

Having by this succession of tests obtained a clue to the active principle with which we have been dealing, we should proceed to identify the substance indicated by the table, through its own characteristic tests.

In the second division of the one large table of elimination, or that which comprises active principles coloured by contact with cold sulphuric acid (Table II.), the same succession of tests is employed. Cold concentrated sulphuric acid at once separates this group of seventeen into eight smaller groups of one, two, or six; and the subsequent warming and heating of the coloured acid solutions occasions such marked differences of colour as to nearly complete the work of elimination. But I have added columns showing the effect of nitric acid, and of

the solution of permanganate of potash. The application of heat to the alkaloids themselves did not prove necessary to the work of elimination.

I submit these tables primarily as specimens of Tables of Elimination; but also incidentally as tables which may prove serviceable to the chemist. I am not certain that they constitute the best aid which could be devised to the diagnosis of the alkaloids. I thought that they might, perhaps, admit of being simplified by adding to the three columns headed "sulphuric acid" a fourth, showing the deposit of carbon resulting from the continued application of heat to the acid solution of the alkaloid. I tried this reaction in the case of fourteen alkaloids and active principles, comprising all the more active poisons of this class, together with the less important principles, emetine and meconine, and found the alkaloids to differ from each other in the amount of carbonaceous deposit. With strychnia, aconitina, and veratria, for instance, the deposit of carbon was less abundant than with brucia, morphia, atropia, and the other poisonous alkaloids. But the difference was not such as to justify me in using it as a diagnostic property. So also with the direct application of heat to the alkaloids themselves. Strychnia, morphia, atropia, cantharadine, meconine, picrotoxia, and delphinia, after melting, deposited but a scanty carbonaceous ash; while emetine, aconitina, and digitaline yielded an abundant ash; and brucia, elaterine, veratria, and solanine yielded an ash intermediate in quantity. This test, again, though fairly applicable to the few cases in Table I., did not seem admissible as a diagnostic on a larger scale. Indeed, the tables, though carefully compiled and based on carefully-conducted experiments, are open to the obvious objection that, with different specimens and different quantities of the alkaloids, and with different specimens of sulphuric acid, we may obtain differences of tint, and more or less ready development of colour under increase of temperature. The majority of the reactions, however, will, I believe, be found constant for all specimens of fair average purity. One

TABLE I.

Alkaloids, &c., giving no Colour with Cold Sulphuric Acid.

Alkaloids, &c.	Sulphuric Acid.			Nitric Acid.	Sulphuric Acid, followed by the Solution of Permanganate of Potash.	Heat.
	Cold.	Warm.	Hot.			
Cantharadine .	o	o	o	o	Red brown	o
Asparagine .	o	o	o	o	Red brown	Carbon ^s
Caffeine & Theine	o	o	o	o	Pink	o
Cinchonine .	o	o	o	o	Pink	Carbon ^s
Codia . . .	o	o	o	Yellow		
Emetine . . .	o	o	o	Yellow-brown		
Strychnia . .	o	o	Yellow ¹	Pink ¹	Blue, Mulberry, Orange	
Brucia . . .	o	o	Yellow ²	Red ²	Red, Brown, Orange, Yellow	
Morphia . . .	o	o	Brown	Orange ³	Brown, then bleached	
Esculine . . .	o	o	Brown ³	Yellow		
Atropine . . .	o	o	Brown ³	o		
Meconine . . .	o	Blue				
Aconitina . .	o	Brown				
Picrotoxia . .	o	Yellow	Brown	o	Pink	Carbon ^s
Santonine . .	o	Yellow	Brown	o	Pink	Carbon ¹
Narcotine . .	o	Yellow	Claret	Yellow		
Amygdaline .	o	Pink	Brown			
Naphthaline .	o	Pink	Brown	Yellow		

alkaloid, hyosciamia, which I was not able to procure, is omitted.⁵

TABLE II.

Alkaloids, &c., giving Colour with Cold Sulphuric Acid.

Alkaloids, &c.	Sulphuric Acid.			Nitric Acid.	Sulphuric Acid, followed by the solution of the Permanganate of Potash.
	Cold.	Warm.	Hot.		
Veratrin ¹	Orange ³	Scarlet	Claret	o	
Piperin ¹	Orange ³	Red-brn.	Green ³	Orange ³	
Delphinia ¹	Pink ¹	Pink ²	Brown	o	Yellow ¹
Salicin ¹	Pink ¹	Pink ²	Claret	o	Claret
Digitalin ¹	Red-brn. ¹	Red-brn. ²	Red-brn. ³	o	Buff
Elaterin ¹	Red-brn. ¹	Red-brn. ²	Red-brn. ³	o	Pink ³
Papaverin ¹	Purple ³	Purple ²	Purple ¹	Yellow	Green; slate Green; brown
Paramorphia	Purple ¹	Purple ¹	Purple ³	Yellow	
Quina ¹	Yellow	Yellow	Yeli-brn.	Yellow	
Alcino ¹	Yellow	Yellow ³	Green	Orange	
Jalapine ¹	Yellow	Orange ³	Red-brn.	Yellow	
Narceine ¹	Yellow	Red-brn. ³	Red-brn. ³	Green-brn. ³	Buff
Phlorizin ¹	Yellow	Red-brn. ³	Red-brn. ³	Red-brn. ³	Pink ³
Solania ¹	Yellow ³	Brown ³	Brown ³	o	
Cubebine ¹	Pink ³			Yellow	
Quinoidin ¹	Brown	Brown	Brown	o	
Sanguinarin ¹	Red-brn. ³	Claret	Claret	Red-brn. ³	

Of the two tables, Table I. affords the best illustration of a table of elimination. For the sake of brevity, and for other reasons already explained, the figures 1, 2, 3, are added to the words describing the colours developed by the tests. These figures indicate different degrees of intensity. In comparing strychnia with brucia, for instance, the figure (1) attached to the word "yellow" opposite strychnia in the third column means a faint yellow colour, less in degree than that indicated by the figure (2) opposite brucia in the same column. So the word "brown" with the annex 3 in the same column opposite esculina and atropine indicates an intense brown colour; the words red 3 and orange 3 in the column headed nitric acid indicate intense red and orange respectively.

On some Combinations of Selenium Analogous to Sulphides, by M. H. UELSMANN.

Selenide of Arsenic, AsSe_3 , deposits itself as a brilliant dark brown powder, when seleniuretted hydrogen is passed into a solution of arsenious acid, slightly acidulated with hydrochloric acid. The seleniuretted hydrogen was prepared by M. Wöhler's process, which consists in making pure, dry, and warm hydrogen act on selenium. Selenide of arsenic melts at 200° , forming a black liquid, which, when cold, forms an amorphous mass, with conchoidal fracture. It is soluble in alkalies, and colours the solution red-brown; nitric acid also dissolves it.

By melting together equal equivalents of arsenic and selenium, a shining, black, amorphous mass, with conchoidal fracture, is formed, soluble in caustic soda by the aid of heat, and yielding a brown-red liquid, and depositing itself from this solution in brilliant, bronze-coloured scales. This body appears to be AsSe_3 .

Selenide of Antimony, SbSe_3 , obtained by precipitating a solution of tartar emetic by seleniuretted

hydrogen, it is a black powder, which, when heated to 146° , suddenly becomes gray. At a red heat it melts into a black liquid, which becomes a granular, crystalline mass. It is soluble by heating in caustic potash, giving it a yellow-brown colour.

Selenide of Tin.—Protoselenide, SnSe , is formed by precipitating a solution of protochloride of tin by seleniuretted hydrogen. It is a brown powder, black when dry, easily soluble in caustic alkalies and in alkaline sulphides, and is neither fusible nor decomposable when heated in a glass tube in an atmosphere of hydrogen.

The same combination is produced with incandescence by heating three parts of tin and four of selenium in a current of carbonic acid. It forms a gray, crystalline, readily-clearable mass, with metallic lustre.

Biselenide of Tin, SnSe_2 , obtained by means of bichloride of tin and seleniuretted hydrogen, is a dark, yellow-orange coloured powder, clear red-brown when dry. By heating in a current by hydrogen, this selenide loses half its selenium and becomes protoselenide. It is soluble in caustic and sulphuretted alkalies; the solutions deposit selenium when exposed to the air.

Selenide of Tungsten, WSe_3 .—A solution of tungstate of soda, saturated with seleniuretted hydrogen, has a brown-red colour. Diluted sulphuric acid precipitates black selenide of tungsten from it, with disengagement of seleniuretted hydrogen. Selenide of tungsten is soluble in caustic alkalies and in alkaline sulphides and selenides.

Selenide of Molybdenum is similarly produced, but it is impure through the presence of a bluish-gray combination.

Selenide of Bismuth, BiSe_3 , is black. It forms, when melted, a metallic-looking button. It is precipitated when a limpid solution of bismuth, with as little acid as possible, is treated by seleniuretted hydrogen. It is soluble in nitric acid, but resists caustic alkalies and alkaline sulphides.

Selenide of Gold, AuSe_3 , is produced by passing seleniuretted hydrogen into chloride of gold in the dark. It is black, and decomposable by heat and by nitric acid.

Selenide of Mercury.—The selenide HgSe is obtained by direct combination, whatever proportions are employed. The combination is effected with hissing sound. At a high temperature the selenide is gaseous, and condenses into brilliant crystals, of a steel-gray colour, with black powder.

By directing a current of seleniuretted hydrogen through a solution of bichloride of mercury, a white precipitate is first produced, containing $2\text{HgSe} + \text{HgCl}$. Nitric acid does not attack this combination. Caustic soda transforms it into $2\text{HgSe} + \text{HgO}$.

Selenide of Cadmium, CdSe , prepared by chloride of cadmium and seleniuretted hydrogen, forms a brown powder, decomposable by hydrochloric acid, with disengagement of seleniuretted hydrogen.

Selenide of Sodium.—Sodium and selenium unite with ignition when heated, forming colourless selenide of sodium. By passing seleniuretted hydrogen into a solution of caustic soda, it assumes the form of a crystalline mass. When the mass is heated, while a current of hydrogen traverses it, the selenide dissolves, and on cooling deposits long, flat crystals. It becomes red and hot in the air.—*Annalen der Chemie und Pharmacie*, vol. cxvi., p. 122.

⁵ In the lectures at the College of Physicians a table was used which showed the colours actually produced by the action of the several re-agents on these poisonous alkaloids and active principles.

TECHNICAL CHEMISTRY.

On the Occurrence of Soluble Compounds of Copper, Lead, and Tin in Newly-distilled Alcoholic Spirits, by Dr. A. A. HAYES.

IN an article discussing the question of the existence of poisonous bodies, especially strychnine, in the spirits produced from grain, which has been so frequently agitated by the public journals during the past few years, Dr. A. A. Hayes expresses his conviction that no good reason can be found for the statements affirming the existence of such substances, which have been made by the newspapers; and remarks that his opinion is founded not only upon an extensive series of analytical observations, but is supported by the testimony of the parties most likely to know of such adulteration if it were really made, none of whom had ever met with a case at the places of manufacture which would lead even to an inference that any deleterious body had been purposely added to the distilled spirits.

But while dismissing this bugbear, Dr. Hayes calls attention to another source of danger which may be of importance. "Newly-distilled spirits, of the most common kind often contain salts of copper, of lead, or tin derived from the condensers, in which the vapours are reduced to a fluid form. The quantity of copper salt contained in the bulk usually taken at a draught, is sufficient to produce the minor effects of metallic poisoning; the cumulative character of these poisons may even lead to fatal consequences. With a knowledge of the fact now stated, instead of resting on a supposition of the existence of an organic poison in the spirit, which may have caused sickness, the physician should notice the symptoms of metallic poisoning in persons addicted to the habit of consuming newly-distilled spirits, and interpose his aid in preventing the fatal termination of vicious indulgence."

The author goes on to remark, that since he first became aware of the frequent occurrence of these metallic salts in recently manufactured spirits, he has made investigations which "prove that as all spirits at one time were new, so with few exceptions—arising from peculiar rectifications—most spirits have been, or are, more or less, contaminated by metallic compounds. Old or more matured spirits have generally lost every particle of the salts once held in solution. Changes in the organic solvent have caused the deposition of the metallic compound accompanied by the organic matter from obvious sources and in such spirits the metallic oxide is always found—if it has been present—in the dark coloured matter which has been deposited at the bottom of a cask at rest. This dark deposit has the appearance of, and has been mistaken for, charcoal, detached from the charcoal staves of the casks in which the spirits have been stored. Of this dark deposit every sample has, on examination, afforded abundance of copper, copper and tin, or copper and lead, even when taken from the finer qualities of foreign spirits. Observations have been made on the nature of this change from a soluble to an insoluble state. Samples of new spirits have been kept in glass vessels until the whole metallic salt has fallen in dark flocks, leaving the clear fluid free from any metallic compound and perfectly pure. It appears, therefore, that matured spirits lose their poisonous impregnation during the time necessary to adapt them for use as beverages, and that while the clear, transparent fluid contains no metallic impregna-

tion, a turbid though ripened spirit may prove deleterious, through its suspended metallic compounds. In order to avoid the poisonous effects of these salts, perfectly well-ripened and clear spirits only should be used in the preparation of medicines, and when ordered as restoratives, no new or turbid alcoholic fluids should be allowed to enter the hospital or room of the patient."

Dr. Hayes explains the origin of these salts as being connected with the production of acids, as well as alcohol in the fermenting vats. "When the wort is subjected to heat in the still, acetic, butyric and other acids rise with the vapour of alcohol, and pass into the condenser, now most commonly made of copper, with masses of solder containing lead. At the instant of condensation, these acids exert a power of corrosion on the metals quite unsuspected, and the salts formed dissolve in the spirit. Where condensers of pure tin are used, no copper salt is formed, and a little tin salt takes place. With the vapour of dilute alcohol some vesicular vapour of the wort is carried forward, and the dextrine which can be found in the spirit; another portion of soluble organic matter is abstracted from the wood of the cask, and this is often tannic acid. In the subsequent chemical changes these organic compounds unite with the salts, and fall in the form of a sub-granular dark matter seen in colourless spirits of all kinds. In detecting the metals held in solution, the extract obtained, after evaporating the spirit, must be destroyed, as usual in toxicological testing, and an acid solution of the oxide obtained; or the extract may at once be mixed with carbonate of soda, and the metal reduced by the blowpipe flame. When the deposit is the subject of trial the metal or metals appear on fluxing with carbonate of soda, in the inner flame produced by the blowpipe, on charcoal.—*Boston Medical and Surgical Journal*, lxiii. 169.

PHARMACY, TOXICOLOGY, &c.

On Liquor Ferri Iodidi, by W. J. M. GORDON, Cincinnati.

NEWLY-prepared syrup of iodide of iron, according to the United States Pharmacopœia, is a transparent liquor of a pale green colour, but frequently becomes a dark red. If exposed to the direct light of the sun, it is restored to its original transparency and colour. The same change will take place if exposed to the indirect light, but requires a much longer time. The loss of colour by the action of light distinguishes the syrup from a watery solution of the iodide of iron, which, if exposed to light, undergoes continual decomposition, becoming darker and darker. The inference is very clear, that the deoxidising influence of the sugar, which is so well known, protracts the decomposition; but the darkening of the syrup continues if the light is excluded, and to the light must be attributed a great influence on the changes going on in the syrup. To ascertain what these changes are, the following experiments have been made:—

1. A dark syrup was filtered; the filtered liquid was clear, of a reddish-brown colour, and contained free iodine, as a test with starch clearly showed. On the filter remained a deposit, which, after being well washed, dried, and heated, smelled strongly of iodine, and left oxide of iron. The solution of this deposit in nitric acid, after an addition of starch, became blue.

2. Some of the dark syrup was exposed to the direct

light of the sun, filtered after it had assumed a light green colour, and the well-washed deposit on the filter was found to be oxide of iron, entirely free from iodine, and in the syrup there was no longer any free iodine, as a test of starch showed.

3. Fifteen grains of iodide of iron and one ounce of water were put in two white bottles; these were exposed to the sunlight; in both of them the liquid soon became turbid, and assumed a red colour, caused by free iodine. To one of the bottles was then added a small quantity of sugar, and its contents became almost immediately nearly colourless and clear, depositing sediment of oxide of iron, while the liquid in the other bottle was constantly becoming darker, and seemed to be entirely decomposed.

From these experiments, the following conclusions have been arrived at. In the dark syrup a combination of iodide of iron is suspended, and free iodine dissolved in it. In the syrup decolorised by the light there is no longer any free iodine dissolved in it, but it contains a sediment of oxide of iron. The sediment in a watery solution of iodide of iron, according to Berzelius, is an oxyiodide of iron, probably $\text{Fe}_2\text{I}_3 + \text{Fe}_2\text{O}_3$. The following will explain this: $4\text{FeI} + 3\text{O}$, form in the dark $\text{Fe}_2\text{I}_3 + \text{Fe}_2\text{O}_3 + \text{I}$.

As experiment No. 2 shows, the oxyiodide is decomposed by exposure to the light, the sediment no longer contains any iodine, and the proto-iodide of iron again formed Fe_2I_3 and $\text{Fe}_2\text{O}_3 + 2\text{I}$. But this iodine, which, according to the formula, is free, can no longer be tested by starch, and the principal purpose of this examination was to ascertain what had become of it. The most simple explanation is, that hydriodic acid had been formed. To test for the hydriodic acid, about one pound of the decolorised syrup was put in a glass retort, and heat applied until half-an-ounce was distilled over. This liquid was found to be a watery solution of hydriodic acid. Nitrate of silver produced a yellow precipitate of iodide of silver, and starch became blue upon the addition of chlorine water.

A solution of iodine in sugar and water, in which iodine is more soluble than in water, has a dark red colour; sun-light changes it to white, and no free iodine can be found in the decolorised liquid, and all reactions show that the iodine is changed into hydriodic acid. The same change takes place in an alcoholic solution of iodine, and in a watery solution of iodine in iodide of potassium. The process given above is a very easy and simple one for testing syrup of iodide of iron for hydriodic acid. How far the medicinal properties of the syrup are affected can only be determined by the careful observation of physicians; but it is obvious that a dark syrup should never be used, in consequence of its containing free iodine.

PHYSICAL SCIENCE.

On the Property possessed by Platinum rendered Incandescent by the Battery to produce Gaseous Combination, by M. E. SAINT-EDME.

I WILL first draw attention to the sensibility of iodised starched paper, as a reagent for oxygenised compounds of nitrogen, hyponitric, and nitric acid, infinitely minute traces of which existing in a medium sufficing to blue very decidedly the iodised starched paper.

M. Leroux's experiment, which consists in obtaining ozone by passing a current of air on a platinum wire,

made red hot by electricity, has led me to examine whether oxygen alone could be modified under the same conditions. I find that pure oxygen passing in contact with a spiral, rendered incandescent by an electric current, has no effect on iodised starched paper, and consequently does not seem to be modified. If, on the contrary, oxygen and nitrogen are made simultaneously to impinge on the red spiral, the issuing gas strongly colours iodised starched paper blue, and litmus paper red; hence, I conclude, that under these conditions nitric acid is formed. I am about to continue these researches with various gases.—*Comptes-Rendus.*

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, June 20, 1861.

W. DE LA RUE, Esq., in the Chair.

Two Papers by Mr. VERNON HARCOURT were read "*On the Peroxides of the Alkaline Metals*," and "*On the Estimation of Nitric Acid*," and one by Dr. MARCET, "*On the Constituents of Gastric Juice*."

Dr. ROSCOE then delivered the following discourse "*On the Application of the Induction Coil to Steinheil's Apparatus for Spectrum Analysis*":—

It is not possible for me this evening to deliver a formal discourse on the subject of the application of spectrum observations to chemical analysis, because the time which has elapsed since the first discovery of this new mode of detecting the elementary bodies is so short, that we are as yet incompletely acquainted with either the power or the capabilities of the valuable instrument which we propose to employ; but I do not know that I need apologise to the Fellows of the Chemical Society for bringing before their notice any new facts, however incomplete, that may at all serve to illustrate the progress of this new and most interesting branch of our science.

It would be unfitting were I here to enter into details or to give the result of the application of the method of spectrum analysis to the detection of the metals of the alkalies and alkaline earths. All of us must have read the important papers of Bunsen and Kirchhoff with a great deal of interest. What I propose to do this evening is to lay before you the method which the German professors intend to employ for the purpose of applying this method of research, not merely to the metals of the alkalies, or the alkaline earths, but to all the metals—we may hope so—and I think our hope is well-founded, and to the non-metallic bodies as well.

Before, however, I pass to the main subject, there are one or two points connected with the application of this method for the detection of the alkalies and alkaline earths, which, as they are of general importance, it may be well to mention.

In the first place, the property which incandescent bodies possess of giving spectra consisting of bright lines depends upon the substance being in the gaseous form. The spectra of luminous gases are broken; those of luminous solid bodies are continuous. In the gaseous state a heated body between certain given limits emits only a few kinds of light of different degrees of refrangibility; whereas a solid body, when heated, emits, between the same limits, rays of every degree of refrangibility. In order, then, that the peculiar spectrum of any substance may be formed, the incandescent substance must be in a gaseous state. It must be exposed to a temperature at which the tension of its vapour is appreciable. The greater the amount of its incandescent vapour present in the unit of space—that is, the higher the temperature or the more volatile the substance, the

greater does the intensity of the spectrum appear. Kirchhoff and Bunsen showed in the first place in their paper to which I have referred, that all the volatile compounds of a given metal produce the same bright lines; that these bright lines may be taken as certain indications of the presence of the metal, and that from the absence of such bright lines we may conclude the absence of any salt of the metal which is volatile at the temperature of the experiment.

They likewise showed that the range of the temperature within which these bright lines produced by any given metal remained constant is extremely large. The lines are produced by heating a body by the flame of sulphur, of which the temperature is only 1820° ; and that produced by heating by the oxyhydrogen flame, in which the temperature is calculated to be above 8000° ; and that produced, also, when an electric spark is allowed to pass from the poles of the metal in question, the temperature of the electric spark being vastly higher than the oxyhydrogen flame. Through these very different ranges of temperature we find we get exactly the same character of lines, that these lines remain fixed and show exactly the same relative position at a high and at a low temperature; and with perhaps the exception of the combining weights, there is no other single property of the metallic elements which remains so constant and serves so well to distinguish the metallic bodies through so wide a range of temperature as this property of the production of certain bright lines in their spectra;—this power of the vapour of metallic compounds to give off certain rays of a definite refrangibility. On the extraordinary delicacy of this investigation and the unlimited field which is opened out by inquiry, I need not now speak. The apparatus which was figured in Bunsen and Kirchhoff's paper has been superseded since that time by one more convenient, and which is in many respects more useful. I have here several of the apparatus—one made in Germany, one kindly lent by Dr. Faraday, and one made on the model of this by Mr. Ladd. These two instruments are made by Steinholt, of Munich. They possess the advantage of being first rate pieces of optical workmanship, the Munich prisms being of world-wide celebrity; and then they possess the other advantages of being more easily moveable and being more firmly fixed. The heavy iron stand renders the thing more secure. The great advantage, however, of this mode of arrangement is the fact that we can use two spectra from two flames; for the back of the slit carries a small prism, which is placed over the upper half of the slit in such a position that the light from one flame, which we place at the side, can fall upon the small prism and be refracted through the prism in this direction parallel to the axis of this tube; so that in that way we get one image from the flame on the right, which passes through the upper half of the slit, and is seen in the lower half of the field here, because this telescope reverses it; and we get, in the second place, the light from the lamp on the left, also, passing into the telescope and through the lower uncovered half of the slit. This is particularly useful, of course, when we have to detect any given substance. If we are looking for lithium, for instance, and are not sure of its presence, we have but to place a piece of a lithium compound in, and we at once see whether the two lines exactly coincide. If they do not, we know there is no lithium present. If the coincidence is perfect, if the red line passes at once all through, then we are quite sure that lithium is present in the body which is undergoing investigation. These apparatus, however, are very useful, inasmuch as the breadth of the slit can be altered at pleasure, and, owing to the accurate workmanship, we can see the double line "D" of the solar spectrum, and the soda line is easily distinguished as double, and we may also see the lines of which I shall have myself afterwards to speak.

In referring to these diagrams of the spectra of the

alkalies and alkaline earths, I would only remark that these diagrams are given by Kirchhoff and Bunsen, not as accurate representations of what we absolutely see with a good instrument when we pass one of these substances in the flame. Bunsen distinctly states in his paper that these lines were drawn so that only the most distinct lines should be seen. These are not intended to give the absolute representation, but to give such a representation as may be most easily applicable for the purpose of chemical analysis. If we were to represent all the lines which are seen, it would be much more complicated. And Bunsen very wisely thought it would be best to take such a breadth of slip that only the most important lines of the solar spectrum, and in the spectra of the alkaline metals, should be visible. I ought also to mention that Bunsen and Kirchhoff were by no means the first to observe the particular lines of metals. In their paper they begin by saying it is well known that certain bodies when placed in a flame produce spectra containing brightly-coloured bands. They were acquainted with the researches that had gone before. I have their own authority for saying this. In our own country, Talbot, and Wheatstone, and Dr. Miller observed closely the same thing; at all events, they observed some of the points which were noted by Bunsen and Kirchhoff, but to Bunsen and Kirchhoff belongs the honour of having first brought the subject to a definite issue, and made it a distinct and prominent branch of chemical analysis. The most interesting point so far as we yet know regarding the discoveries which have been made by this method, is, of course, the discovery of the two new alkaline metals. The first of these, cesium, was discovered in the springs of Dürkheim and Baden; and he found that most of the springs containing chloride of sodium also contained this cesium. It is very widely distributed in most springs containing chloride of sodium. In forty tons of this Dürkheim water, he obtained only five grammes of cesium. The atomic weight of cesium is 123.4. An extremely high atomic weight, and one which bears no relation to the atomic weights of the other alkaline metals, lithium, sodium, and potassium, which have a common difference of sixteen. The same common difference does not exist in the case of cesium. The chloride is soluble, and deliquesces in the air; the sulphate is anhydrous, and so soluble that one part of water at 20° Centigrade dissolves one and a half of the salt. Water at the same temperature dissolves only about one half its weight of the sodium sulphate. The nitrate crystallizes, and is also anhydrous; the salt crystallizes in the hexagonal system, and is therefore isomorphous. The carbonate of cesium is soluble in alcohol. In this respect it differs from carbonate of potassium. The double platinum salt is most insoluble in water. And this is the mode in which he first separates the two substances. The second new alkaline metal is rubidium, so called from the splendid red lines which it gives. It gives two red lines, splendid beyond anything we can see in the visible continuous spectrum of the luminous gas flame. They are of an extremely low degree of refrangibility. From these two magnificent lines, he gives it the name of rubidium. This gives all the reactions of potash, and forms the platinum salt; therefore it is extremely difficult to separate it from potassium and cesium. From what I heard the other day, we may hope soon to have a full account of Bunsen's researches on those two metals. I believe he has very nearly finished his paper upon the subject.

It is obvious that only the spectra of such can be examined by means of the colourless gas flame as are volatile at the temperature of the flame. If the bead of the calcium salt remains long within the flame, it loses its red tinge, which calcium gives it, and we see no longer the characteristic spectrum of the calcium. This is not because the calcium salt is volatilised, but because it has become basic, and the basic salt is not volatile at the temperature of the flame. You will see also that the

magnesium salts are not included in the diagram of the alkalis and alkaline earths, because the magnesium salts are either non-volatile at the temperature of the flame, or are very easily decomposed. There are, indeed, a few salts of the heavy metals, such as the salts of copper, which tinge the flame, and which, therefore, give the spectrum peculiar to these metals; but by far the greater number of the metallic salts are non-volatile at the temperature of the flame, and require peculiar means of producing the incandescent vapour of them, in order to arrive at the result. By far the best and most generally useful manner of obtaining the characteristic spectra of the metals, is by employing the electric spark, and to observe the spectrum thus produced. Every metal can thus be readily converted into vapour, and bright lines distinctly seen. The easiest mode of obtaining a succession of sparks in less time than the impression on the retina remains, is by employing a spark from a powerful induction coil, such as we have here; by placing the Leyden jar with the outer coating in connection with one of the wires of the induction coil, and the inner coating in connection with the other wire, we obtain a much brighter spark than we otherwise should obtain. Masson has shown that the luminosity of the electric spark is in proportion to the heat, and the heat is dependent on the amount of electricity which passes from time to time; so that the larger the jar, and consequently the greater the amount of electricity which is discharged, the greater will be the amount of heat, and the greater will be the amount of light produced. In this way we can easily obtain a bright spark. [Experimenting.] This we can use, as I shall explain, for the purpose of seeing the spectra produced by the various metals. Wollaston, who first observed the dark lines in the solar spectra, was also the first to observe the bright line in the electric spark. Fraunhofer, in the year 1814, also observed many of these lines. Mr. Faraday appears to have been the first to declare that the electric spark consisted only of ponderable matter, heated in the same manner, and according to the same laws, as the voltaic current heats and volatilizes a metallic wire. Wheatstone, in the year 1835, at one of the meetings of the British Association, was the first, I believe, who pointed out that the spectra produced by the sparks from the poles of different metals were dissimilar; he concluded that the electric spark results from the volatilization or ignition, not of combustion of ponderable matter of the poles. The tables which accompany Wheatstone's paper are very incomplete; and although he first pointed out the practicability of employing such a method for the detection of the metals, his observation seems to have attracted but little attention. "These differences," says Wheatstone, "between the spectra observed when a spark passes, are so obvious, that any one metal may be instantly distinguished from others by the appearance of its spark."—I suppose he means the spectrum of its spark,—“and we have here a means of discriminating more readily than by chemical examination, and which may be therefore employed for useful purposes;”—he means, of course, the electric spark from the machine. Masson was the next, in 1851, to observe the light-giving power of the electric spark, and to observe the spectra thus produced by the spark passing from poles of different metals; and in his paper “On the Luminosity of the Electric Spark,” he devotes a certain portion of space to the consideration of the spectra produced by the passage of the spark from the various metals.

It is to the Swedish philosopher Angstrom, who, in the year 1855, investigated the subject so thoroughly, that we are indebted for the greatest part of our knowledge. Angstrom was the first, previous to Bunsen and Kirchhoff, to draw attention to this important fact, that the spectrum we observe from the spark passing the platinum wires is really due to the super-position of two spectra, and that the one spectrum is produced by the ignition of the simple

particles of platinum, which are mechanically carried over from pole to pole, and heated to an enormously high temperature, and volatilized—that is, that one set of bright lines is owing to the ignition of the platinum, and the other set is owing to the ignition of particles of air through which the spark passes; and that, in fact, when we pass the spark through the air, we have two spectra, one consisting of the metallic spectrum, properly so-called, and the spectrum produced by the gas through which the metal passes. Now, these differences of brightness and distinctness of the lines produced by the air, is quite as great as the brightness and distinctness which the spectra of the metal itself possesses. In fact, we cannot distinguish between the two until we use some other mode of discrimination than I have yet spoken of. The effect of the different gases upon the light may be very well, and very easily and distinctly seen by passing the spark, at the same time, through the air from the platinum poles, and through hydrogen gas from two other platinum poles; we shall see that the colour of the hydrogen spark is totally different from the colour of the spark as it passes through the air. And if any gentleman afterwards will look through this, he will see a most beautiful phenomenon indeed. In the upper half of the field he will see the bright metal lines and the bright air lines; whether these are due to oxygen, nitrogen, or carbonic acid remains yet to be decided. Here is the bright spectrum of the platinum lines and the line of the air, and below is a most extraordinary spectrum, which is the hydrogen spectrum. We will notice the two lines of the platinum which pass through both, for those are extremely faint. First, there is one very splendid red line, which vies in intensity and in beauty with the red lithium line α , which, I dare say, most of you know. And this red line is produced by the hydrogen. Besides this, there is a bright band of green, which can be split up into a vast number of very thin and beautiful green rays. And, further on, there is one bright band of violet, which can also be split up into a great number of separate bands. Between these three bands there is nothing at all. The hydrogen spectrum is a most beautiful and a most peculiar one. The difference of the colour of the sparks is evident to all. The difference of the spectra I can only make visible to those who look through the apparatus.

In this way, then, by using different atmospheres for the same metal, we can tell which lines are due to the air, and which lines are due to the metal itself. We must, of course, in this way make use of the little prism. The light which will come from the hydrogen spark will be refracted through the prism to the upper half of this tube, and the light which comes from the air and the platinum will pass directly through the open slit below into the telescope. Without this arrangement of the little prism on the end of the slit, this experiment would be perfectly impossible. We could not detect which were the platinum lines and which were the lines of the air. I will now pass a spark through, and you will see the red spark of hydrogen, and the blue spark which is shown by the α r. [Experimenting.] There is, however, another way by means of which we may know which are the lines of the metal proper, and which are the lines of the gas; and this of course we can tell by using two sets of poles of different metals. The air sparks pass through both these series of poles, and if we take away this small vessel which contains the hydrogen, and replace it by a similar arrangement to this, which contains, however, instead of platinum poles, poles of iron or copper, we shall then see that certain lines go all through from the top to the bottom; those are the lines of the air, and the lines which are only seen in the upper half of the field are the platinum lines proper; and those lines only seen in the lower half of the field are the copper lines proper, and in this way it is extremely easy to detect the presence of any of those metals. Just as in the case of the

alkalies and the alkaline earths, it is not necessary for the purpose of chemical analysis to have a complete representation of the whole lines, we only need to seek out two or three of the most characteristic and most distinct. Here, in the case of these lines of the metals, we have not to do with one or two lines, as we have to do in the case of sodium,—we have to do with hundreds and thousands, and we only need to look for the most distinct; we only need to distinguish certain lines as always occurring when copper is present, and certain others as always occurring when we have platinum present, and to get these distinctly, and to look for these lines when we want to know whether copper is present. That is the way in which it is done. If we have poles of platinum or poles of iron, we get the same result. If we have, for instance, two sets of platinum poles, we get, of course, the platinum spectrum and the air spectrum. If, then, we want to get the iron spectrum, we only need to moisten the surface of the platinum poles with a little solution of iron—I believe any solution of iron will do,—and we then get at once the bright iron lines. So with any of the metals. In the same way we can obtain spectra of the other metals; and in this way Kirchhoff distinguished, with the greatest ease, between yttrium, erbium, and terbium, lanthanum, didymium, and cerium. Each of these metals possesses distinct and characteristic lines, which are easily recognised and distinguished.

To what extent this method will prove applicable for the purpose of detecting the different salts of the different metals, remains still to be seen. You must remember that I bring these matters before you as mere beginnings of what is, I hope, yet to be done. Of course, very little has yet been done; but still I thought it might be interesting to the Fellows of the Society if I brought before them the method by which these ends may be accomplished. Of the details of the method, of course it is quite impossible for me or any one else to say anything. It would seem, however, that this method, I have just stated, like all other methods that are worth anything, has its difficulties. We often find, on placing two solutions of two metals on the poles, or when we take poles of certain compound metals, that we see only the spectrum characteristic of one. For instance, if we take German silver we sometimes get only the spectrum of one of the constituents of the alloy. This, however, is a difficulty of detail which can easily be overcome. It is not, however, quite so simple as electrolytic decomposition. As in the case of decomposition by the current in the wet way, we find that one metal is deposited before another. This subject is, however, being worked out by our German friends, and we may hope to hear, before long, something more about it.

We will now pass on from this consideration of the power of incandescent gases to give off light of a certain definite degree of refrangibility, or to furnish spectra consisting of certain fixed lines, to the remarkable consequence which Kirchhoff has deduced—that these luminous gases must also absorb light of the same degree of refrangibility as that which they emit. This proposition is the application to the vibrations which we call light of the celebrated statement that a body absorbs all those series of oscillations which it can propagate. Thus, if we sound a certain note in the neighbourhood of a piano, we know that only the corresponding note in the piano vibrates. This proposition Kirchhoff has applied to light; and he not only showed theoretically that this was true, but he confirmed his conclusions by experiment, and he showed that heated sodium vapour which has the power of giving off light only at the one refrangibility D, has also the power of absorbing that ray alone; and that the vapours of other metals, such as lithium, potassium, calcium, strontium, and so on, which have the power of giving off a particular kind of rays, has also the power of absorbing those same rays. If we place sodium vapour

before the slit of our apparatus, whilst the sodium flame is burning, we shall have the sodium light entirely cut off. If while the potassium flame is burning here we put potassium vapour before it, we get these two bright rays cut off. This can be very well seen by a little experiment which was first proposed by Mr. Crookes. He takes a hydrogen flame coloured by sodium, and holds before it a smaller flame coloured still more intensely with sodium. If you look at the hydrogen jet which contains the larger amount of ignited sodium vapour, or sodium vapour of a higher temperature, and, therefore, giving off more intensely the line D—if, I say, you look at that on the background of a light less intense, you see that the sodium flame is surrounded by a dark mantle which is caused by the absorption of the light which passes from the hydrogen flame through the light behind. Another way is to take a tube and put some metallic sodium in it, and then pass in an atmosphere of hydrogen, and after rendering it vacuum to seal it up; and then by gently warming it over a large flame, a tolerably large quantity of the sodium can be brought into a vaporous state at a temperature lower than that at which it attacks the gas. When this sodium tube is held before a piece of white paper and looked at by the yellow day-light we see that the tube appears perfectly black wherever there is any sodium vapour at all. That is to say, the sodium-light cannot pass through; it is absorbed by the sodium vapour, whereas, by the common gas-light it is perfectly transparent.

Kirchhoff found in the solar spectrum lines which coincided exactly when allowed to fall simultaneously into his telescope. That is to say, when the solar light fell through the bottom half, or the uncovered part of the slit, and when the light from a metallic spectrum—the spectrum of iron, for instance,—was allowed to fall into the upper part of the slit, he found that certain lines in the sun corresponded with every line which the iron gave—that there was no iron line which had not its corresponding and coincident line in the spectrum of the sun. The bright iron line and the bright solar line corresponded perfectly. This was found to be the case not only with iron, but with chromium, and nickel, and magnesium. I have here a representation of a drawing by Kirchhoff of a very small portion of the solar spectrum—and it is allowed to be a very beautiful one indeed. It is a chromo-lithograph, but made with tinted inks instead of coloured inks to show the different darkness and the different tints and shadings of the lines on the solar spectrum. This shows the different lines of chromium and magnesium from “E” to “b.” This is “E” [pointing] in the solar spectrum; this is “b” [pointing] so that the whole of these lines occur in the solar spectrum as represented here. Here you see a great number of lines; of which some are iron, some chromium, and some magnesium, and these show the coincidence of the iron lines. This line is a very dark one, and is one of the most easily visible lines in the solar group. It is represented in the map, and marked as little “b.” This is found to be magnesium, and there is also another magnesium line as well. This then is the representation of a very small portion of the solar spectrum as yet given by Kirchhoff. This is only a very small piece of the map which he is about to publish.

In justice to Angstrom we must acknowledge that he came very close upon the explanation given by Kirchhoff. “The analogy,” says Angstrom, “between the two spectra, the metal and the solar, may be more or less complete, when we consider the details. When taken altogether, they produce the impression of one being the reverse of the other. I am, therefore, convinced that the cause of the lines of the solar spectrum involves that of the bright electric lines.” He supposed that it was, and Kirchhoff thought it was. I think the difference between these two will be understood by all. Kirchhoff proved it, and Angstrom suggested that it might be so.

Amongst one or two of the large number of interesting

fields which this has opened out may be mentioned the fact of the experiments and observations which have been made by Sir David Brewster and Dr. Gladstone regarding the appearance of certain other lines in the solar spectrum, lines which are seen under peculiar circumstances, either when the sun is low or when it is high. I am sorry Dr. Gladstone is not in the room. I believe at this moment he is at the Royal Society, giving an account of this same thing. Sir David Brewster found that certain dark lines or bands appeared in the solar spectrum when the sun was low; and these are represented in No. 2 in this diagram—the first red line representing the ordinary solar line; and the lines No. 2 are marked in the second spectrum red, in order to distinguish them from the other dark lines, so as to show that there has been a difference observed. Now, then, Angstrom drew maps of the bright lines which he noticed when the electric spark passed through the air. Now, it is curious that those two sets of experiments, made at different times quite independently of one another, seem to agree in every form. In this way we see very clearly—and I do not know that Dr. Gladstone was aware of this—that Angstrom's bright atmospheric lines correspond very nearly—as nearly as you can expect under such circumstances—with the dark bands observed in the actual observations made by Brewster and Gladstone. Here you see, for instance, between small "b" and "G" there is the same number of bands in both series, and you see the general character of these is the same, so that it appears then that in the atmosphere, even at this ordinary temperature, thick bands or thick columns of gas absorb the same kind of rays which they give off. That seems to be not only true, then, for high temperatures, but it would appear that it is also true of low temperatures as well. That is to say, that at low temperatures certain substances have the power of absorbing the same rays which, at the higher temperatures, they have the power of giving off.

(To be continued.)

NOTICES OF PATENTS.

An Improved Apparatus for Carburing Gas for Gas-lighting. E. C. SHEPARD.

THE specification refers to a particular method of effecting the impregnation of lighting gas with naphtha vapour by the use of an apparatus therein described. A single carburator serves either one or several burners, being placed near or remote as may be most convenient, but preference is given to an arrangement whereby each jet of gas is provided with a naphthalizing apparatus placed near the burner. By means of a float the saturation of the gas by the fluid, hydrocarbon is rendered tolerably constant, notwithstanding the varying depth of this latter in the reservoir, and diaphragms of wire-gauze are employed to ensure uniformity of impregnation.

A suggestion for increasing the illuminating power of coal-gas by the admixture of naphtha vapour was practically and successfully carried out many years since by the late Mr. Charles B. Mansfield, whose enthusiasm in this direction of chemical study led unfortunately to his falling a victim whilst being engaged in the distillation of a large quantity of coal-tar naphtha. Since the period of Mr. Mansfield's original experiments, efforts have from time to time been made with the view of converting the suggestion into a scheme of practical utility. More recently the naphthalizing apparatus has been adapted to the gas-lamps in Moorgate Street, London, and we are assured, as the result of this trial, that, whilst the photogenic value of the light has suffered no diminution, a saving in cost has been effected. The great demand which has lately arisen for the lighter products of coal-tar naphtha to be used in the preparation of the purple dyes

will probably restrict the application to these purposes of the less volatile hydrocarbons, (toluol, &c.), which may nevertheless be well-fitted for employment as auxiliaries in gas lighting.

Treating Lava and other Volcanic Substances in order to fit them for Employment in Certain Arts and Manufactures. R. A. BROOMAN. A Communication. (Not proceeded with.)

THE inventor contemplates the utilization of lava, puzzolano, and other volcanic substances by melting them alone or with the addition of any suitable flux, such as red-lead, borax, lime, potash, or soda; and, accordingly as the heated materials are fluid or pasty, they may be cast or moulded into objects of various shapes; for instance, slabs for tables and mantelpieces, architectural ornaments, tiles, and blocks suitable for building. The specification describes the several mechanical operations to which it is advisable to submit the volcanic matters prior to their treatment in the furnace. Also, it asserts a claim for the employment of the same substances, in the state of powder, for the purposes of a glaze in the manufacture of pottery.

In judging as to the expediency of employing the substances of volcanic origin enumerated in the specification, we are reminded of the oft-repeated attempts to utilize the abundant sources of ready-fused material offered by the blast furnace, and in so many other similar metallurgical operations. Blocks of slag have been used for rough building purposes, and not long since a Company was established (with what success we know not) for the purpose of undertaking the manufacture of useful articles from the unsightly heaps of slag which occupy and disfigure so many acres of land in the neighbourhood of our copper and iron-works.

An Improved Lubricating Grease or Paste for Railway Wheels and all Kinds of Machinery. F. W. SERROTT, Hanover Cottage, Walworth.

THIS invention consists in the use of micaceous ore combined, by preference, with oleaginous or fatty matters, or both together, or other suitable substances, in the manufacture of grease or paste for lubricating the axles of locomotives and carriages, the bearings of engines, and working parts of machinery. The micaceous ore which is employed for this purpose is that well known as a species or variety of hæmatite iron ore found in various parts of this kingdom. It is soft, greasy, and friable in its nature, and forms a good base or principal component part of a lubricant when combined with oleaginous or fatty matters, or both. Being of considerable specific gravity, it has more body or substance than many other materials used for lubricating purposes, and does not so readily run or melt from the action of the sun or continuous working of the machinery to which it is applied, and, therefore, lasts longer, and when properly prepared and mixed with oleaginous or fatty matters will not injure the axles, shafts, axles, or bearings of any engine, carriage, or machine. Most chemists are aware of the apparent greasy properties of some hæmatites. These are here utilised in an ingenious manner; but Mr. Serrott is mistaken if he thinks his idea possesses much novelty. It has been employed frequently for lubricating the bearings of axles and machinery which have to be subjected to great heat.

Brewing Malt Liquors. H. MEDLOCK, Great Marlborough Street, London.

IN this invention the inventor soaks the hops in water, the temperature of which must not be high enough to coagulate the albuminous matter; 160° F. will generally

be found to be the maximum temperature which can be safely used. He prefers a heat from about 120° to 150° . During this process the volatile aromatic substance is produced and dissolves in the water. The albuminous matter is also dissolved, the heat not being sufficient to coagulate it. He then strains the water from the hops, removing by this means the volatile aromatic substance, and albuminous matter. After the hops have been so treated, he boils them with the wort in the usual way, by which he effects the combination of the resinous matter with the gluten of the malt, the absence of the albuminous matter greatly facilitating the extraction of the former, and of the bitter flavour of the hops. The combination of the resinous matter of the hops and the gluten of the malt subsides on cooling the wort, and may be separated from it in the usual way. When the wort has sufficiently cooled, and before or shortly after the fermentation begins, he adds to it the cooled solution of the volatile aromatic substance, which, with the albuminous matter, has been extracted. Mr. Medlock's patent was not proceeded with, —not, we should imagine, from any want of success in carrying it out, for the plan seems to be highly original and ingenious.

Improvements in the Manufacture of Sulphurous Acid, Sulphites, and Bi-Sulphites, and Sulphuric Acid, and in the Application of one of the Products of such Manufacture to the Bleaching of Textile, Animal, and Vegetable Substances. C. A. DREVIT, Rouen. (Not proceeded with.)

To produce sulphurous acid the inventor introduces through an opening into a cast iron retort a quantity of sulphur, which he melts by heat from a furnace; when it is inflamed, the retort is hermetically closed, and a current of air is caused to enter through a pipe through a perforated plate by means of a bellows. The sulphurous acid thus produced passes through a receptacle divided into compartments (in which the flower of sulphur deposits), and is cooled in a leaden worm, whence it is passed into the receptacle with compartments containing either water or a solution of a base or of a carbonate; the free nitrogen and carbonic acid gas (when he employs a carbonate) escape into the atmosphere through an opening.

Paper Pulp. W. CLARK, Chancery Lane, London. A Communication. (Not proceeded with.)

This invention relates to the manufacture of a new pulp for cardboard with tanners' waste, such as spent tan and wazec (sea-weed), which are submitted to various preparatory processes.

Preserving Animal Substances. W. CLARK, Chancery Lane, London. A Communication.

This invention relates to the preservation of animal substances by the employment of talc, laminated or scaly, or of other kinds, and generally in the employment of any other inorganic matters of insoluble nature, or of argillaceous earths, serving as ceramic bodies, or sulphate of baryta, carbonate of lime, and others. All these substances are reduced to powder to be applied on the animal substances, which are preserved in a fresh state, and ultimately brought to a proper state of desiccation. We are at a loss to conceive what possible effect smearing an animal substance over with an earthy powder could have upon its preservation from decay. If the animal matter can be kept perfectly dry it will not alter. If, however, it gets damp, the superficial coating of a perfectly indifferent body cannot save it.

Grants of Provisional Protection for Six Months.

1671. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in apparatus for manufacturing and bottling aerated liquids."—A communication from Messrs. Auguste Mondolot and Jean Alphonse Mondolot, Paris.

1817. Robert Mushet, Coleford, Gloucestershire, "Improvements in the manufacture of cast steel."

1869. Edward Haefely, Kearsley, Lancashire, "Improvements in extracting copper from its ores."

1893. Wentworth Lascelles Scott, Bayswater, Middlesex, "Improvements in preparing red, purple, and certain other dyes."—Petitions recorded July 29, 1861.

1955. Alcidalis Auguste Romain Damoiseau, Paris, "Improvements in apparatus for drawing blood or other fluids from the human or animal body."

Notices to Proceed.

894. Charles Noyes Kernot, Gloucester House, West Cowes, Isle of Wight, and Martin Diedrich Rucker, Fenchurch Street, London, "Improvements in the method of obtaining ammoniacal salts and other valuable products from liquors or substances containing ammonia, and for utilising the residuum."

925. Robert Charles Furley, Edinburgh, Mid Lothian, N.B., "Rendering pills tasteless by means of a coating of albumen."—Petitions recorded April 15, 1861.

948. Hugo Carstanjen, Cologne, "Improvements in the apparatus for, and method of, increasing the illuminating power of gas."

955. Richard Archibald Brooman, Fleet Street, London, "Improvements in producing photographic pictures."—A communication from Adolphe Fargier and Nicolas Charavet, Paris.—Petitions recorded April 18, 1861.

1076. William Edward Newton, Chancery Lane, London, "Improvements in desiccating and torrefying farinaceous and other substances."—A communication from Francis Huckins, Roxbury, Norfolk, Massachusetts, U.S.—Petition recorded April 29, 1861.

CORRESPONDENCE.

White Gunpowder.

To the Editor of the CHEMICAL NEWS.

SIR,—I find some difficulty in reconciling the account given by Dr. Pohl, and that given by Mr. Hudson relative to the manufacture of white gunpowder. Dr. Pohl speaks of the ease and safety with which it may be manufactured; also of the impossibility of causing it to explode by rubbing between wood and metal, or between stones. And yet Mr. Hudson's assistant almost lost his eyesight by simply crushing some of the larger pieces with a spatula in a porcelain dish. If white gunpowder is to be prepared moist, what is to prevent it caking when dry? and, when caked, is it all to be broken up by hand because it will not bear friction? And suppose the white gunpowder made, dried, and pulverised, what is to prevent it caking again? I apprehend the difficulties and dangers in the manufacture of this material will be found greater than those who are practically unacquainted with the manufacture of gunpowder, black or white, imagine.

I am, &c.

J. M. C.

Antidote to the Phosphorus Disease.

To the Editor of the CHEMICAL NEWS.

SIR,—In reading the interesting account in the CHEMICAL NEWS of Aug. 17, of "The Origin and Progress of the Phosphorus and Match Manufactures," by Mr. Gore, my attention was drawn to that part wherein he states that the manufacture of matches "is not without some serious drawbacks;

it produces a most painful disease, which attacks the jaw-bones of the work-people, and causes them to decay." This, of course, is caused by the inhalation of the poisonous garlic odour. Now, it occurs to me, that as arsenic (which also, when heated, emits a strong odour of garlic) has been found useful in malignant or cancerous ulcerations, especially of the skin of the face, in lupus, and Hospital gangrene, and as phosphorus is seen to produce a disease similar to the above, might not the cautious use of arsenic as a prophylactic be tried in this formidable disease? Both give out a garlic odour. The one cures a disease, the other, carefully administered, has been found to cure diseases something akin to the one produced by phosphorus. I am not a believer in Homœopathy, but there is something here, to my mind, like *similia similibus curantur*.

I read of a case lately, where a lady, for long a martyr to asthma, was induced to smoke a small quantity of arsenic with stramonium, beginning with about half-a-grain. After persisting in its use for some time to her great relief, she experienced a complete immunity from the paroxysms of asthma, and much improvement of health. The only inconvenience she felt was, at first, some giddiness and tightness of the eyeballs, which gradually went off. The foregoing preamble brings me to this, viz.:—Might not those operatives who are exposed to the vapours arising from the match composition, and even those affected with disease already, smoke arsenical cigarettes, made of a safe and proper strength?—

I am, &c. HENRY DUNBAR, M.D.
Glasgow.

Preparation of Iodine.

To the Editor of the CHEMICAL NEWS.

SIR,—In the preparation of iodine, after the mother-liquor has been exhausted by the sulphuric acid and manganese, on testing with starch there still remains a considerable apparent quantity. Is there any means of separating it from the residue?—I am, &c.

A CONSTANT READER.

Chemical Notices from Foreign Sources.

I. GENERAL CHEMISTRY.

Reciprocal Relations of Atomic Weights.

Stas has just published (*Recherches sur les Rapports Réciproques des Poids Atomiques*, Bruxelles, 1860) the results of ten years' experiments, made with the view of testing the truth of Prout's theory—that the atomic weights of all simple bodies are divisible by that of hydrogen. These experiments, we are informed, have been conducted with a care and perseverance never surpassed in the history of experimental investigation, and the conclusion which Stas has arrived at is, that there is no common divisor for the equivalent weights of the simple bodies, and he expresses his conviction that Prout's law, and the later modification of Dumas, are untenable. The differences in the numbers Stas has obtained are not very great, but some of his conclusions are important. We shall return to the work soon for further details, and for the present only quote two or three of the corrected numbers:—

Potassium, 39.13 Silver, 107.943 Nitrogen, 14.041
Sodium, 23.05 Chlorine, 35.46 Sulphur, 16.0371

Hubasse.—A beautiful red-coloured stone has lately been popular in Paris, and brought high prices to the jewellers. Schaffgotsch (*Polytech. Notizblatt.*, Bd. xviii. s. 10) examined a specimen, and found that, when placed in ammonia, it soon lost its beautiful colour, and became a simple piece of rock crystal. It was, indeed, a specimen of quartz, the minute fissures in which had apparently been filled with a solution of carmine.

Preparation of Hydrate of Baryta.—Müller recommends (*Journ. für Prakt. Chem.*, Bd. lxxxii. s. 52) the preparation of hydrate of baryta from sulphide of barium and oxide of zinc. The last is slightly soluble in baryta water, but it can be easily removed by a little sulphide of barium reserved for the purpose. Any traces of sulphur remaining may be got rid of by adding a little sulphate of copper. If any thionic acid is formed during the operation, it may be got rid of by calcining the baryta obtained with a little nitrate of baryta.

II. ORGANIC CHEMISTRY.

Oil of Cajeput.—Schmidt publishes (*Trans. Royal Soc. Edin.*, vol. xxii., p. 360) some researches on cajeput oil. His analysis agrees with that of Blanchet and Sell— $C_{20}H_{16} + 2HO$,—but instead of the name *Dadylo* given to it by these chemists, Schmidt proposes to call the oil the *Bihydrate of Cajeputene*. It boils at $175^{\circ}C$, becomes acid in the air, especially in the presence of alkalis; hydrochloric acid separates a resinous body. When heated to boiling and some sulphuric acid is added gradually, the oil becomes coloured, and between 170° and 175° a liquid distils having the composition $C_{20}H_{16} + HO$, which the author considers the *monohydrate of cajeputene*. The vapour density of this body is 5.02, and corresponds to 4 volumes, which, as it contains an odd atom of oxygen, is an exceptional case. Repeatedly distilled with anhydrous phosphoric acid this monohydrate forms a series of hydrocarbons of different degrees of volatility. The first *cajeputene* $C_{20}H_{16}$ boils between 160° and 165° , and is a colourless liquid, smelling like the hyacinth: it is insoluble in alcohol, but soluble in ether and oil of turpentine. Its sp. gr. at 15° is .850; vapour-density 4.717. Between 176° and 178° , *Isocajeputene* passes; like the former it is insoluble in alcohol, but soluble in all proportions in ether and oil of turpentine.

Paracajeputene comes over about 316° : it is a viscid fluid of a lemon-yellow colour, slightly fluorescent: unlike the two former, it is insoluble in turpentine; in the air it soon changes to a red resin.

Iodine acts on these hydrocarbons when heated, and hydrogen is evolved. Bromine thickens cajeputene and isocajeputene, which are converted into a friable resin by a mixture of sulphuric and nitric acids. Hydrochloric acid colours all three violet, but does not form a definite compound with them. When, however, oil of cajeput is shaken with hydrochloric acid an unstable compound $C_{20}H_{16} + 2 HCl$ is formed, which heated to 160° parts with one equivalent of the acid.

Bichloride of Cajeputene is formed when a current of chlorine is passed through a solution of cajeputene in nitric acid. It is a heavy crystallizable oil with an agreeable smell; it decomposes on distillation, and detonates with nitrate of silver.

The author has also obtained a bromide, $C_{20}H_{16} + 4Br$, and a hydriodate, $C_{20}H_{16} + HI$.

ANSWERS TO CORRESPONDENTS.

* * All Editorial Communications are to be addressed to the EDITOR; and Advertisements and Business Communications to the PUBLISHER at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

* * In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Lightning.—Connect the lightning-rod with the gas or water-pipes of your house.

F. S. L.—We have had some experience in the matter you speak of, and have no hesitation in saying that you will not succeed unless you use a stronger acid. There ought to be at least 1 part of sulphuric acid to 4 of water, by measure.

Brratum.—Article "On Rosolic Acid and the Rosolates." No. 39, p. 90. The Author's name is erroneously printed "Jouvin;" it should be "Jourdin."

THE CHEMICAL NEWS.

VOL. IV. No. 92.—September 7, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On a New Sulphide of Chromium, by T. L. PHIPSON, Phil. Nat. Doct., Brussels University, Member of the Chemical Society of Paris, &c.

In endeavouring to form sulphochromic acid, Cr_2S_3 , corresponding to chromic acid, CrO_3 , I discovered the existence of a new sulphide of chromium, Cr_2S_7 , which was obtained in the following manner:—

Bichromate of potash is neutralised, and then supersaturated with ammonia; it is then placed in a three-bulb tube and a rapid current of sulphuretted hydrogen is passed through it for a long time. The liquid, sooner or later, becomes dark and cloudy, and then a brown precipitate is formed. This precipitate is Cr_2S_7 . When it is collected upon a filter, the liquid which passes through is dichroic; it is reddish-brown, with green reflections, and probably contains an alkaline sulphochromate. However, upon adding diluted hydrochloric acid to this liquid, care being taken not to saturate completely, I could not obtain Cr_2S_3 (sulphochromic acid), but a new quantity of persulphide, Cr_2S_7 . Here are the analyses:—

I.	II.		
Body deposited during the passage of HS .	Body precipitated by the acid.	Mean.	Calculation for Cr_2S_7 .
Chromium 32.64	32.21	32.42	32.42
Sulphur . 67.36	67.79	67.58	67.58
100.00	100.00	100.00	100.00

This new body is not a mixture of sulphochromic acid and sulphur, $2\text{Cr}_2\text{S}_3 + \text{S}$, but a distinct and definite compound, Cr_2S_7 ; for sulphide of carbon cannot extract any sulphur from it. Besides, the following reactions tend, as well as its composition, to point out its true nature:—

Acids decompose it, with evolution of sulphuretted hydrogen and formation of a chromic salt, whilst sulphur is liberated,—



With caustic soda, chromic oxide, bisulphide of sodium (which gives to the liquid a yellow colour), and sulphur, are formed,—



Persulphide of chromium is easily decomposed, and requires, therefore, to be isolated and dried promptly. In contact with water or damp air it, sooner or later, forms chromic oxide, Cr_2O_3 , and sulphur. When heated it gives off sulphurous acid, and leaves a residue of peroxide of chromium, CrO_2 , of a brown colour, which, when heated more, evolves oxygen, and becomes Cr_2O_3 (green), with evolution of light. When washed and

dried it forms an amorphous powder, of a greenish-brown colour. Its composition renders very probable that of perchromic acid, Cr_2O_7 , discovered by M. Barreswil.

In the dichroic liquid mentioned above, I fancy sulphochromic acid, Cr_2S_3 , must be present, though I have not yet isolated this compound; for as soon as a dilute acid is added to the liquid, the sulphochromic acid set at liberty immediately combines with more sulphur to form Cr_2S_7 , which is precipitated.

When, instead of passing HS into a chromic solution supersaturated with ammonia, this gas is passed into bichromate of potash supersaturated with soda, a very small quantity of brown precipitate is formed, and soon disappears, giving place to a magnificent green precipitate, which upon analysis gave:—

	Found.	Calculated for $\text{Cr}_2\text{O}_3 + 8\text{H}_2\text{O}$.
Chromic oxide. . .	51.61	51.81
Water	48.39	48.19
	100.00	100.00

It is, therefore, the common green hydrate of chromic oxide.

The persulphide of chromium, Cr_2S_7 , is not a sulphide; it does not dissolve in alkaline sulphides, and by this circumstance coincides with certain theoretical data which I shall make known at some future period.

Chemical Researches on the Assay of Silver by the Moist Way, by G. J. MULDER, Math. Mag. Phil. Nat. Medicinæ, Artis Obstetriciæ et Pharmaceuticæ Doctor, Professor of Chemistry at the University of Utrecht, Grand Cross of the Order of the Netherlands Lion; of the Luxembourg Order of the Couronne de Chêne; of the Swedish Orders of Wasa and Polar Star, &c., &c.

(Continued from page 101.)

When equal quantities of sulphate of potassa and chloride of magnesium are dissolved in water and left to crystallise, there is first obtained $\text{SO}_3\text{K}_2\text{O}$, afterwards a double salt $\text{K}_2\text{O} + \text{SO}_3\text{MgO}$, next MgCl , and finally SO_3MgO . (*Berthollet, Stat. Chimie*, i. p. 100.) A similarly varying separation of potassa and magnesia salts may be observed on evaporation of the brine of sea-water. A great many instances are known relating to the solubility of salts, which otherwise are insoluble, in case other salts are present at the same time in the same liquid. Two solutions, the one containing 1 per cent. of carbonate of soda, the other containing 1 per cent. of sulphate of magnesia, do not form a precipitate when added to each other, but a precipitate is immediately formed in case the liquids contain in solution $1\frac{1}{2}$ or 2 per cent. of the said salts.

The magnesia alba which is formed in this case remains soluble in 200 parts of water, while it requires

otherwise 2500 parts of water to become dissolved (*Pyfe*).

If two solutions, each of which contain upon 100 parts of water the first $7\frac{1}{2}$ parts of chloride of calcium, and the other an equal quantity of sulphate of magnesia, are added together, no precipitate will be formed in these 200 parts of water; but the 5.75 parts of gypsum which are here generated, require usually for every 1 part of gypsum 438 parts of water for solution (*Anthon*). (*Brandes in Schweigger's Journ., N. R., Bd. 13, s. 158*).

If a solution of gypsum is added to a solution of a salt of strontia, sulphate of strontia is produced. That precipitate gets slowly dissolved on addition of common salt. This does not take place with sulphate of baryta. Common salt, therefore, dissolves sulphate of strontia.

Gypsum, which requires, according to *Anthon*, 438 parts of water for solution, is completely dissolved in 122 parts of a saturated solution of common salt in water. Rock salt often contains gypsum. Gypsum is also soluble in a solution of acetate of soda, and there is no reason to suppose, that in this case a double salt is formed. If sufficiently strong solutions of sulphate of soda and of acetate of lime in water are added together, and afterwards divided into two equal parts, then it will be seen that on addition of common salt to one of the two parts, that that portion to which common salt was added will become clear, the other remains turbid from the first formed precipitate of sulphate of lime. Lime water, which has become turbid by the action of carbonic acid, becomes, according to *Guyton*, clear on addition of a solution of chloride of potassium or of sulphate of potassa (*Berthollet, Stat. Chim. i., p. 103*).

The solutions of the salts of the peroxide of iron are precipitated by the carbonates of the alkalies, but such does not happen when tartaric acid is, at the same time, present in the solution. Sugar produces the same effect; and although it be supposed for the first case, that the tartrate of iron which gets formed cannot become decomposed by alkalies or their carbonates—which supposition, however, is not in concordance with general chemical rules, even then it is in every case still an anomaly to chemical affinities, that sugar should prevent the precipitation of salts of peroxide of iron. The salts of oxide of copper behave similarly under the same conditions. Oxalic acid behaves in a curious way with salts of peroxide of iron and the so-called yellow prussiate of potassa, ferrocyanide of potassium. In case oxalic acid is present, Prussian blue is also formed, but this, instead of being precipitated, remains in solution (blue ink). There can be no doubt here of real chemical action, since the new compound is equally well formed, but remains in solution.

These, then, are a few instances of what we call physical force exercising influence upon the chemical formation of substances; for there can be no doubt whatever, that these substances do not exist as such in the solutions, when, in accordance with their nature, they ought to have been precipitated long before. The idea we ought to have about these phenomena may be explained shortly, according to *Berthollet*, in the following lines:—

When two salts which are in solution decompose each other mutually, then there are not formed two, but four salts, which do exist in greater or less quantity in the liquid, just as long a time as is required to restore the equilibrium between chemical and physical action. If, for instance, a solution of chloride of calcium and a solution of sulphate of magnesia are mixed together, then there will soon be two other salts in the solution—viz., sulphate of lime and chloride of magnesium. Whether a precipitate

is formed in this case, does not only depend upon the insolubility, or upon the more difficult solubility, of one of the salts in water—in this case, for instance, the gypsum—for we have here not simply water; it entirely depends upon the insolubility of that salt in the saline mixture. In case the substance is insoluble therein, then it becomes entirely thrown down; if it is only slightly soluble therein, then much of it becomes thrown down; if, on the contrary, it is very soluble therein, then none, or only a little, becomes thrown down. A very marked example thereof is exhibited by sulphate of potassa and chloride of calcium; the precipitate, (of sulphate of lime,) which they produce is by far smaller than is in relation to the solubility of gypsum in water. That which remains in solution of the four salts—viz., sulphate of potassa, chloride of calcium, sulphate of lime, and chloride of potassium, is in the state of physico-chemical equilibrium.

Wherever, therefore, we have to do with salts, or generally with substances of different nature which are dissolved together in a liquid, there we have to do more or less with influences similar to those which we have mentioned in the foregoing instances. The experiment of determining silver in the moist way is only excluded from such influences for him, who thinks chloride of silver to be perfectly insoluble, and who despises the great facts brought to light by the master hand of *Berthollet*. He, however, who is deeply penetrated by the truth that probably the presence of some substance, but certainly that of many substances, may exercise its influence on the chemical separation of another substance, will surely investigate with great care if such an influence does exist, and what may be its value in every peculiar case.

As soon as we have to do with pure silver in estimating that metal by the moist way, the presence of these substances which are unavoidably necessary, and which do not seem to exercise any chemical action at all, can, however, be of some notice; but in case we have to do with impure silver, then the influence of these other foreign substances can become so great, as to interfere largely with the results.

The first part of this memoir treats on the investigation of the influence of these foreign substances.

2. **Chloride of Silver.**—The formation of the chloride of silver forms the chief and principal point in the assay of silver by the moist way; and although it may not be our intention to treat here of the generally well-known properties of this substance, still there are some of its properties which must be mentioned if we wish to become thoroughly acquainted with the whole method of assaying silver by the moist way, and of that which has any influence thereupon.

Hydrochloric acid and soluble chlorides precipitate from a soluble salt of silver, white chloride of silver in the form of a so-called curdy precipitate. It is a loosely coherent, flocculent precipitate, which by shaking, or by being heated, collects together the smallest particles of the precipitate and renders the liquid quite clear.

There are only a few substances which yield so spongy a precipitate as chloride of silver. It appears as flocks, the particles of which retain, even after strong agitation of the liquid in which they float about, their coherence, and they even resume that coherence if it has been disturbed momentarily by a strong stirring of the liquid. The smaller particles of the precipitate which float about in the liquid, are taken up in that spongy mass more or less in the same way as the flocks of coagulated albumen render clear a turbid liquid. Application of heat produces movement in the liquid, and what-

ever produces motion in the liquid brings the smaller particles in contact with the more flocculent ones, whereby, while the liquid is rendered clear, the smaller particles are taken up by the larger.

The liquid, after shaking, does not always become perfectly clear; the operator has the faculty, and it is even optional with him, to render the liquid more or less clear. A more coarsely flocculent precipitate is obtained, when after the first addition of the solution of common salt to the silver salt, some of the latter is still left unprecipitated, if then a fresh portion of the solution of common salt is added, and the liquid briskly shaken, the first formed more coarse precipitate takes up very readily the large number of smaller particles of chloride of silver obtained after the second addition of the solution of common salt, and the liquid is rendered always perfectly clear. I shall have occasion to return to this fact, which is of very great importance for the estimation of silver in the moist way.

Chloride of silver is soon discoloured by the light of the sun, being rendered violet and heavy, whilst some chlorine is set free. According to Pohl (*Jahresb. von Liebig und Kopp*, 1851, s. 369) chloride of silver, which is precipitated at a temperature of from 60° to 80° C., is not discoloured by light for many days, if it remains in the liquid kept at that temperature. According to the same, discoloured chloride of silver would become, under these circumstances, somewhat more white, although not quite so white as when it was first formed. These observations, however, I have not found confirmed.

Wittstein (*Report. für die Pharm.* Bd. xxxvi. p. 170) assumes the existence of a subchloride, Cl Ag_2 , in the chloride of silver which is discoloured by light: he affirms that it is not attacked by nitric acid, and that it is decomposed by ammonia into chloride of silver, which is dissolved therein, and into metallic silver, which remains undissolved.

Dumas (*Ch. App. aux Arts*, tome iii. p. 653) is perfectly right when he says, that the quantity of subchloride which gets formed is very small, and the quantity of chlorine which is set free, therefore, very small also. According to Rose (*Analyt. Ch.* ii. s. 400), it is impossible to find any difference in the weight of the white and discoloured chloride of silver, when even a very sensible balance is used. A quantity of 1 gramme of pure silver was dissolved in nitric acid, and precipitated by a solution of common salt added in precisely sufficient quantity to obtain the point of chemical neutrality,—there was, therefore, neither excess of silver, nor excess of salt,—the bottle containing the precipitate was exposed to light, and sufficiently shaken from time to time, in order to discolour as much as possible all the particles of the chloride of silver. I did not find in the liquid afterwards a larger quantity of free chlorine than could be saturated by 6.25 milligrammes of silver. One gramme of silver forms 1.33 grammes of chloride of silver. The 6.25 milligrammes of silver required to saturate the chlorine which was set free in this experiment, represent only 2.06 milligrammes of chlorine; in case subchloride of silver only had been formed, the half of 0.33 gramme, that is 0.165 gramme of chlorine, should have been set free. In this experiment only $\frac{1}{8}$ chlorine of the chloride of silver had been set free. It is accordingly perfectly possible to weigh the difference, and therefore what Rose mentions is not quite correct.

The violet-coloured matter which is formed when chloride of silver is exposed to light is a mixture; part of it is perfectly soluble in liquid ammonia, while

another part of it gets decomposed by ammonia, of which part again is rendered insoluble, the latter being metallic powder.

Some chloride of silver, which had been precipitated from a solution of nitrate of silver by means of common salt, and which had been exposed for some time to the light of the day, was carefully freed from the supernatant liquid, and some liquid ammonia was poured over it. Nearly all of it got dissolved, a small portion of it only, after having been in contact with excess of strong liquid ammonia for some time, remained insoluble. This small portion exhibited the same colour as the powder of minutely-divided silver, as it is separated, for instance, by precipitation by formic acid; it did not possess any violet tinge, and therefore the violet tinge of discoloured chloride of silver cannot be attributed to this substance.

The metallic particles, which are insoluble in liquid ammonia, are of variable size, few of them are so large that they can be retained by a filter. They are soluble in nitric acid, and the solution is precipitated by a solution of common salt; these small particles are, therefore, metallic silver.

Besides these particles of metallic silver, violet coloured, flocculent chloride of silver, when dissolved in liquid ammonia, gives rise to the formation of a colourless liquid, which being supersaturated with nitric acid, yields again pure white chloride of silver, all the violet colour being gone. The explanation of these phenomena is the following:—Daylight in fact reduces a small portion of chloride of silver to subchloride; from Cl Ag is made a small portion of Cl Ag_2 , which latter has a violet colour. If to the Cl Ag_2 ammonia is added, or if ammonia is poured over it, it then yields a colourless solution, while metallic silver is separated, and from Cl Ag_2 there is made Cl Ag and Ag . The former is readily dissolved in ammonia, and can be reprecipitated again as white Cl Ag on addition of excess of nitric acid. The silver can be dissolved by nitric acid. Wittstein's observation, therefore, is quite correct. Even on being warmed with it, nitric acid does not change violet-coloured chloride of silver to white; chlorine water, however, produces this effect instantaneously. The reason why chloride of silver, which is so readily changed by the daylight into subchloride, is not totally changed into it, is entirely due to the chlorine which is set free. An excess of chlorine is an impediment to the formation of the violet material, a little chlorine prohibits its formation. This also is an interesting instance of the influence of mass on chemical affinity.

(To be continued.)

PHARMACY, TOXICOLOGY, &c.

On Coca, or Folha Sagrada—Erythroxyton Coca.

T. PECKOLT reports, in *Archiv. der Pharmacie*, ciii. pp. 39—42, on the following paper of Dr. Valdez:—

Coca is the leaf of a shrub, growing to the height of not over six feet, and cultivated almost exclusively in Peru and Bolivia, recently also in the Brazilian province of Alto Amazonas. At the time of discovery the plant was used only by the Incas and the royal family, and looked upon as the image of a deity. Nobody entered the enclosure or sanctuary where it was cultivated, without previously kneeling down as a sign of adoration. Sacrifices were believed not to be acceptable unless crowned with a twig of this shrub. Oracles would not

answer, and their prophecies were terrible, if coca was not chewed on interrogating them. The Indian found consolation with this divine plant in all misfortunes and when praying for happiness.

Gradually its use extended to the whole population, and it became an important commercial article. Shortly before the independence from Spain, Peru produced coca valued at 2,841,487 dollars, and the leaves circulated occasionally as money. The Indians, in using them, are thereby enabled to perform the hardest mining operations in the midst of deadly metallic exhalations, without resting, and without protection against the unfavourable influences of climate. They march hundreds of miles through deserts, and over steep mountains, subsisting solely on coca, and frequently carrying burdens over places impassable for beasts of burden. Some travellers ascribe this astonishing frugality and abstinence from food to the effect of habit, without remembering that the Indians are naturally very voracious, and lose their strength when abstaining from chewing the leaves.

Coca leaves have a faint and agreeably aromatic odour, particularly when chewed; their taste is somewhat bitter and astringent, and colours the saliva green. They are strengthening, giving tone to the stomach, and are employed against intermittents, though with less decided results than cinchona. They are mixed with lime prepared from certain plants, and used like betel in the Indies. With it and a handful of roasted corn (maize), an Indian travels a hundred miles afoot, keeping pace with a travelling horse. Travels in the Andes are undertaken with an Indian, and it happens on hot days, that the horse is fatigued before the Indian.¹

It is a narrow leaf, little over an inch in length; there are two species, one 2, the other $1\frac{1}{4}$ inches in diameter (length?). The former is called *cacha*, and is superior and more esteemed than the commoner one, which is used only in cases of need and by the poorest people. The culture of this shrub is very troublesome, it being subject to the attacks of a little insect, which sometimes devastates a whole plantation in a single night. Three years after planting, the shrub commences to bear fruit. The harvest of coca, called *palla*, is very laborious, and usually left to women; they commence at dawn to collect the coca into little trunks, leaf by leaf, with the greatest care, so as not to break the stalks or otherwise injure them; a good workman fills ten such little trunks by night. The collected leaves are dried in the sun for two or three days with great care, so as not to become wet or moist, whereby they would be entirely ruined. This procedure is called *seca*, the box containing the fresh leaves *gerga*, and the collectors *palladoras*. The leaves are afterwards packed in bags of 1 arroba (= 32 lb.), and sent to the cities, among which Cusco receives the largest cargoes; from here all the departments of Peru and Bolivia are supplied. But a few years ago, coca was cultivated scarcely outside of Valles de St. Anna; now there are plantations at Paz, Cochabamba, and Alto-Peru, but the quality is inferior.

Coca drank as tea has the same aroma and taste as the finest pearl tea, without any stimulating qualities; from its numerous good properties, it promises to become an important commercial article of Peru and Bolivia.

¹ It has been variously proposed to supply emigrant and other ships with coca leaves, to be used as food in case of shipwreck and other accidents. Their introduction into the army and navy has likewise been advocated, and it is likely that they will be employed in some of the European armies, in order to test their efficacy in keeping up the strength and vigour of the men in urgent cases when food cannot be had.

PHYSICAL SCIENCE.

On Nöbert's Test Plate and the Striae of Diatoms,
by W. S. SULLIVANT and T. G. WORMLEY.

THE limit of the resolvability of lines, or how small a space can exist between lines and still admit of their being separated under the microscope, appears to be an undecided point. Professor Queckett ("Treatise on the Microscope," third edition, p. 238, 1855) asserts that "no achromatic has yet been made capable of separating lines closer together than the $\frac{1}{75000}$ th of an inch." In the same work, p. 245, it is stated that Mr. Ross found it impossible to ascertain the position of a line nearer than the $\frac{1}{80000}$ th of an inch. We also find on p. 512 that Mr. De La Rue, in his extended examination of Nöbert's test plates, was unable to resolve any lines closer than the $\frac{1}{81000}$ th of an inch. In Professor Carpenter's work ("The Microscope," second edition, p. 189, 1859), this sentence occurs:—"The well-defined lines on Nöbert's test plates have not yet been resolved when they have approximated more closely than the $\frac{1}{85000}$ th of an inch."

From the foregoing, it appears that actual experiment fixes the limit of resolvability at about $\frac{1}{75000}$ th of an inch. This does not, as is said, vary widely from the deductions of Fraunhofer and others, based on the physical properties of light. In this connection the remark (*op. cit.*, p. 47) of Professor Carpenter may be cited, "there is good reason to believe that the limit of perfection (in the objective) has now been nearly reached, since everything which seems theoretically possible has been actually accomplished."

On the other hand, there are authorities who assert that lines much closer than the $\frac{1}{85000}$ th of an inch are resolvable. A few years since Messrs. Harrison and Sollitt published (*Microscopical Journal*, vol. ii., p. 61, 1854) their measurements of the striae of several diatoms, assigning to *Amphipleura pellucida* striae as close as the $\frac{1}{150000}$ th to $\frac{1}{180000}$ th of an inch. These measurements have recently been repeated, and with exactly the same results, by Mr. Sollitt alone (*Mic. Jour.*, viii., p. 51, 1859), who furthermore expresses the opinion that striae as close as the $\frac{1}{175000}$ th of an inch, can, with proper means, be seen. Mr. Sollitt's measurements have been adopted in the *Micrographic Dictionary* (1860) and most of the modern works on the Microscope, no one, Professor Carpenter (*op. cit.*, p. 183) excepted, suggesting a doubt as to their accuracy; on the contrary, their correctness seems to be expressly recognised by Dr. G. C. Wallich ("Ann. and Mag. Nat. Hist." for February, 1860).

Such being the conflicting testimony and opinion of distinguished microscopists on the capacity of the modern objective for separating lines, it is somewhat surprising—in view of the high state of perfection now attained by the microscope, and of the number of its zealous devotees—that so few experiments have been made bearing on this interesting point.

As a contribution toward that object, we propose to offer presently an analysis from actual measurements, as far as we were able to carry them, of one of those "marvels of Art," Nöbert's Test Plates. In such investigations the quality of the instruments used being all-important, we would state that the optical apparatus at our command was ample, consisting of a first-class Smith and Beck microscope stand, a Tolles' $\frac{1}{16}$ objective of 160° angular aperture,—an objective of rare excellence in all

respects,—besides $\frac{1}{16}$ ths and $\frac{1}{8}$ ths of other eminent opticians, both English and American; also a solid eye-piece micrometer by Tolles, and an improved cobweb micrometer of Grunow's accurate workmanship. Smith and Beck's stage scales furnished the standards for fixing the micrometrical values of the eye-pieces. By means of Tolles' amplifier, an achromatic concavo-convex lens between the objective and the eye-piece, an amplification (by the standard of 10 inches) as high as 6000 times was obtained. This high amplification, with sunlight variously applied after passing through a small achromatic lens of long focus, was effective in resolution, and essential to the distinct counting under the micrometer, of the lines of the test plate. The test plate used consisted of 30 bands of lines, each band varying but little from the $\frac{1}{100}$ th of an inch in width, and having its lines a uniform distance apart. On one end of the plate is engraved by Nobert, in parts of the Paris line, the distance apart of the lines composing the first band, and thence on, the distance between the lines of every fifth band, as in the second and fifth columns of the following table:—

Band.	Par. line.	Eng. in.	Band.	Par. line.	Eng. in.
1	0'001000	11228	20	0'000167	87118
5	0'000550	20271	25	0'000143	76787
10	0'000275	40542	30	0'000125	80000
15	0'000200	50667			

We add the third and sixth columns, giving the distances in parts of the English inch found by multiplying the decimals in the second and fifth columns by .08835.

Analysis of Nobert's Test Plate of Thirty Bands.

Bands.	Lines in each band.	Parts of an English inch.	Bands.	Lines in each band.	Parts of an English inch.
1	7	11228	16	30	87118
2	8	13082	17	31	88833
3	9	15073	18	32	90548
4	10	17100	19	33	92263
5	12	20271	20	34	93978
6	13	23442	21	36	95693
7	15	26613	22	37	97408
8	17	29784	23	38	99123
9	20	32955	24	40	100838
10	22	36126	25	41	102553
11	24	39297	26	42	104268
12	25	42468	27	43	105983
13	26	45639	28	44	107698
14	28	48810	29		109413
15	29	51981	30		111128

The figures in the third and sixth columns, showing the distance apart of the lines in each band, are the mean of numerous and slightly variant trials, particularly on the higher bands. Up to the twenty-sixth band there was no serious difficulty in resolving and ascertaining the position of the lines, but on this and the subsequent ones, spectral lines,—that is, lines each composed of two or more real lines,—more or less prevailed, showing that the resolving power of the objective was approaching its limit. By a suitable arrangement

¹ The tendency of lines near the limit, either way, of the objective's resolving power, to run into each other and produce spectral or spurious lines, is readily shown by a low objective on the lower bands. Hence, the mere exhibition of lines is not always conclusive evidence of their ultimate resolution. A practised eye will generally distinguish the false from the true. Recourse to a higher objective often accomplishes the same; but when these fail, the micrometer only, together with a previous knowledge of the actual position of the true lines, can determine whether the lines exhibited are real or spurious. A $\frac{1}{16}$ th or $\frac{1}{8}$ th will show the three or four highest bands on this plate regularly and beautifully striped with lines much coarser than the true ones; the same with the $\frac{1}{10}$ th on the test band.

however, of the illumination, these spurious lines were separated into the ultimate ones on the whole of the 26th and very nearly on the whole of the 27th band; but on the 28th, and still more on the 29th, they so prevailed that at no one focal adjustment could more than a portion (a third or a fifth part) of the width of these bands be resolved into the true lines.

The true lines of the 30th band we were unable to see, at least with any degree of certainty; still, from indications, we have no doubt they are ruled as stated by Nobert.

It will be observed that our measurements of the lines on the 1st, 5th, 10th, 15th, 20th bands vary somewhat from Nobert's registration on the plate, as given in the first table above. Such discrepancies are to be expected, and by microscopists familiar with operations of this kind, are looked upon as unavoidable; but that on the 25th band is rather large to be accounted for in this way. We are unable to explain it, and can only say that our repeated measurements of it were very carefully made.

These experiments, together with those of others before noticed, induce us to believe that the limit of the resolvability of lines, in the present state of the objective, is well nigh established; but that this limit may be carried somewhat higher we are not prepared to doubt since the handsome advance lately achieved by Mr. Tolles in his $\frac{1}{10}$ —combining wide aperture, fine definition, and high amplification—shows that the objective had not, as we were inclined to think, reached the stationary point.

The theoretical view of this question,—that is, what may be the closest approximation of lines, consistent with their separation under the microscope—we leave to those competent to the task, by whom, it is to be hoped, we may be favoured with further information on this point.

With regard to the striation of diatoms, an opinion generally prevails that the number of striæ on a given portion of a frustule varies among individuals of the same species, within wide extremes. This opinion is probably traceable in part to one of the earlier publications on the subject, the paper of Messrs. Harrison and Sollitt before referred to, wherein (as in the more recent paper of Mr. Sollitt) measurements of several diatoms are given showing great variableness in their striation. To these gentlemen much credit is due for their discovery of high markings, before unsuspected, on certain diatomaceous frustules; their measurements, however, and the alleged variableness of these markings we have not been able to verify, as will be seen by the following extract from our paper on this subject:—

	Number of striæ in .001"		
	H. and S.	Sm. Syn.	S. and W.
Navicula rhomboides	60 to 111	85	70
Pleurosigma fasciola	50 to 90	64	52 to 56
Pleurosigma strigosum	40 to 80	44	42
Nitzschia sigmoidea	105	85	70

Many frustules of this species, from different localities, have been measured by us, and always with the same results. Pleurosigma fasciola has been specially designated by Mr. Sollitt, and also by Dr. Wallich, as very inconstant in its markings. Of this diatom we are fortunate in being supplied with abundant specimens, from various localities in England, particularly from the neighbourhood of Hull. Several hundred valves, not a few under $\frac{1}{100}$ th of an inch in length, were measured, and on no one were found striæ less than 52 or more

than 56 in '001", much the larger number being 54. A similarly uniform striation has always been observed among the individuals of many other species examined by us.

To such uniformity of striation *Amphipleura pellucida* forms as yet no exception; this diatom is still a "res vexata" among microscopists; neither the striation nor the structure of its frustule is at all satisfactorily understood. The record of its striation is found to be thus:—In 1854 Messrs. Harrison and Sollitt's measurements made its striae 120 to 130 in '001"; Professor Carpenter (1856) first suggests the probability of some error in these measurements; the writers of this paper declared themselves (this Journal, March, 1859) unable to glimpse the striae; Mr. Sollitt (*Microscopical Journal*, Oct., 1859) measures them again and finds them still as low as 120 to 130 in '001", but gives it as the opinion of Mr. Lobb that "even those figures are too low, and that they ought to be set down at 140 in '001"; in the same Number of the *Microscopic Journal*, Mr. Ryland "sees striae, but much more distant than the 130 in '001" of the Hull microscopists;" lastly, Mr. Hendry states (*Microscopic Journal*, July, 1860) that he has "come to a satisfactory conclusion that it is a sad misrepresentation to set down the lines so high in the scale as 130 in '001", and that on a few shells lines may be counted at 42, and many at 60, 70, and 80 in '001". A perplexing record, truly! reminding one of the celebrated Torbane Hill coal case (*Microscopic Journal*, ii., p. 64.)

It is our impression, notwithstanding these conflicting statements, that the diatom before us presented to all these gentlemen the same appearances, but their interpretation of these appearances have been widely different.

The testimony of our objectives, as we understand it, seems to indicate that this diatom has a minutely and irregularly broken-up surface, which, even on the same valve, can be made to show an apparent striation, varying from moderately coarse to extremely fine, according to the obliquity or intensity of the illumination, and to the grade, whether low or high, of the objective used, thus proving beyond question, that the exhibition is illusory. In numerous trials, particularly on fine English specimens from Hull, sent us by Mr. G. Norman, we have entirely failed, with glasses, too, of unsurpassed excellence, to bring out regular, distinct, and unmistakable striae, such as would be at once so recognised by an eye practised on the striae of other diatoms.

After all, it is not improbable that true striae yet unresolved, may exist on the valves of this species, and, furthermore, that the apparent striae of different observers may be similar to the spectral or spurious lines before noted as occurring on the bands of Nobert's Test Plate, when examined by an objective incapable of resolving them.

A summary of the foregoing may be briefly stated thus,—that our experiments lead us to believe:—

1. That lines on Nobert's Test Plate, closer together than about the $\frac{1}{1000}$ th of an inch, cannot be separated by the modern objective.
2. That no true striae have yet been seen on the valves of *Amphipleura pellucida*.
3. That the alleged variableness in the striation of diatoms among individuals of the same species has been greatly exaggerated; on the contrary, we find a remarkable uniformity, thus sustaining the opinion of Professor Smith ("Synop. Br. Diat." v. 2, Introd., p. 26), that for characterising species "striation is the best guide."—*American Journal of Science*.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, June 20, 1861.

W. DE LA RUE, Esq., in the Chair.

(Continued from page 122.)

After the paper by Dr. Roscoe was read, the following discussion took place:—

The CHAIRMAN: I am sure, gentlemen, you will agree with me that we are always grateful to those philosophers who, from time to time, are willing to bring before us and recapitulate not only their own discoveries but the discoveries of others. The fact is, science is so rapidly advancing, that it is necessary from time to time to halt and to take a retrospective view, and when we do halt, we find ourselves accompanied by a crowd of facts which have broken down the artificial barriers which were formerly raised up to separate one science from another, and it is quite necessary to re-arrange those barriers. It is not an uncommon thing for the physicist to tread upon the ground which a chemist thinks belongs to him, and for the chemist to tread upon the ground of the physicist. Now, we have the chemist occupying the ground of the astronomer, and if the astronomer wants to know something of the constituents of the heavenly bodies, he must come to the chemist. The physicist and the chemist have brought before us a means of analysis so wonderfully exact that, as Dr. Faraday recently said, if we were to go to the sun, and to bring away some portions of it and analyze them in our laboratories, we could not examine them more accurately than we can by this new mode of spectrum analysis. Dr. Roscoe has shown us that Bunsen has been able by its means to discover two new metals which exist only to the extent of five grammes in forty tons of a mineral water—that is, one part in a hundred millions of substance. Only conceive what refinement of analysis and methods of investigation we have now at our command. As we know, Kirchhoff and Bunsen have shown with the greatest probability, that there are certain metals known to us upon the earth existing in the sun's atmosphere. Professor Roscoe has told us this evening that the methods are applicable also to the discovery of the gaseous constituents—not necessarily metals in a gaseous state, but the ordinary gaseous bodies. Professor Faraday has recently given us another proof of the advantages of this method of observation. But I will not occupy your time, gentlemen, by any remarks of my own. There are very many who have given their attention to this subject, and who, I am sure, can throw considerable light on the investigations, and give us very valuable suggestions; besides which, I am aware that Dr. Roscoe is willing to show in a palpable manner the very refined methods which he has recently described, and, therefore, gentlemen, I will first ask you to return your thanks to Dr. Roscoe, and then I will ask any gentleman who may have anything to say on the subject to make his remarks.

Dr. MILLER: By way of starting the discussion, I will first offer a few remarks. No one can have listened to this exceedingly interesting discourse of Dr. Roscoe without feeling the great debt which we owe to those philosophers, Kirchhoff and Bunsen, who have just opened up before us so vast a field. I can only say, that one or two points have struck me since reading their papers which have induced me to make some additional experiments, and I will mention one or two points which have occurred to myself in discussing the subject. I have lately made some experiments with Ruhmkorff's electric coil. I have experimented with one of low power. I used a peculiar arrangement which indicated a difference of power between the two wires which are submitted to experiment. For instance, in the case of platinum the sparks produced are different in length

according to the power which is employed. It is well known from the investigations of Mr. Cassiot, that the negative pole becomes hotter than the other. Bright lines project half way across the luminous field, and those correspond in their starting point with the negative terminal; and on reversing the current, the position of the lines is reversed also, showing that it accompanies the direction in which the current is passing. If then, having put this to the test, you raise the heat by a spirit lamp you get the whole distance traversed by the bright lines, fully corroborating the remark which was made by Dr. Roscoe, that these appearances are solely due to those portions of the metals which are in a vaporous condition. The temperature is not sufficiently raised to establish the whole breadth of the band. There is also another point which struck me incidentally, and which I will mention, if I may be excused for making these desultory remarks. In experimenting with magnesium, Dr. Roscoe remarked that it was not easy to get the magnesium spectrum, obtained from the common salts of the metal. There is a little artifice which may be used, and that is, adding some common salt, by which you facilitate the volatilisation of the magnesium. The definite nature of the light given out by the sodium prevents any confusion in the results, and it enables you to get sufficiently well the lines of the other metal. In experimenting with magnesium wire, if at the same time you let daylight through, certain lines of the magnesium spectrum will be seen to correspond with the lines δ in the solar spectrum.

Dr. FRANKLAND: I am sure, Sir, there can only be one opinion among the Fellows of the Society as regards the beauty and usefulness of this great instrument of analysis which has been placed in our hands by Bunsen and Kirchhoff, and I trust we shall now see our way very clearly to use it. I wish to ask one question of Dr. Roscoe for my own information, and possibly it may contribute to the information of others. I was not here when the Lecture commenced, so possibly the question may have been answered. I have recently read, with very much interest, the beautiful researches which Dr. Miller made some sixteen years ago upon this very subject. It is really wonderful that sixteen years ago we had the real pith of the whole of this matter thrown before us, but up to the present time we have been unable to use it. I will not acquit Dr. Miller from being partly to blame, for, perhaps, he himself ought to have shown us the practical way of employing the instrument at the time it was revealed to us. But in these experiments, I think, if I am not mistaken,—and Dr. Miller will set me right if I am mistaken,—he showed that one and the same element did not yield in all forms of combination the same spectrum. Now, I wish to know how far experiments have been made upon the different metallic compounds, or upon the different salts of the metals, as to whether they really do yield the same spectra as the uncombined metals themselves. Dr. Miller found that nitrogen, for instance, produced certain spectra which were visible in perhaps one or two of its compounds, which were totally invisible in its other compounds with oxygen. Chlorine, again, produced a distinct spectrum, which was visible, I think, in nearly all its compounds with oxygen, but in one or two this spectrum was very materially changed. Now, I think it would be desirable, at the outset of our endeavours to use this process, to investigate this subject, so as to be as well acquainted as possible with the various metals; therefore, I wish to know from Dr. Roscoe how far such experiments have yet been made—how far we can rely upon these spectra as being in all cases true indications of the presence or the absence of what we are seeking for.

Dr. Roscoe: In answer to Dr. Frankland's very important question, I should say, to begin with, as far as regards the alkalis and alkaline earths, there can be no doubt that all those salts which in the colourless flame are volatile, give the same spectra; and if we take the

sulphate, or chloride, or nitrate, we do not see, with the instrument that Bunsen and Kirchhoff propose to use, any difference in the spectra of the different salts of the alkalis and alkaline earths. I imagine the explanation of what Dr. Frankland has just pointed out is to be found in the fact, that those substances, which at the ordinary temperature are gases, or which at a temperature very much lower than the temperature of the flame become gases, require a very much higher temperature to become luminous and give out light, than those substances which are either solid at the ordinary temperature, or very much less volatile. Now, I really cannot speak positively upon this point, nor do I think the subject is so far advanced that we can have positive information on it, but I do not myself suppose if we take the nitrate of strontium and the chloride of strontium, for instance, that in the spectra obtained by the electric spark, we should see exactly the same bands produced by the two compounds. I imagine that we should see in the one case something due to the presence of nitrogen, and in the other case something due to the presence of chlorine. How far this may interfere with the use of the spark, of course, I am not in a position to say. With regard to the beautiful experiments of Dr. Miller, published in the year 1845, I believe that the differences which Dr. Frankland alludes to were due to the absorption of those different vapours. I won't be quite sure whether that is the case. Dr. Miller will be able to give us positive information. I believe that Dr. Miller found that peculiar bands due to the oxides of chlorine were not the bands produced by the luminosity or by the ignition of the chlorine, but dark bands produced by absorption, and in that way, of course, he found different oxides of nitrogen gave different results, some of which gave coloured bands. I do not know whether Dr. Miller observed the luminous spectra of those substances. Thus NO_2 and NO gave bands which the elements themselves did not give.

Dr. MILLER: I may state, Sir, in reply to the interesting statement which has been made by Dr. Frankland, that it seems to me that there is a distinction between those bodies which are metallic and those which are not metallic. In the case of the non-metallic compounds, especially oxides, we have a different series of lines to those furnished by the elements in their uncombined state. For example, notwithstanding the green colour of chlorine, pure chlorine gives no lines to ordinary observation, and hypochlorous acid gives no lines. Chlorous acid and the peroxide of chlorine both give lines, and lines which even appear to coincide. In the case of the peroxide of nitrogen there is a series of lines characteristic of that vapour, which Dr. Gladstone has recently most elaborately investigated. I was not able at that time to isolate nitrous acid, corresponding with the chlorous acid, with such a degree of accuracy as would make the comparison worth anything. As has been stated, there is certainly a difference in the bodies, according to their state of combination. The appearance of lines is not always dependent upon colour. In a great many cases coloured vapours appear to give no lines; in other cases they do give lines. The intensity of the lines does not depend upon the intensity of the colour. A very curious illustration of that appears in the contrast between bromine vapour and the peroxide of nitrogen. Bromine gives a series containing a great number of lines, which look almost as if they were ruled with a ruler from one end of the spectrum to the other, and each of these ruled lines consists of an infinity of finer lines. In the case of iodine a similar appearance is observed, but it is much more graduated. I do not mean to say that they fall with the same intensity; but there is the same regularity and the same kind of gradation from darkness to light. Hydrobromic acid and hydriodic acid both give colourless vapours entirely destitute of lines, except so far as the lines are produced by ordinary solar light.

Dr. FRANKLAND: Would it not follow that if any vapour or gas was incapable of absorbing those rays of a particular refrangibility it would also be incapable of emitting them upon ignition? I think if the theory now propounded would account for those lines, such would be the case. It is only because these particular vapours possess the property of taking up those particular rays which move at a certain velocity that they are capable of absorbing to a great extent the light from the nucleus of the sun, and it is almost proved that when a vapour is incapable of absorbing certain rays of light, it is incapable, also, of emitting those rays at those particular temperatures, at all events.

Dr. MILLER: There is one observation which I should make. In experimenting with the salts of metals I generally use chlorides, because they are more soluble in alcohol. They were dissolved in that liquid, and in this way their flame could easily be examined. I always found that in the case of the chlorides there was a band about the green, and I attributed that to the escape of chlorine. I found in all cases where I used a chloride that I got this band, and I did not get it in other cases where I did not use a chloride. I did not investigate it with a sufficient degree of minuteness to say that it was due to the chlorine or not, but I thought it was due to the escape of the chlorine. I have formerly stated that as my opinion. The effect of the absorptive influence of these compound gases upon light it would be very difficult to ascertain, because none of these compound gases will stand a very high temperature without undergoing decomposition.

The CHAIRMAN: There is one matter, gentlemen, which has struck me in this discussion, and which it is necessary to bear in mind; that is, that with respect to light we do not see all the light, that is to say, all the force emitted by a luminous body, or a body in a state of incandescence, or having a temperature different from that which it has in its normal state. We do not see all the effect, and it is very possible that some of those luminous or incandescent bodies may give off rays which would be visible to a sensitised photographic plate; and that their vapours might also stop off a portion of those rays. Hence, I think it is very desirable that the experiments should be extended in that direction. I do not at all put this forth as an idea of my own, because I am quite ready and desirous to attribute it to the philosopher who propounded it, and that was Professor Faraday. Probably the prosecution of experiments in this direction might lead to the detection of bands which are not visible in the luminous part of the spectrum; I dare say it has struck other philosophers who are pursuing the subject, and it is very likely that investigation will be carried on in that direction.

Mr. HERSCHHEL: There is a question that I should like to ask Dr. Roscoe, if I may be allowed to do so, about the absorption of the sodium ray by the sodium flame. It seems to me that by the explanation given in which it is compared to the action of a sounding-board, that instead of the vibrations being absorbed when they came to it, they ought, on the contrary, to be heightened in intensity, it has always been difficult for me to understand the usual explanation.

Professor FARADAY: It has always seemed to me a very difficult thing to see how of necessity there was a depression of the light instead of an increase and exaltation of it.

Dr. ROSCOE: It seems to me that Mr. Herschel has hit the nail on the head. It is a very difficult point to answer, and I really do not feel myself to be able to give a satisfactory explanation of it. It is quite true that the simile of the sounding-board is not quite an exact one, because the vibration is taken up in the one case, and in the other case it is absorbed. I cannot explain it at all, and I don't know that it ever has been explained. About the fact of the absorption, I have no doubt at all. It appears to me a sort of interference, because certainly the intensity of

the light is diminished, and not increased. The simile of the sounding-board I believe was first given by Mr. Stokes, simply as one which would help us to form an idea how one body, which is capable of taking up a vibration, may be set in vibration by another body at a distance, undergoing the same vibration. It is, however, a matter which I must leave to physicists to explain.

Dr. MILLER: May I, Sir, venture a very crude suggestion upon this matter, as it has been started? May it not arise in this way?—If it is a case of interference, as Mr. Roscoe says, it may arise from the fact that in particular positions of the flame the phases of vibration coincide, and then we have increase; and that in other positions of the flame which is continually flickering, the phases of vibration vary, and neutralise each other. Thus, we may get the black in the one case, and the brightness in the other. I have repeated the experiments, and I have found in some cases an increase of light by interposing the electric light between the sodium light and the eye, and in other cases there has been a dark line produced. Now, may it not possibly arise from the fact of the flickering of this sodium light giving sometimes vibrations which coincide, and therefore reduplicate the light, and at other times the reverse, thus producing darkness?

Dr. GLADSTONE: I think, Sir, I may be permitted to say a word upon this subject, although I did not hear the discourse; for it so happened that it was necessary for me to leave the room to lay before the Royal Society a communication upon the same subject. I had to go and see how they got on there. My paper was read, although there were twenty-one papers before the Society, and there was a vigorous little discussion upon it, so we have been talking in another place upon just the same subject. There are one or two points which have been started in the discussion, which I may just allude to. A thought has been propounded by Dr. Frankland whether a gas which gives no lines of absorption, ought to emit lines when it was heated very strongly. Now, whatever we may think of that theoretically, we know in practice that it is absolutely not so, because nitrogen and oxygen and other gases are in the atmosphere, and in that condition. The second of those maps shows certain lines which make their appearance when the sun sinks towards the horizon—when his rays, therefore, pass through a large stratum of air. Now, it occurred to me that we might find out what constituent gave out these lines, by comparing those dark lines, or lines of absorption, with the brilliant lines given out when various constituents of the atmosphere are strongly heated. Therefore I compared the position of those with the position of the bright lines which appear when nitrogen is strongly heated in tubes by the electric spark, as shown by Angstrom and Plucker. I made the same experiments with oxygen; and I examined also the lines given by the oxy-hydrogen flame, and by carbonic oxide burning in the atmosphere, thinking these two flames might consist, more or less, of incandescent nitrogen and oxygen. The result of this comparison is, that I found no one point of similarity between those lines limited by these gases when strongly heated, and the lines which appear in the atmosphere when the sun's rays go slantingly through it. The conclusion, therefore, is that two or three hundred miles of mixed oxygen or nitrogen gases, do not exhibit any indication that we can appreciate of absorption bands in the same position as oxygen and nitrogen would emit rays, if strongly heated. The same holds good of water and carbonic acid. Hydrogen is the only doubtful gas in the matter; and there may be three of these particular bands which may possibly be due to hydrogen, but I don't think so myself. Another point which I may allude to is this, that we have in this way a most powerful means of showing the absence of certain constituents from the atmosphere. We can all see that if such a substance as peroxide of nitrogen—(and here I must correct a statement of Professor Miller, who described me as having

examined this particular gas: it was not examined by me, but by Sir David Brewster long ago, and all the credit which I can claim is that of putting Brewster's investigations together—but as these lines do not coincide with the dark lines that appear in the spectrum of the sun, we see that there is none of this gas in the atmosphere, or the moisture of the atmosphere would resolve it into other things. But we have the means thus of excluding bromine and iodine from the atmosphere. We have been told by Dr. Miller that these two gases produce most beautiful lines of absorption, which cannot be mistaken when they have been once seen. A small quantity of either will produce these lines. Now, these lines do not appear, and no trace appears in the spectrum of the sun. I calculated the matter roughly, and I found that we should be able to distinguish in this way one part of bromine in 950,000,000 parts of air, and there is not that.

Dr. Roscoe: I can only say one word of thanks to Dr. Gladstone for having mentioned the results of his experiments, which enables me to set myself right before the Society in referring to these spectra. Dr. Gladstone was not in the room when I spoke of them, or else he would have heard that I gave a different account of it, for I only compared Dr. Gladstone's table with Angstrom's table, and saw in it a very fair coincidence, and not having had time to investigate the matter I think it was a very fair assumption to suppose that there really was this absorption by the electric spark; but it appears now that this is not the case, and we must not rely on these drawings, then, but take Dr. Gladstone's own experiments. I only mention this because I stated before that it appears that it was a case of coincidence in the tables published.

Dr. MILLER: I think, in reference to the photography of the invisible lines of the spectrum, it should be stated that experiments have already been made in that direction. Becquerel and myself have made experiments on this, and I recollect some time ago seeing some very nice spectra which Mr. Crookes produced of the lines in the solar as well as metallic spectra.

The CHAIRMAN: It was not with respect to the photography of the spectrum, but with respect to showing photographically the separation or absorption of certain rays which are photographed when the metallic vapours are produced. It only remains for me now to state that the meeting is adjourned to November 7.

NOTICES OF BOOKS.

Lights and Lighthouses: Report of the Commissioners appointed to inquire into the Condition and Management of Lights, Buoys, and Beacons.

SECOND NOTICE.

IN continuation of the remarks on the general subject of this inquiry which appeared on a former occasion in our columns,¹ we have now to pass in review some of the results arrived at, and to point out other promising directions of investigation, which are treated of in detail or suggested by the perusal of the voluminous mass of evidence which has been accumulated by the Commissioners for the purpose of enabling themselves and others to decide upon the best methods of producing and judiciously applying the brilliant lights which serve as beacons on the coasts of the United Kingdom.

The first and perhaps the most important subject of inquiry refers to the character and source of light best adapted to general lighthouse use, and here at once a dis-

inction has very properly been drawn between methods applicable to all localities, and those which are only suitable for illumination on the mainland, or at most to those only which have the advantage of occasional intercourse with commercial districts in the immediate neighbourhood. Thus it is found easy to apply gas for the illumination of the harbour light at Leith Pier Head, but the carrying out of the same system at the Eddystone would, from its isolated position, be attended with much greater difficulty.

In the present state of the subject, it seems to be generally admitted that the use of the oil-lamp is on the whole the most satisfactory, especially in remote situations, but as the result of a canvass of the scientific opinions, it appears that whilst the majority concede this as an established proposition, twelve among the fifty authorities consulted give their votes in favour of other means of illumination. From these we select the following as representing the systems which appear most worthy of comparative trial. The Earl of Rosse recommends the substitution of the lime light, which recommendation is also confirmed by the opinions of Sir John Herschel, Mr. A. H. Renton, C.E., and Mr. William Austin, C.E. We may here remark that by a recent announcement in the *Times* the public is informed that a trial of the lime light was commenced at the South Foreland Lighthouse on Monday, the 26th of August last. Mr. Copeutt recommends that experiments be made with the Fitzmaurice light. Sir David Brewster advocates the use of a gas light fed with pure oxygen. Admiral Fitzroy and Dr. Robinson, of Armagh, suggest the application of oxygen in conjunction with the present oil-lamp. Mr. James Mackintosh recommends a trial of the Bude light. The various modifications of the electric light as proposed by Dr. Watson and Professors Holmes and Way have each their advocates; but on this head we refer with confidence to the reports rendered by Professor Faraday to the Trinity Board. The particulars relating to these inventions form a conspicuous section of the scientific evidence, and to these we propose again to revert. Dr. Robinson suggests that, in the event of the magneto-electric light being found too expensive for general use, the apparatus should nevertheless be provided, and used only during the prevalence of fog or snow. The same gentleman points out the advantage of the naphthalising process employed as a means for increasing the brilliancy of gas, whenever circumstances should render the use of this source of light possible and desirable. Paraffin oil, as a substitute for "refined rape," is preferred by Dr. Joule for use in lighthouses easy of access. Suggestions have also been made in reference to the use of certain pyrotechnic mixtures for producing lights of great intensity; but the objections incident to the employment of this class of materials appear almost insuperable, on account, not merely of the uncertainty in regard to regularity of burning, but mainly by reason of their combustion being invariably attended with the formation of sublimes, which are liable to condense on the inner surfaces of the glass lanterns, and thus materially obscure their transparency.

It is to be regretted that the recommendation to consult Mr. Babbage, who has made many points in reference to lighthouses the subject of particular study, does not appear to have been acted upon by the Commissioners, although the advantage of taking this course was suggested independently by the Earl of Rosse, Sir John Herschel, and Professor C. P. Smyth.

Among the propositions which appear to offer the greatest advantages for illuminating purposes the lime-light, at present under trial at the South Foreland, and the magneto-electric light of Professor Holmes, formerly tried at the same station, have been deemed those most worthy of careful comparative experiment. In order to prepare with facility the gases necessary for the first of these methods, it is recommended to employ the recent

¹ CHEMICAL NEWS, Vol. IV., p. 37.

suggestion of MM. Deville and Debray for procuring oxygen by the decomposition of sulphuric acid by heat; and also to use as occasional substitutes for pure hydrogen ordinary coal-gas and the highly-carburetted gas afforded by the destructive distillation of oil. These when not produced on the spot are to be conveyed thither in strong iron cylinders under powerful compression. It is manifest, therefore, that the employment of lamp-oil possesses in a single particular—that of portability—an advantage over the newer system, especially when applied in difficultly accessible positions.

With reference to the burning of coal as a means of producing light, it is interesting to compare the results obtained by the old fire-basket of the last century with those furnished by the combustion of gas prepared from one portion of coal by the consumption of another; and this system again with that which converts a given weight of coal burnt under a steam-boiler into motive power, and by the employment of this force in the rotation of magnets to produce an equivalent in the shape of the magneto-electric light. The practical solution of this and other equally important problems will not, we trust, be long deferred. These directions of scientific inquiry have a charm not only for Faraday, who introduced them as the subject matter of a lecture at the Royal Institution, but must always be appreciated by those who feel a general interest in these questions in economic science.

As to the optical apparatus, the dioptric system, invented and first employed in France, has been gradually adopted in our own country, and in Scotland some improvements have been made in it. There is, however, this important difference: in France the new apparatus was adopted throughout the whole lighthouse service; and in Spain and the States of America it has lately been exclusively adopted in the great reformation of their lighthouse system just effected by the Governments of those countries; but in the United Kingdom the old reflectors have only been replaced from time to time by the refracting apparatus; and the Board of Trade now lay down the principle that the expense should only be incurred when the reflectors are worn out. A judicious combination of metallic reflectors with the prismatic (dioptric) apparatus of Fresnel will often prove the most efficient arrangement, especially when the light is a fixed one and placed in a position where no illumination is required on the land side.

The distinguishing colours most suitable for lighthouses are red, green, and white; it would appear that white and red have greater fog-penetrating power, but Sir David Brewster justly remarks in reply to this question, that "it is not a matter of opinion, but should be experimentally determined." Sir John Herschel writes, "pale red and pale green give a good contrast, but there is a chance of colour-blind persons not perceiving it." Has the necessity for examining the eyesight of mariners employed on the watch been efficiently appreciated? Dr. George Wilson's researches on the subject of colour-blindness have proved that a very notable proportion of individuals are in some slight degree liable to errors in attempting the discrimination of colour.

Mr. George Gladstone calls attention to the beneficial results likely to arise from the extension of a system of telegraphic communication between lighthouse stations; whereby many disasters at sea might be avoided if it were possible to communicate by a code of signals, the information of coming storm to all vessels within sight.

In concluding our notice, we cannot refrain from offering willing testimony to the value of the labours of Mr. John Hall Gladstone in connection with the scientific questions involved in the inquiry. The suggestions referring to points still requiring investigation have been drawn up by this gentleman in a manner which seems to have exhausted the whole subject.

NOTICES OF PATENTS.

A New Paste made from Wood, to be Used in the Manufacture of Various Articles. C. STEVENS. A Communication.

THIS invention refers to the manufacture of a kind of pulp or paste from the fibrous tissue of wood, the material so prepared being capable of adaptation to many purposes similar to those for which papier maché is at present employed. The treatment to which the wood is subjected may be generally described as follows:—The shavings of soft woods, as poplar, birch, lime, and fir, are used by preference; these are boiled in a solution of caustic soda for a time sufficient to effect the tolerably complete separation of the woody fibres, which are then taken out, drained, and allowed to remain for several days in a mass until fermentation has set in, when the disintegration of the fibres will by this means have been further facilitated. During this operation the mass will require frequent watering to prevent the formation of a hard outer crust upon the glutinous material. Eventually the structure of the fibres will become soft and supple, and may then be further broken up by a kind of tearing engine, constructed of toothed rollers and surfaces, which is fully described in the specification. The washing and after treatment are very similar to the processes ordinarily followed in the making of pulp for paper.

The system of manufacture here defined, yields a description of pulp certainly inferior to that usually employed in paper-making, but which will nevertheless be suitable for moulding into shapes and for a rougher class of purposes where the production from cotton rags would be met by insuperable difficulties on account of cost.

Tanning Hides and Skins. J. H. JOHNSON. A Communication. (Not proceeded with.)

It is proposed to employ for the purpose of tanning hides a new vegetable decoction prepared from maruta cotula, to a gallon of which are added about four ounces of catechu, and two ounces each of alum and common salt. In this mixed tan-liquor the skins or hides, with or without hair upon them, are to be immersed, and there left for a period determined by the thickness of the skins, and by other circumstances which ordinary experience will dictate.

Sir Humphry Davy was, perhaps, the first to show that catechu alone could be employed as a tanning material. We are unacquainted with any special properties possessed by the vegetable decoction referred to, which would render advantageous its employment in conjunction with the foregoing.

Manufacture of Fuel. A. G. LASSEUR, Bordeaux.

THE intention of the patentee is to agglutinate small coal, coke, or anthracite into blocks or lumps of various shapes and dimensions convenient for employment as a compact kind of fuel. In carrying out this process the inventor heats the small coal to a softening temperature in a closed oven, and then, by a suitable mechanical contrivance, effects the complete admixture of the same with either coal tar, pitch, or resin, and moulds the hot plastic material into blocks of the shape required. The treatment of peat, also, in a similar manner is contemplated in the specification.

The general principle upon which the present process is based has repeatedly formed the subject of previous patents for the same object. Many years since the "Patent Fuel" Works were established at Swansea and elsewhere with precisely the aim of converting the dust and small coal into serviceable blocks of fuel, admirably adapted for shipping and other purposes where economy of space

is of the highest importance. By operations almost precisely identical, small coke, also, is daily reconverted into larger masses by the aid either of pitch, tar, resin, asphalt, or similar materials, the number of which is only exceeded by the perfect host of specifications descriptive of these manifold patented processes.

Portland Cement. J. LARK, Streod. (Not proceeded with.)

THIS invention relates to an improved method of drying the mixed materials (marl and chalk) after their incorporation by grinding with water, and prior to being burnt in the kiln to make Portland cement. The moist material is spread out upon the bed of a kind of drying stove, constructed with a series of wrought iron pipes laid underneath, and communicating with a steam boiler, which constitutes the source of heat employed in the process of desiccation. The inventor claims for this system the advantages of greater rapidity in drying, and more uniformity in regard to the degree of heat.

Improvements in Mordanting and in the Manufacture of Products to be Used in Mordants and Otherwise. J. TOWNSEND and J. WALKER, Glasgow.

THE improvements here referred to consist in the substitution of the hyposulphite and sulphite of alumina to be used in preference to other compounds of this base for the purpose of a mordant, particularly adapted to the dyeing of Turkey red. The specification describes the mode of preparing the mordant from alum and other salts of alumina, and likewise refers to the production of "precipitated antichlore" by double decomposition between the sulphite or hyposulphite of lime and a salt of alumina.

Dyes and Colouring Matters. C. H. G. WILLIAMS.

Obtaining Colouring Matters. V. HALL.

Preparing Colouring Matters for Dyeing and Printing. C. A. GIRARD, Paris.

THE patent of M. Girard is that alone which has been proceeded with. It refers to the production of variously-coloured dyes from aniline red by treatment either with aniline or any homologous base.

Mr. Williams' invention relates to the preparation of blue and purple dyes from the organic alkaloids, quinine, cinchonine, strychnine, and brucine; and is partly in continuation of a patent formerly granted to him (April 30, 1859) for the production of dyes from these sources.

The specification of Mr. Hall, of the College of Chemistry, describes minutely the steps taken for the production of colouring matters from the heavy oil of coal-tar, technically known as "dead oil." This is submitted to fractional distillation, and the separate portions severally acted upon with concentrated nitric acid, the nitro-compounds so formed being subsequently reduced by any one of the chemical agents specified, either by sulphide of ammonium, nascent hydrogen, or acetic acid and iron filings.

Grants of Provisional Protection for Six Months.

1748. Joshua Kidd, Strand, London, "Improvements in the manufacture, application, and combustion of gases, and in apparatus connected therewith."

1774. Rees Taylor and Thomas Price, Tydu-Tin Plate Works, Bassaleg, Monmouthshire, "Improvements in the manufacture of tin andterne plates."—Petition recorded July 15, 1861.

1928. Charles Schinz, Offenburg, Grand Duchy of Baden, "Improvements in glass furnaces."

1958. Peter Spence, Newton Heath, near Manchester, and James Mellor, Manchester, "Improvements in separating copper from its ores."—Petitions recorded August 6, 1861.

1972. Richard Jukes, Sheffield, "Improvements in puddling furnaces."

1978. Louis Le Paige, Merxem, near Antwerp, Belgium, "Improvements in treating fatty and oily matters obtained from wash waters to deprive them of their smell."

1983. James Hemingway, Robert Town, Yorkshire, "Improvements in machinery or apparatus for working coal, ironstone, or other minerals."

1985. Joseph Griffin and Charles Griffin, Walsall, Staffordshire, "New or improved machinery for the manufacture or cutting of corks and bungs."

1994. Henry Wilde, Manchester, "Improvements in electro-magnetic telegraphs, and in apparatus connected therewith."

2009. Joseph Jacob, Golden Square, London, "Improvements in producing on porcelain and other ceramic products, on glass, Venetian enamels, and on metallic surfaces, designs in colours, and in gold, silver, and other metals."—A communication from Franz Kosch, Vienna, Austria.

2013. Christopher Binks, Gray's Inn, London, "Improved methods of, and apparatus for, treating linseed and other oils and fats."

2023. Richard Archibald Brooman, Fleet Street, London, "Improvements in coating wire with copper, silver, gold, or other metal or alloy, in order to prevent oxidation."—A communication from Martin Miller, Vienna, Austria.

2025. Thomas Silvester, West Bromwich, Staffordshire, "Improvements in spring balances or weighing apparatus."

Notices to Proceed.

924. Thomas Miller, Fossoway, Perth, N.B., "Improvements in the method of, and machinery for, preparing india-rubber and other similar gums for insulating telegraphic wires, and in machinery for laying or applying strips of india-rubber and other similar gums or strips of fibrous or textile material on to telegraphic wires."—Petition recorded April 15, 1861.

940. Henry Anthonissen, Brussels, Belgium, "An improved method of making bread and obtaining starch from the materials employed simultaneously."

962. Philippe Mingaud, Rue Laffitte, Paris, "Improvements in obtaining jellies, syrups, and drinks from the tree 'arbutus unedo, or arbousier.'"

1006. Peter Ward, Clouds Hill Villas, Bristol, "Improvements in the manufacture of sulphuric acid."—Petition recorded April 23, 1861.

1019. Charles Stevens, Charing Cross, London, "A new artificial manure."—A communication from Hypolite Louis Marie Planchais, Rue Laffitte, Paris.

1057. Edmund Hamborough Joynson, St. Mary's Cray, Kent, "Improvements in machinery for the manufacture of paper."

1117. William Edward Newton, Chancery Lane, London, "Improvements in the treatment of copper ores."—A communication from Joseph Marie Puisetinne, Paris.—Petition recorded May 3, 1861.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Action of Ammonia on Chlorides.—M. Deherain has communicated to the Chemical Society of Paris the first part of an extended study of the action of ammonia on metallic chlorides (*Bulletin de la Société, &c.* No. iii., p. 51). When chloride of zinc is dissolved in liquor ammonia, the solution on cooling deposits a white crystalline powder, ZnCl_2NH_3 . The powder dissolved in hydrochloric acid yields, when the solution is evaporated, a well-crystallized salt, $\text{ZnCl}_2\text{NH}_4\text{Cl}$. The ammoniacal liquor which yielded the first salt evaporated at a gentle heat gives another, with half the amount of ammonia, ZnCl_2NH_3 . This salt dissolved in hydrochloric acid, like the former, gives a double salt, $\text{ZnCl}_2\text{NH}_4\text{Cl}$. The salt, ZnCl_2NH_3 , when ignited, loses half its ammonia, and then forms a yellow,

transparent, gummy mass, which is a very stable compound, $2 \text{ZnCl}_2 \cdot \text{NH}_4\text{Cl}$. The author adds nothing to our knowledge of the action of ammonia on bichloride of tin. Protochloride of antimony fused in an atmosphere of ammonia yields on the surface the compound $\text{SbCl}_2 \cdot \text{NH}_3$. Water decomposes this compound, forming a white precipitate, and leaving in solution the salt $\text{SbCl}_4 \cdot \text{NH}_4\text{Cl}$. The compound $\text{SbCl}_2 \cdot \text{NH}_3$ dissolves in hydrochloric acid, and yields on evaporation brown deliquescent crystals of a very unstable salt, $\text{SbCl}_2 \cdot \text{NH}_4\text{Cl}$. When ammoniacal gas is carried into the mass of fused protochloride of antimony, a white volatile salt is formed, having the formula $\text{SbCl}_2 \cdot 2\text{NH}_3$. With perchloride of antimony ammonia forms a compound to which Rose assigned the formula $\text{SbCl}_2 \cdot 2\text{NH}_3$, but which M. Deherain's analyses show contains $\text{SbCl}_3 \cdot \text{NH}_3$; and another, with 4 atoms of ammonia, $\text{SbCl}_4 \cdot \text{NH}_3$. Both of these form crystalline salts when dissolved in hydrochloric acid and evaporated. The author is inclined to regard the compounds of chlorides with ammonia as *chlor amides*, but admits that there are serious objections to the view.

Nitrous and Nitric Acid in the Atmosphere.—Cloeys has proved (*Comptes-Rendus*, T. lii. p. 527) the presence of nitric acid by passing air through solutions of the carbonates of potash and lead. In both cases he obtained nitrates. Other oxygen compounds of nitrogen, the author says, are present in the atmosphere, which makes him doubt the accuracy of the ozonometric observations made with iodized papers.

II. ORGANIC CHEMISTRY.

Formation of Fumic Acid.—The formation of fumic acid, so important, apparently, to the nutrition of plants, has received a long investigation at the hands of M. Paul Thenard, the discoverer of it (*Bulletin de la Société Chimique de Paris*, No. ii. page 33), who has come to the conclusion that it is a compound of ammonia or certain ammoniacal salts with vegetable principles. He wetted straw, dry leaves and sawdust with ammonia, and the carbonate and sulphate, and found that it was formed in abundance. He found, also, that when glucose, or sugar, was heated in a tube nearly to the temperature at which it decomposes, and a current of ammoniacal gas was passed, a large quantity of the ammonia was absorbed, and substances produced greatly resembling fumic acid. That formed from glucose was of a brown colour, was soluble in water, acids and alkaline solutions, but insoluble in alcohol. That formed from cane sugar was brown, uncrystallizable, soluble in alcohol and insoluble in water. Carbonic and the other acids dissolved it freely and alkalies precipitated it from the solution. Another body formed with the last had similar properties, but was insoluble in alcohol. The analyses of these three bodies gave the following results:—

	I.	II.	III.
Carbon	52.28	65.66	54.26
Hydrogen	6.38	6.05	5.34
Nitrogen	9.94	19.36	18.78
Oxygen	31.40	8.93	21.61

Similar substances to the above were formed when starch, mannite, cane sugar, or sugar of milk was heated with liquid ammonia in a sealed tube. When syrup was heated with liquid ammonia to 180° Centigrade, carbonate of ammonia, a black liquid and a black solid substance were formed, the two last of a very complex nature. The author speculates on the constitution of these bodies, admitting that he has but incompletely studied them. We do not, therefore, quote his speculations. He mentions a fact, however, which may be of value to some of our readers. A farmer, near Châlons, sprinkles his dung-heaps with ammoniacal gas liquor, and thereby obtains an excellent manure. The gas liquor used in this way producing a much better effect than when applied directly to the soil. M. Thenard examined the dung-heaps and found an abundance of fumic acid, or rather fumate of lime.

MISCELLANEOUS.

Popular Science.—A Mr. S. Holmes, writing to the *Mechanics' Magazine*, a weekly newspaper, which sometimes dabbles in science, favours its readers with the following piece of information:—"It is a proved fact, that the smoker's bugbear—nicotine—being an alkaloid, is instantly decomposed by the acid reaction of the saliva, and assumes the form of a harmless hydrocarburet." This reminds us of the celebrated definition of a lobster by the French *savans*; in two lines there are no less than four blunders—1, the saliva does not decompose nicotine; 2, it is not acid; 3, if it were, it would not form with nicotine a hydrocarburet; and 4, the compound it would form would not be harmless. The same paper, a few weeks ago, devoted an article to sneering at our statement that red-hot carbon decomposed aqueous vapour with formation of two inflammable gases.

The Polytechnic.—Return of Mr. Pepper.—The great services rendered to the cause of popular education by Mr. Pepper, and his untiring zeal in catering for the public amusement, during his former management of the Polytechnic, received a gratifying recognition on Monday evening, in the vast throng assembled to welcome him back to that Institution, and by whom his appearance on the platform of the large hall was hailed with the most enthusiastic plaudits. The subject chosen for the first of a new series of those popular scientific lectures which Mr. Pepper made so distinctive and pleasing a feature in the Polytechnic entertainments, was Ventilation, the discourse being one which, even without the lecturer's rare felicity of illustration and lucidity of explanation, would have been listened to with interest at this sultry season. The Polytechnic is certainly one of the best ventilated places of entertainment in the metropolis, as was shown by the comfortable temperature of the large hall on Monday, while crowded from floor to roof; but the difficulties which have hitherto rendered domestic ventilation unattainable, except by the rough expedient of opening the window, at the risk of the draught disarranging papers, covering furniture with dust, introducing a tormenting wasp, and probably catching cold, disposed every one to listen attentively to Mr. Pepper's exposition of the principles of ventilation, to feel interested in his ingenious experiments, and, we doubt not, to adopt the simple, inexpensive, and efficient apparatus which he recommended, and which has been patented by Mr. Cooke, C.E. The folds of wire gauze affixed to the upper sash prevent draughts by diffusing the current of air admitted, exclude dust and insects, effectually ventilate the apartment, and are easily fitted. After the lecture the auditory dispersed to view the pictures, sculptures, models, &c., so profusely displayed in the body of the building, and the ever attractive operations of the diver; and the entertainments concluded with the exhibition of a series of beautiful dissolving views, illustrative of a "Voyage down the Stream of Time," commencing with the rock-temple of Abou-Simbel, in Egypt, and terminating with the great fire at London Bridge.

ANSWERS TO CORRESPONDENTS.

Alchemy.—This is too often confounded with the search after the philosophers' stone and the gold-making of the sixteenth and seventeenth centuries. Alchemy was the science of chemistry, and included all those processes in which chemistry was technically applied. To the alchemists we owe modern chemistry and experimental philosophy.

Adulteration of Butter with Lard.—A correspondent wishes to know a ready means of detecting the presence of lard in butter, an adulteration which he is certain is almost universally practised with metropolitan "best fresh" and inferior qualities.

X. Y. Z.—Write to the Secretary, Dr. Odling, Burlington House, Piccadilly. He will give you the necessary information.

THE CHEMICAL NEWS.

VOL. IV. No. 93.—September 14, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Chemical Researches on the Assay of Silver by the Moist Way, by G. J. MULDER, Math. Mag. Phil. Nat. Medicinæ, Artis Obstetriciæ et Pharmaceuticæ Doctor, Professor of Chemistry at the University of Utrecht, Grand Cross of the Order of the Netherlands Lion; of the Luxembourg Order of the Couronne de Chêne; of the Swedish Orders of Wasa and Polar Star, &c., &c.

(Continued from page 127.)

EVEN were the change which chloride of silver undergoes by the effect of daylight far smaller than it really is, it is quite clear that the estimation of silver by the moist way would yield incorrect results, if during the experiment discoloration were allowed to take place. It is therefore necessary to put all the bottles used during the experiment in cylindrical boxes made of some suitable metal, painted black inside, and to perform the experiment so that the light of the day can only have access to the bottles at the moment of observation, and even then the chloride of silver should not be quite uncovered, but the bottle be left in the metallic box, and a portion of the bottle only lifted out of the box sufficient to give space to inspect the supernatant liquid. When the bottles are to be shaken in the boxes, the necks of the former are covered with a suitable cover, or with a double cap made of black cloth, so as to expose nothing of the chloride of silver to the light during the operation of shaking. That the chloride of silver, when losing some of its chlorine, is really changed into the already-mentioned violet substance, may be readily shown, for if the bottle containing the chloride, is filled with chlorine gas it can be exposed to the daylight without discoloration.

When heated in a dry state, chloride of silver melts, it loses a little of its chlorine, and undergoes a similar change as when exposed to the light. All quantitative determinations, therefore, both of silver and chlorine, when made by heating and igniting the chloride of silver before weighing, yield a result which is a little below the truth. The white chloride of silver gets the more coloured the more chlorine is expelled. Chloride of silver is to a very great extent insoluble in water. If one-fifth part of a milligramme of silver in solution is added to 200 cubic centimètres of water, which are equal to 200,000 milligrammes, and there is then added as much common salt in solution as is equivalent to the silver, then it will be seen that the liquid is rendered blueish and milky. $\frac{1}{100000}$ th of silver can therefore be recognised as chloride of silver in water; this property is one of the most important in the estimation of silver by the moist way. Four times as much silver, however, is required to distinguish a precipitate. Nitric acid added to the last-mentioned liquid does not produce, as

far as can be seen, any diminution of the milkiness or of the precipitate. Chloride of silver, therefore, is not rendered more soluble in water by the presence of nitric acid. This fact, also, is of great importance for the estimation of silver by the moist way. Chloride of silver is rather, although even in small proportion, more soluble in strong nitric acid than in water. When 50 cubic centimètres of nitric acid of a specific gravity of 1.2 are taken, and one-twentieth part of a milligramme of silver is dissolved in this acid, and there is then added as much solution of common salt as is equivalent to that weight of silver, then the temporarily formed chloride of silver disappears. I will describe among the apparatus a dropping apparatus fit for such experiments. In the nitric acid solution mentioned in the last experiment, both nitrate of silver and common salt in solution can produce a trace of precipitate. When strong nitric acid, which contains suspended in an undissolved state a well visible trace of chloride of silver, is diluted with water, it is rendered perfectly clear. Chloride of silver, therefore, is soluble in a rather concentrated nitric acid, but it is still more soluble in nitric acid diluted with water.

Chloride of silver may be said to be equally little soluble in water as in weak nitric acid.

It is impossible to detect in either of these cases $\frac{1}{100000}$ th part of silver; $\frac{1}{100000}$ th may be detected in both cases: therefore, neither water nor weak nitric acid interfere with the estimation of silver by the moist way. But we have not only to deal, when actually experimenting, with the great insolubility of the chloride of silver, the question also presents itself at what degree of dilution a precipitate can be still perceptible. This may be put without any exaggeration at a dilution of one in 200,000 parts.

Chloride of silver is, to a certain extent, soluble in hydrochloric acid. In case hydrochloric acid is added to the water which contains, in a suspended state, the chloride of silver formed by $\frac{1}{100000}$ th part of metallic silver, then the milkiness formed by the precipitate disappears totally. Larger quantities of chloride of silver mixed through water are also dissolved by hydrochloric acid, and increasing temperature promotes this solvent property of hydrochloric acid. This goes on even to such an extent, that boiling concentrated hydrochloric acid dissolves a sufficient quantity of chloride of silver to make the latter precipitate on cooling in the form of octohedral crystals, the quantity of which increases on evaporation. Pierre (*Journ. de Pharm.* 3me Série, tome xii., p. 237) has found, that the solubility of chloride of silver in concentrated hydrochloric acid was about $\frac{1}{100}$ th, and in hydrochloric acid, diluted with twice its weight of water $\frac{1}{100}$ th. It may therefore give rise to some astonishment, when Otto (*Lehrbuch der Chemie*, Aufl. 3, bd. ii., abth. 2, s. 747) advises to precipitate a solution of a silver salt, wherein the quantity of silver has to be estimated, while warm, with hydrochloric acid; he even says to precipitate while the liquid is very warm, in order that, after shaking, the liquid may be rendered so

clear as to allow of decantation. In so doing, a weighable quantity of silver is lost. The idea given by Rose (*Analyt. Ch.*, ii., s. 199) is better, since he recommends to leave the liquid after having been warmed standing at rest for twelve hours, then to pour off the supernatant liquid, and to wash the chloride of silver with warm water.

It may be true that for all the usually-occurring cases in analytical chemistry the dilution with much water is sufficient to prevent hydrochloric acid to keep in solution the chloride of silver. It is equally true that even very much diluted hydrochloric acid has the property of dissolving minutely-divided chloride of silver to a visible extent.

Chloride of silver is not any more soluble in dilute sulphuric acid than it is in dilute nitric acid; sulphuric acid, therefore, may be quite as safely used for the dissolution of silver, as nitric acid, as far as this point is concerned.

Chloride of silver is, at a higher temperature, visibly soluble in a solution of tartaric acid; so much so, that a precipitate formed in the presence of tartaric acid at common temperature, disappears at boiling heat, but re-appears again on cooling.

Chloride of silver is soluble in all chlorides. In a quantity of one cubic centimetre of a concentrated aqueous solution of chloride of zinc, more than one milligramme of metallic silver may exist in solution without producing any turbidity at all. When diluted with water, chloride of silver makes its appearance.

The chlorides of sodium, potassium, calcium, barium, and strontium all dissolve some chloride of silver; an increased temperature increases the quantity of dissolved chloride of silver. On cooling cubical crystals are precipitated, which have been found to consist of double chlorides of silver and one of the mentioned metals.¹

The chlorides of magnesium, ammonium, and mercury (corrosive sublimate) do all dissolve some chloride of silver; chloride of mercury, however, does so to a small extent only.

The already-mentioned double chlorides do not bear the admixture of nitric acid; they are decomposed by it. It is therefore unnecessary to consider the presence of a double salt of chloride of silver and chloride of sodium in the estimation of silver by the moist way.

The nitrates of soda, potassa, lime, magnesia, and ammonia dissolve chloride of silver; this solubility is not great however at common temperature, but is so large in the heated, clear liquids, that they all become turbid on cooling by the precipitation of chloride of silver.

The solubility of chloride of silver in nitrate of soda will presently bring us to the explanation of a phenomenon which has a great influence on the estimation of silver by the moist way. Solutions of caustic potassa and soda and of carbonate of potassa and soda, do not decompose chloride of silver in suspension in a liquid at common temperature. On boiling such a liquid, chloride

of potassium and sodium are formed, as also oxide of silver. The best solvents for chloride of silver are liquid ammonia, cyanide of potassium, and the hyposulphites of potassa and soda. The two latter form well-known double salts.

The solution of chloride of silver in liquid ammonia, when exposed to the action of atmospheric air, deposits octahedral crystals of chloride of silver; when slightly warmed, shining scales are deposited; on boiling such a solution, detonating silver is formed. Acids precipitate from the solution of chloride of silver in ammonia chloride of silver. Nitrate of peroxide of mercury dissolves a large proportion of chloride of silver. The nitrates of most other metals are not endowed with this property. In every case it is so small as not to interfere with the estimation of silver in the moist way.

3. *Point of Neutrality and of Mutual Precipitation.*—I assume that to properly understand the following the reader is acquainted with the ordinary process of testing for silver by the moist way. The first question which immediately arises is, what quantity of the decimal solution of chloride of sodium has to be used after the addition of the standard solution of chloride of sodium? Suppose we had dissolved in nitric acid 1002 milligrammes of pure silver, and that 1000 milligrammes had been precipitated by the standard solution, 2 milligrammes have been left to be precipitated by the decimal solution of chloride of sodium. Let it further be supposed that the quantity of the decimal solution therefore required be added *guttatim*, 20 drops equal to 1 cubic centimetre, and that after every addition the liquor has been well shaken. At what moment is the addition of the decimal solution to cease?

I assume that the operator only adds the solution drop by drop, and shakes the liquid until clear again; that he can very clearly see that the bulk of the precipitate, formed after every addition of the decimal solution, diminishes, and that he comes at last to the certain conviction gradually, that after the addition of but one drop more, no precipitate would be any more obtained, the extreme limit of chemical accuracy has only then been reached: too much salt has not then been added. 100 cubic centimetres of the standard salt solution have precipitated 1000 milligrammes of pure silver; and assuming 20 drops to 1 cubic centimetre,² then every drop of the decimal salt solution has precipitated $\frac{1}{20}$ th milligramme of silver. Has everything been well and properly done, so that if 40 drops of the decimal solution had been required, we should have been justified in assuming the presence of 1002 milligrammes of silver?

This question requires a very accurate investigation, and the more so since, as far as I am aware from printed and published matter, it has never been fully gone into. The question itself is a general chemical one, and not merely a particular technical question only bearing upon the assay of silver, it is therefore a question of great scientific interest. It is almost unnecessary to mention that there is a very intimate relation between the right appreciation and solution of this question and the assay of silver; and it is equally needless to show that whatever bears upon the precipitation of silver, is applicable to all similar cases where precipitation takes place in general, and since the volumetrical method has justly become more and more general, and since precipitation is intimately connected with that process, I judged it best to treat this subject more minutely, also in a general chemical point of view, although I intend to pay most

¹ On account of the solubility of chloride of silver in common salt, or other chlorides of the metals of the alkalies, it is Rose's advice (*Analyt. Chem.*, ii., s. 198), when the presence of the said chlorides cannot be avoided, and when they are met with to a larger extent, to evaporate the liquid, after separation of the chloride of silver by filtration, nearly to dryness, and to add nitric acid, by which the chlorides are decomposed to nitrates, and to heat afterwards, so as to promote the precipitation of the small quantity of chloride of silver kept in solution by the chlorides. This is certainly a good plan, but it does not answer the purpose for which it is given completely, since chloride of silver, although less soluble in the nitrates which have been formed, than in an excess of chloride of sodium, for instance, is, however, kept in solution by the nitrates then present in the liquid to a very noticeable extent.

² In the part entitled the "Method" will be given the description, and some drawings of the necessary apparatus.

attention to that kind of analysis which bears upon silver, which, when well understood, may be taken as the type of the whole question.

When, to the clear and well-shaken liquid which has received 1002 parts of a solution of salt (and in which there is neither one drop too much, nor one too little of the decimal salt solution), a decimal solution of silver is added drop by drop, a precipitate is seen to make its appearance; if the liquid be shaken, and the precipitate be allowed to settle, then upon a fresh addition of more drops of the decimal solution of silver, another portion of precipitate will make its appearance. This precipitate diminishes after another addition of decimal solution of silver, and finally ceases to make its appearance when, at a temperature of from 15° to 17° C., 20 drops of decimal solution of silver have been added.

(To be continued.)

TECHNICAL CHEMISTRY.

Local Decomposition in Lead Aqueduct Pipes, by Mr. J. R. NICHOLS.

THE author calls attention to a source of danger attending the use of leaden pipes used for the conveyance of drinking-water which seems to have been hitherto disregarded. Even if it be admitted that the water which is supplied to the city of Boston from Lake Cochituate, like that of most New England ponds, be such that it may be safely used after having passed through lead pipe under ordinary circumstances, it would nevertheless be wrong to infer that this water can be employed with entire safety at all points of delivery, without first inquiring whether special conditions may not exist in some localities by which the character of the water may there be changed. Having observed several instances in which the inmates of a single house had suffered from lead disease induced by the use of aqueduct water, while the inhabitants of other parts of the city supplied with water from the same original source were unaffected, and having in one instance detected the presence of considerable quantities of lead in one of the cases first mentioned, while no reaction for lead could be obtained from a specimen of the same aqueduct water taken from another locality, the author proceeded to inquire into the cause which produces this lead impregnation in certain houses or districts while the general waters of a supply remain unaffected. He has noticed in the leaden pipes removed from cesspools, sinks, and wells, that the intensity of corrosive action had been in great measure confined to the sharpest bends and depressions in the pipe, while in some instances other portions remained intact. "I have in my possession," he says, "a section of supply pipe, removed from the aqueduct of a neighbouring city, in a portion of which corrosive action had proceeded so far as to cause leakage. The part thus acted upon was confined to an acute angle, and there is evidence to show that the plumber, in placing it in position, bent it in the wrong direction, thus creating the necessity for another turn in the opposite. This pipe had, doubtless, been subjected to two violent turns, which seriously impaired the homogeneity of the metal. An examination of lead pipe removed from buildings will certainly show that where there has been any perceptible amount of decomposition, it has been confined to the angles and depressions in its course. There are three causes or agencies which may perhaps

be sufficient to produce these results:—1. The disturbance in the crystalline structure of the metal by bending, whereby its electrical condition is changed and voltaic action promoted, giving rise to chemical decomposition. [Together with the galvanic action which must be induced wherever connections or faucets of copper, or alloy, are fastened to the leaden pipes, or where a crack or fissure in the latter has been filled with solder.] 2. The presence of organic matter, such as fragments of leaves, and impurities pervading all pond waters, and which may be detained in angles and depressions of the pipes. Their presence, undoubtedly, promotes oxidation (?) and the protoxide of lead will remain in solution, unless sufficient carbonic acid is furnished to change it. It is easy to conceive of conditions where this could not be the case. 3. Corrosions may be produced in lead pipes by the accidental presence of pieces of mortar. Where mortar is present, the lime would assist in oxidising the metal, and also aid in the solution of the oxide. Considerable portions of fresh mortar are frequently deposited in lead pipes during the erection of buildings. When the family commence the use of the water, it holds the salts of lead in solution, and its presence may be detected for months. The process of oxidation, which is retarded or prevented altogether by the presence of neutral salts in water, could not be materially interfered with under the conditions considered. It is obvious, if these observations and conclusions are correct, that much care should be exercised in placing pipes in position in buildings. In those leading to the culinary department, angles and depressions should be avoided. Violent twists and turns should not be permitted, and during the erection of houses, the open ends of protruding pipes should be carefully closed. Assuming the general fact that lead pipes, conveying the waters of our New England ponds, become coated and protected by an insoluble lead salt, the question arises, how long before this protection is secured, or, how soon may a family commence the use of water passing through new pipes with safety? In view of the manifest danger from local disturbances, the most sensible reply would be, *never*. A section of new lead pipe immersed in Cochituate water one hour, at a temperature of 65° F., gave a decided lead reaction with sulphydric acid. Removed and placed in six fresh portions of water, one hour in each, the waters, when tested, gave similar results. The experiment continued during two weeks. Varying the time of immersion in fresh portions of water from one to ten hours, the lead indications continued, although at last feeble. These results are sufficient to show that individuals or families should not commence the use of water flowing through new pipet until considerable time has elapsed, and much water contact secured."—*Boston Medical and Surgical Journal*, lxiii. 149.

PHARMACY, TOXICOLOGY, &c.

On a New Anæsthetic, Kerosolene, by EPHRAIM CUTTER, M.D., of Woburn, Mass., U.S.

THIS new anæsthetic, kerosolene, is obtained as a product, which hitherto has been thrown away as useless, in the manufacture of kerosene oil, by the destructive distillation of coal, not coal-oil nor tar. If I understand it aright, the crude volatile matters that come over from the still at a temperature of 150° Fahrenheit are condensed and treated with sulphuric acid and then redi-

titled. The result is the article you see before you. It may be largely produced. I am informed that from forty to sixty thousand gallons could be annually made by one establishment alone in Boston from the waste of the usual course of manufacture for that period. It could be furnished at a dollar a gallon.

The chemistry of the article has not been described. It has been named by the discoverers Kerosolene or Keroform, possibly under the impression that it may be a new organic radical, or a compound of a new organic radical. Whether this has been definitely determined by analysis is not known. From its mode of production, specific gravity, physical and physiological properties, one would fain class it as an ether or an analogous hydrocarbon. Kerosolene is a beautiful colourless and volatile liquid, with a specific gravity of .634 (roughly determined). Ether has a specific gravity of about .750, chloroform 1.49. Tested with litmus paper, it was found neutral. As regards the appearance to the sight, it cannot be distinguished from ether. The odour is not peculiar and hardly perceptible, and what is perceptible is very pleasant, resembling that of chloroform. It leaves no persistent smell in the apartment where it is evaporated. In this respect it is very much superior to ether, and is not so sickish as chloroform. It is tasteless. It evaporates wholly on exposure. A given quantity of ether and kerosolene was evaporated in capsules floating in water at a temperature of 108° F. The ether disappeared first. It is highly inflammable. Besides the odour just mentioned, there is a faint smell, like kerosene-oil. It seems as if the specimen examined was not chemically pure, but contaminated with a small quantity of the kerosene-oil. (The different samples seen have varied, some in regard to this kerosene odour. The second sample contained much more than the first, and occasioned a slight headache upon inhalation, which a third did not, having very much less of the kerosene odour.) This suspicion is confirmed by agitating the kerosolene with twice its bulk of water. The phenomena witnessed are the same as are seen when ether is similarly treated, only that the kerosolene forms larger and more persistent balls, and the water is much more and longer turbid. A portion allowed to stand twelve hours after agitation with the water presented a distinct layer of oily matter, between the upper surface of the water and the lower surface of the kerosolene. Ether furnished a similar one, but not so distinct and voluminous.

The vapour applied to the skin, as is sometimes done with chloroform in neuralgia, is irritating, but not to the same degree as that of the latter. The first intimation that this substance had anæsthetic properties, was declared by the fact that an Irishman, sent in to clean out a still, became totally insensible. Upon being withdrawn, and recovering his faculties, he expressed himself as having had a very fine "dhrame." The article was thus suspected: a quantity of it was procured, and given to mice and flies. One mouse was killed by it. Mr. W. B. Merrill, an *attaché* of the Works (Downer Kerosene-Oil Company, Boston), then introduced it to the notice of Dr. H. J. Bowditch, who in turn called the attention of the Boston Society of Medical Improvement to it. This body appointed a commission to examine into its claims. In prosecuting their inquiries Dr. H. J. Biglow was probably the first to try it upon the human subject, and that subject was himself. He also exhibited it to some patients. An account of his experiments was published in the *Boston Medical and Surgical Journal*.

The writer has experimented upon himself and others. In my own case, it was given to me by my friend and student, Mr. S. W. Abbott. I remained under its influence full half-an-hour twice. Perhaps four ounces of the kerosolene were consumed in each trial. The first impression is sudden, powerful, and pleasant. Almost immediately I passed into a state of pleasurable insensibility, which at no time was perfect. At least there would be responses to the pricks of a pin, and the muscles were not quiet—although afterwards I had no recollection of any such thing. The pulse and respiration were not much altered from their normal rate, the face was rather pale. At the first trial, Mr. Abbott states that I travelled about and grasped the napkin on which the kerosolene was found with my teeth, so strongly that he could hardly get it away. The stimulus of the vapour penetrated the whole frame, even to the tips of the fingers. No unpleasant effects have been experienced from the two experimentations, except a slight headache, which I think may be due to the presence of the adventitious kerosolene.

As you breathe the vapour, you seem to float away into a wavy maze, with a sense of complete loneliness. There appears to be but one object in the universe, and that object is yourself. On recovery, the first thing seen is deemed the next only existence in the universe. It takes some little time to regain all the faculties.

At the meeting at which the above was read, quantities of the kerosolene were distributed, and several of the gentlemen of the Society inhaled it in the presence of the assembled company. They came under its influence immediately, and also recovered from it well. Dr. Ingalls reported two trials of its external use in cases of neuralgia, in one of which it succeeded. The mode of procedure is to moisten a pledget of cotton wool with the kerosolene, lay it upon the skin, and cover it with a watch crystal.

From the above trials it is proper to infer that kerosolene is a very fit article for further experiment. It must be presumed to be almost safe, as in all the trials of it upon man and the lower animals, only a mouse has been killed. Its price, odour, and instantaneousness of action are advantages over ether.—*American Medical Times*.

PHYSICAL SCIENCE.

Notes on the Atmospheric Lines of the Solar Spectrum, and on certain Spectra of Gases, by Dr. JOHN HALL GLADSTONE, F.R.S.

IN the paper of Sir David Brewster and myself on the lines of the solar spectrum¹, attention was drawn to the following among other phenomena:—

1st. "When the sun descends towards the horizon and shines through a rapidly increasing depth of air, certain lines which before were little if at all visible, became black and well defined, and dark bands appear even in what were formerly the most luminous parts of the spectrum." These we termed "atmospheric lines." We did not wish to express by that term anything beyond the fact above mentioned; yet we threw out the idea that these lines may have their origin "in the air that encircles our globe."

2nd. In the case of those artificial flames whose spectra "consist of a series of luminous bands separated

¹ *Philosophical Transactions*, 1860, p. 149.

by dark spaces. . . . These luminous bands sometimes coincide with the dark lines of the solar spectrum."

About the same time Kirchhoff² published his theory that this remarkable coincidence is due to the presence in the atmosphere of the sun of the substances which emit these luminous bands, and their appearance as dark lines is because "the spectrum of an incandescent gas becomes reversed, when a source of light of sufficient intensity, giving a continuous spectrum, is placed behind the luminous gas." It also appears that "sodium vapour at a temperature much below that at which it becomes luminous, exerts its absorptive power at exactly the same point of the spectrum as it does at the highest temperatures which we can produce." It is, however, as yet an undetermined question at what temperature below that at which it becomes luminous, a gas ceases to absorb; though, from the close connection which has been shown to exist between emission and absorption, there can be little doubt that such a limit would be arrived at.

This theory of the origin of Fraunhofer's lines, and the uncertainty just alluded to, suggested the desirableness of comparing the "atmospheric lines" of absorption with the luminous bands which make their appearance in the linear spectra of the light emitted by the different constituents of the atmosphere when sufficiently heated. If all the rays emitted by these different constituents should be found to coincide with the "atmospheric lines," it would show at once the origin of the lines, and prove that gases at the ordinary temperature will absorb rays of the same refrangibility as those they emit when themselves glowing. If, on the contrary, they should be found not to coincide, it would prove by an extreme case (for we operate thus upon many miles of oxygen and nitrogen gases at least) that this connection between absorption and emission either does not extend to these elements, or is confined within those narrower limits of temperature which theory seems to require. It was also conceivable that the atmospheric lines might coincide with the rays emitted, not by all, but by a portion of the constituents of the atmosphere.

The following data exist for this comparison:—Angstrom³ has delineated the luminous bands due to the gas when the electric spark is sent through nitrogen, oxygen, carbonic acid, hydrogen, &c. Plücker⁴ has described and measured those bands which appear in the spectra of the same and other gases when they serve as the residuary gas in Geissler's tubes. Under the influence of the electric discharge, the vapour of water, carbonic acid, ammonia, and other compounds are decomposed; but it occurred to me that the oxyhydrogen flame must consist partly, if not entirely, of glowing steam, since it is only when the two elements combine that the heat is evolved; and that similarly the flame of carbonic oxide burning in air must contain the light emitted from glowing carbonic acid. I therefore subjected these two flames to prismatic examination.

The result of the comparison has been that not one of these observations shows any accordance between the luminous bands due to the gas, and the dark lines that make their appearance in the solar spectrum when the sun is shining through a great depth of air. Hydrogen alone is inconclusive. Neither is there any accordance between these luminous bands and the more prominent lines of ordinary solar spectrum.

This shows that oxygen and nitrogen, and perhaps other gases, though in enormous quantity, do not absorb at the ordinary temperature rays of the same refrangibility as those they emit when heated by the means specified.

It would not be legitimate to infer from this that the atmospheric lines have not their origin in the absorbent power of one or more of the common constituents of the atmosphere. It is well known that some gases when placed before a continuous spectrum produce lines of absorption; among these are bromine and iodine vapour; yet the dark lines caused by these two halogens⁵ do not coincide with the bright lines into which Plücker found the light of Geissler's tubes containing bromine and iodine to be resolved by the prism; nor have I succeeded in reversing them by bringing these substances into a very hot but little luminous flame. Bright lines were discerned, but in other positions. In connection with this subject, it may be worth noting that a prismatic examination of the sun's rays passing through three inches of mercury vapour at above 300° C., did not afford the least indication of the reversal of the bright rays that appear when that metal is rendered incandescent.

From the fact that the atmospheric lines do not always present the same appearance when the sun is on the horizon, and that the band δ has been observed during a shower, and the most prominent lines during a fog, it has been sometimes supposed that the aqueous vapour in the atmosphere is the cause of them. Yet this can scarcely be. They seem not to be due to little vesicles of condensed vapour; for the sun's rays, when passing through the edges of a cloud, do not exhibit them, unless, of course, near the horizon. And they seem not to be due to gaseous water; for they appear near sunset when the aqueous vapour in the atmosphere is reduced to a minimum by frosty weather, though they are not seen when the sun is higher up in the heavens on a damp warm day. From Sir David Brewster's notes, it appears that February 10, 11, 12, and 13 of the year 1838 were frosty days; yet the lines were well seen: δ , ϵ , η , C 6, C 15, C 16, and D are specially mentioned. On the 13th, when the thermometer stood at 23° F., ϵ is said to have been the most prominent.

In the paper referred to at the commencement, we, in common with most of those who worked on the subject before the appearance of Kirchhoff and Bunsen's paper⁶, ascribed a bright line coincident with D to other spectra than that of soda. This was no doubt owing to the almost universal trace of that substance.

The electric lights produced between charcoal points by Professor Holmes's magneto-apparatus, and by a galvanic battery with M. Serin's lamp, were found to be identical, when subjected to prismatic analysis. Each exhibited a continuous spectrum, and not those variations of bright and dark which other observers, as well as myself, have noticed in electric lights from charcoal points of but inferior quality. The spectrum extended at both ends beyond that of ordinary direct sunlight, and the only lines which I could discern were bright ones in the violet or lavender region. The following refractive indices were determined for the magneto-electric light; and they are compared with the refractive indices of the nearest principal dark lines of the solar spectrum as determined with the same prism. The relative position

¹ Pogg. Ann. cix. pp. 148, 275; ex. p. 187.

² Ibid. xciv. p. 141.

³ Ibid. cvii. pp. 497, 638.

⁴ See "Experiments and Observations on some cases of Lines in the Prismatic Spectrum produced by the passage of light through coloured vapours and gases, and from certain coloured flames," by Professor W. A. Miller. *Phil. Mag.* August, 1845.

⁵ Pogg. Ann. cx. p. 161.

of these bright rays and the dark lines in the lavender part of the spectrum, which are only visible in the sun's light under the most favourable circumstances, must not be relied on as accurate, since it was determined by measurement, and not by direct comparison. Possible inaccuracy of adjustment will render the fourth place of decimals quite uncertain. The remarkable extension of the spectrum of this electric light, both at the red and violet ends, is, however, indisputable.

Electric Light.	Sunlight.
Limit of red 1'6025 (about)	Line X. . . 1'6038
—	„ A. . . 1'6069
—	„ G. . . 1'6404
Bundle of violet rays . . 1'6436	—
Faint violet ray . . . 1'6516	„ K. . . 1'6513
2nd „ . . . 1'6531 (about)	—
Bright violet ray . . . 1'6550	„ I. . . 1'6548
Faint ray 1'6562 (about)	—
„ 1'6574 (about)	„ L. . . 1'6567
Limit of violet 1'6664 (about)	„ N(1st) 1'6642

—*Proceedings of the Royal Society.*

PROCEEDINGS OF SOCIETIES.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE Thirty-first Annual Meeting of this Association commenced on Wednesday, September 4, at Manchester, under the direction of the following officers:—

President.—William Fairbairn, Esq., LL.D., C.E., F.R.S.

Vice-Presidents.—The Earl of Ellesmere, F.R.G.S.; the Lord Stanley, M.P., D.C.L., F.R.G.S.; the Lord Bishop of Manchester, D.D., F.R.S., F.G.S.; Sir Philip de Malpas Grey Egerton, Bart., M.P., F.R.S., F.G.S.; Sir Benjamin Heywood, Bart., F.R.S.; Thomas Bazley, Esq., M.P.; James Aspinall Turner, Esq., M.P.; James Prescott Joule, Esq., LL.D., F.R.S., President of the Literary and Philosophical Society of Manchester; Joseph Whitworth, Esq., F.R.S., M. Inst. C.E.

General Secretary.—Rev. Robert Walker, M.A., F.R.S., Professor of Experimental Philosophy, Oxford.

Assistant General Secretary.—John Phillips, Esq., M.A., LL.D., F.R.S., F.G.S., Professor of Geology, Oxford.

General Treasurer.—John Taylor, Esq., F.R.S.

Local Secretaries.—Robert Dukinfield Darbishire, Esq., B.A., F.G.S., 21, Brown Street, Manchester; Alfred Neild, Esq., Mayfield, Manchester; Arthur Ransome, Esq., M.A., St. Peter's Square, Manchester; Professor Henry Enfield Roscoe, B.A., Owens College, Manchester.

Local Treasurer.—Robert Phillips Greg, Esq., F.G.S., Manchester.

The Council met at ten o'clock in the morning, and at one o'clock the General Committee held its first meeting in the Town-hall, for the election of sectional officers and the despatch of business.

The first general meeting of the Association was held in the Free Trade Hall, at eight p.m., when at a few minutes after eight Lord WHORRESELEY, the retiring President, took the chair amid loud cheers. He said:—

Ladies and Gentlemen,—In retiring from the office I have had the honour to hold, it is a great pleasure to me to know that I am to be succeeded by one who is so well worthy of your support. We may derive important instruction from the career of Mr. Fairbairn, whether we view him as the successful engineer, or as a distinguished man of science. In the former capacity he is one who has, by perseverance combined with talent, risen from small beginnings to the summit of his profession, and he forms one of that noble class of men, the Stephansons, the Brunels, the Whitworths, the Armstrongs, which have conferred such important

services on their country, and some of whom, unfortunately for that country, have perished, alas, too soon, exhausted by their arduous toils. Mr. Fairbairn is, therefore, one of the many examples of what can be done in England by such men, who resolve, undaunted by the difficulties and obstructions that beset their path, to struggle gallantly onward till success crown their efforts. Again, if we look at Mr. Fairbairn's claims to scientific distinction, they read to us an important lesson; for they show what can be done by zeal and energy, and the exercise of a strong and resolute will, fully determined to carry out objects in which the public is deeply interested. It is extraordinary that any man should have been able during the few leisure hours that can be snatched from an important and engrossing business, to accomplish for science what Mr. Fairbairn has done; and not only has he been a most successful contributor to mechanical science, but his liberality has been unbounded in placing all his great mechanical resources at the disposal of his fellow-labourers in the same field. Well and truly was it said when the Royal medal was conferred on Mr. Fairbairn for his researches:—"Perhaps there is scarcely a single individual living who has made so many careful experimental inquiries on subjects of primary importance to the commercial and manufacturing interests of the country, and has so liberally contributed them to the world." Such a man is one whom all should delight to honour. And to such a man I resign, with great satisfaction, the chair which I now vacate.

Mr. FAIRBAIRN then took the chair amid loud cheers. He spoke as follows:—

Gentlemen of the British Association,—Ever since my election to the high office I now occupy I have been deeply sensible of my own unfitness for a post of so much distinction and responsibility; and when I call to mind the illustrious men who have preceded me in this chair, and see around me so many persons much better qualified for the office than myself, I feel the novelty of my position and unfeigned embarrassment in addressing you. I should, however, very imperfectly discharge the duties which devolve upon me, as the successor of the distinguished nobleman who presided over the meetings of last year, if I neglected to thank you for the honourable position in which you have placed me, and to express at the outset my gratitude to those valued friends with whom I have been united for many years in the labours of the Sections of this Association, and from whom I have invariably received every mark of esteem.

A careful perusal of the history of this Association will demonstrate that it was the first, and for a long time the only institution, which brought together for a common object the learned Professors of our Universities and the workers in practical science. These periodical *réunions* have been of incalculable benefit in giving to practice that soundness of principle and certainty of progressive improvement which can only be obtained by the accurate study of science and its application to the arts. On the other hand, the men of actual practice have reciprocated the benefits thus received from theory, in testing by actual experiment deductions which were doubtful, and rectifying those which were erroneous. Guided by an extended experience, and exercising a sound and disciplined judgment, they have often corrected theories apparently accurate, but nevertheless founded on incomplete data or on false assumptions inadvertently introduced. If the British Association had effected nothing more than the removal of the anomalous separation of theory and practice, it would have gained imperishable renown in the benefit thus conferred. Were I to enlarge on the relation of the achievements of science to the comforts and enjoyments of man, I should have to refer to the present epoch as one of the most important in the history of the world. At no former period did science contribute so much to the uses of life and the wants of society. And in doing this it has only been fulfilling that mission which Bacon, the great father of modern science, appointed for it, when he wrote that "the legitimate goal of the sciences is the endowment of human life with new inventions and riches," and when he sought for a natural philosophy which, not spending its energy on barren disquisitions, "should be operative for the benefit and endowment of mankind." Looking, then, to the fact that, while in our time all the sciences have yielded this fruit, engineering science, with which I have been most intimately connected, has pre-eminently advanced the power, the wealth, and the

comforts of mankind, I shall probably best discharge the duties of the office I have the honour to fill by stating, as briefly as possible, the more recent scientific discoveries which have so influenced the relations of social life. I shall, therefore, not dwell so much on the progress of abstract science, important as that is, but shall rather endeavour briefly to examine the application of science to the useful arts, and the results which have followed, and are likely to follow, in the improvement of the condition of society.

The history of man throughout the gradations and changes which he undergoes in advancing from a primitive barbarism to a state of civilization shows that he has been chiefly stimulated to the cultivation of science and the development of his inventive powers by the urgent necessity of providing for his wants and securing his safety. There is no nation, however barbarous, which does not inherit the germs of civilization, and there is scarcely any which has not done something towards applying the rudiments of science to the purposes of daily life. Among the South Sea Islanders, when discovered by Cook, the Applied Sciences—if I may use the term—were not entirely unknown. They had observed something of the motions of the heavenly bodies, and watched with interest their revolutions, in order to apply this knowledge to the division of time. They were not entirely deficient in the construction of instruments of husbandry, of war, and of music. They had made themselves acquainted with the rudiments of ship-building and navigation in the construction and management of their canoes. Out off from the influence of European civilization, and deprived of intercourse with higher grades of mind, we still find the inherent principle of progression exhibiting itself, and the inventive and reasoning powers developed in the attempt to secure the means of subsistence. Again, if we compare man as he exists in small communities with his condition where large numbers are congregated together, we find that densely-populated countries are the most prolific in inventions, and advance most rapidly in science. Because the wants of the many are greater than those of the few, there is a more vigorous struggle against the natural limitations of supply, a more careful husbanding of resources, and there are more minds at work. This fact is strikingly exemplified in the history of Mexico and Peru; and its attestation is found in the numerous monuments of the past which are seen in Central America, where the remains of cities and temples, and vast public works, erected by a people endowed with high intellectual acquirements, can still be traced. There have been discovered a system of canals for irrigation; long mining galleries cut in the solid rock in search of lead, tin, and copper; pyramids not unlike those of Egypt; earthenware vases and cups, and manuscripts containing the records of their history; all testifying to so high a degree of scientific culture and practical skill that, looking at the cruelties which attended the conquests of Cortes and Pizarro, we may well hesitate as to which had the stronger claims on our sympathy, the victors or the vanquished.

In attempting to notice those branches of science with which I am but imperfectly acquainted, I shall have to claim your indulgence. This Association, as you are aware, does not confine its discussions and investigations to any particular science; and one great advantage of this is, that it leads to the division of labour, while the attention each department receives, and the harmony with which the plan has hitherto worked, afford the best guarantee of its wisdom and proof of its success. In the early history of Astronomy, how vague and unsatisfactory, were the wild theories and conjectures which supplied the place of demonstrated physical truths and carefully observed laws! How immeasurably small, what a very speck, does man appear, with all the wonders of his invention, when contrasted with the mighty works of the Creator; and how imperfect is our apprehension, even in the highest flights of poetic imagination, of the boundless depths of space! These reflections naturally suggest themselves in the contemplation of the works of an Almighty Power, and impress the mind with a reverential awe of the great Author of our existence. The great revolution which laid the foundation of modern astronomy, and which, indeed, marks the birth of modern physical science, is chiefly due to three or four distinguished philosophers. Tycho Brahe, by his system of accurate measurement of the positions of the heavenly bodies; Copernicus, by his history of the solar system; Galileo, by the application of the telescope; and Kepler, by the discovery of the laws of the planetary motions, all assisted in advancing, by prodigious strides, towards a

true knowledge of the constitution of the universe. It remained for Newton to introduce, at a later period, the idea of an attraction varying directly as the mass, and inversely as the square of the distance, and thus to reduce celestial phenomena to the greatest simplicity, by comprehending them under a single law. Without tracing the details of the history of this science, we may notice that in more recent times astronomical discoveries have been closely connected with high mechanical skill in the construction of instruments of precision. The telescope has enormously increased the catalogue of the fixed stars, or those "landmarks of the universe," as Sir John Herschel terms them, "which never deceive the astronomer, navigator, or surveyor." The number of known planets and asteroids has also been greatly enlarged. The discovery of Uranus resulted immediately from the perfection attained by Sir William Herschel in the construction of his telescope. More recently, the structure of the nebulae has been unfolded, through the application to their study of the colossal telescope of Lord Rosse. In all these directions much has been done, both by our present distinguished Astronomer Royal and also by amateur observers in private observatories, all of whom, with Mr. Lassells at their head, are making rapid advances in this department of physical science. Our knowledge of the physical constitution of the central body of our system seems likely, at the present time, to be much increased. The spots on the sun's disc were noticed by Galileo and his contemporaries, and enabled them to ascertain the time of its rotation and the inclination of its axis. They also correctly inferred, from their appearance, the existence of a luminous envelope, in which funnel-shaped depressions revealed a solid and dark nucleus. Just a century ago, Alexander Wilson indicated the presence of a second and less luminous envelope beneath the outer stratum; and his discovery was confirmed by Sir William Herschel, who was led to assume the presence of a double stratum of clouds, the upper intensely luminous, the lower grey, and forming the penumbra of the spots. Observations during eclipses have rendered probable the supposition that a third and outermost stratum of imperfect transparency incloses concentrically the other envelopes. Still more recently, the remarkable discoveries of Kirchhoff and Bunsen require us to believe that a solid or liquid photosphere is seen through an atmosphere containing iron, sodium, lithium, and other metals in a vaporous condition. We must still wait for the application of more perfect instruments, and especially for the careful registering of the appearances of the sun by the photeliograph of Sir John Herschel, so ably employed by Mr. Warren De La Rue, Mr. Welsh, and others, before we can expect a solution of all the problems thus suggested. Guided by the same principles which have been so successful in astronomy, its sister science, Magnetism, emerging from its infancy, has of late advanced rapidly in that stage of development which is marked by assiduous and systematic observation of the phenomena, by careful analysis and presentation of the facts which they disclose, and by the grouping of these in generalisations, which, when the basis on which they rest shall be more extended, will prepare the way for the conception of a general physical theory, in which all the phenomena shall be comprehended, while each shall receive its separate and satisfactory explanation. It is unnecessary to remind you of the deep interest which the British Association has at all times taken in the advancement of this branch of natural knowledge, or of the specific recommendations which, made in conjunction with the Royal Society, have been productive of such various and important results. To refer but to a single instance: we have seen those magnetic disturbances, so mysterious in their origin and so extensive in simultaneous prevalence,—and which, less than twenty years ago, were designated by a term specially denoting that their laws were wholly unknown,—traced to laws of periodical recurrence, revealing, without a doubt, their origin in the central body of our system, by inequalities which have for their respective periods the solar day, the solar year, and, still more remarkably, and until lately unsuspected, solar cycle of about ten of our terrestrial years, to whose existence they bear testimony in conjunction with the solar spots; but whose nature and causes are in all other respects still wrapped in entire obscurity. We owe to General Sabine, especially, the recognition and study of these and other solar magnetic influences, and of the magnetic influence of the moon similarly attested by concurrent determinations in many parts of the globe, which are now held to constitute a distinct branch of this

science, not inappropriately named "celestial," as distinguished from purely terrestrial magnetism.

We ought not in this town to forget that the very rapid advance which has been made in our time in Chemistry is due to the law of equivalents, or Atomic Theory, first discovered by our townsman, John Dalton. Since the development of this law, its progress has been unimpeded, and it has had a most direct bearing on the comforts and enjoyments of life. A knowledge of the constituents of food has led to important deductions as to the relative nutritive value and commercial importance of different materials. Water has been studied in reference to the deleterious impurities with which it is so apt to be contaminated in its distribution to the inhabitants of large towns. The power of analysis, which enables us to detect adulterations, has been invaluable to the public health, and would be much more so if it were possible to obviate the difficulties which have prevented the operation of recent legislation on this subject. We have another proof of the utility of this science in its application to medicine; and the estimation in which it is held by the medical profession is the true index of its value in the diagnosis and treatment of disease. The largest developments of chemistry, however, have been in connexion with the useful arts. What would now be the condition of calico printing, bleaching, dyeing, and even agriculture itself, if they had been deprived of the aid of theoretic chemistry? For example, aniline—first discovered in coal tar by Dr. Hoffman, who has so admirably developed its properties—is now most extensively used as the basis of red, blue, violet, and green dyes. This important discovery will probably in a few years render this country independent of the world for dye stuffs; and it is more than probable that England, instead of drawing her dye stuffs from foreign countries, may herself become the centre from which all the world will be supplied. It is an interesting fact that at the same time, in another branch of this science, M. Tournet has lately demonstrated that the colours of gems, such as the emerald, aquamarine, amethyst, smoked rock-crystal, and others, are due to volatile hydro-carbons, first noticed by Sir David Brewster in clouded topaz, and that they are not derived from metallic oxides, as has been hitherto believed. Another remarkable advance has recently been made by Bunsen and Kirchhoff in the application of the coloured rays of the prism to analytical research. We may consider their discoveries as the commencement of a new era in analytical chemistry, from the extraordinary facilities they afford in the qualitative detection of the minutest traces of elementary bodies. The value of this method has been proved by the discovery of the new metals, cesium and rubidium, by M. Bunsen; and it has yielded another remarkable result in demonstrating the existence of iron and six other known metals in the sun. In noticing the more recent discoveries in this important science, I must not pass over in silence the valuable light which chemistry has thrown upon the composition of iron and steel. Although Despretz demonstrated many years ago that iron would combine with nitrogen, yet it was not until 1857 that Mr. C. Binks proved that nitrogen is an essential element of steel; and more recently M. Caron and M. Frémy have further elucidated this subject; the former showing that cyanogen, or cyanide of ammonium, is the essential element which converts wrought iron into steel; the latter combining iron with nitrogen through the medium of ammonia, and then converting it into steel by bringing it at the proper temperature into contact with common coal gas. There is little doubt that in a few years these discoveries will enable Sheffield manufacturers to replace their present uncertain, cumbersome, and expensive process by a method at once simple and inexpensive, and so completely under control as to admit of any required degree of conversion being obtained with absolute certainty. Mr. Grace Calvert, also, has proved that cast iron contains nitrogen, and has shown that it is a definite compound of carbon and iron, mixed with various proportions of metallic iron, according to its nature. Before leaving chemical science, I must refer to the interesting discovery by M. Deville, by which he succeeded in rapidly melting thirty-eight or forty pounds of platinum—a metal till then considered almost infusible. This discovery will render the extraction of platinum from the ore more perfect, and, by reducing its cost, will greatly facilitate its application to the arts.

It is little more than half a century since Geology assumed the distinctive character of a science. Taking into consideration the aspects of Nature in different epochs of the history

of the earth, it has been found that the study of the change at present going on in the world around us enables us to understand the past revolutions of the globe, and the conditions and circumstances under which strata have been formed, and organic remains embedded and preserved. The geologist has increasingly tended to believe that the changes which have taken place on the face of the globe, from the earliest times to the present, are the result of agencies still at work. But, while it is his high office to record the distribution of life in past ages, and the evidence of physical changes in the arrangement of land and water, his results hitherto have indicated no traces of its beginning, nor have they afforded evidence of the time of its future duration. Geology has been indebted for this progress very largely to the investigations of Sedgwick and the writings of Sir Charles Lyell. As an example of the application of geology to the practical uses of life, I may cite the discovery of the gold-fields of Australia, which might long have remained hidden but for the researches of Sir Roderick Murchison in the Ural Mountains on the geological position of the strata from which the Russian gold is obtained. From this investigation he was led by inductive reasoning to believe that gold would be found in similar rocks, specimens of which had been sent him from Australia. The last years of the active life of this distinguished geologist have been devoted to the re-examination of the rocks of his native highlands of Scotland. Applying to them those principles of classification which he long since established, he has demonstrated that the crystalline limestone and quartz rocks which are associated with mica schists, &c., belong by their embedded organic remains to the Lower Silurian rocks. Descending from this well-marked horizon, he shows the existence beneath all such fossiliferous strata of vast masses of sand-stone and conglomerate of Cambrian age; and, lastly, he has proved the existence of a fundamental gneiss, on which all the other rocks repose, and which, occupying the north-west Hebrides, and the west coasts of Sutherland and Ross, is the oldest rock formation on the British Isles, it being unknown in England, Wales, or Ireland.

It is well known that the temperature increases as we descend through the earth's crust, from a certain point near the surface, at which the temperature is constant. In various mines, borings, and artesian wells the temperature has been found to increase about one degree Fahrenheit for every sixty or sixty-five feet of descent. In some carefully conducted experiments during the sinking of Dukinfield Deep Mine—one of the deepest pits in this country—it was found that a mean increase of about one degree in seventy-one feet occurred. If we take the ratio thus indicated, and assume it to extend to much greater depths, we should reach at two and a-half miles from the surface, strata at the temperature of boiling water; and at depths of about fifty or sixty miles the temperature would be sufficient to melt, under the ordinary pressure of the atmosphere, the hardest rocks. Reasoning from these facts, it would appear that the mass of the globe, at no great depth, must be in a fluid state. But this deduction requires to be modified by other considerations, namely, the influence of pressure on the fusing point and the relative conductivity of the rocks which form the earth's crust. To solve these questions a series of important experiments were instituted by Mr. Hopkins, in the prosecution of which Dr. Joule and myself took part; and after a long and laborious investigation it was found that the temperature of fluidity increased about one degree Fahrenheit for every five hundred pounds of pressure in the case of spumaeeti, bees'-wax, and other similar substances. However, on extending these experiments to less compressible substances, such as tin and barytes, a similar increase was not observed. But this series of experiments has been unavoidably interrupted; nor is the series on the conductivity of rocks entirely finished. Until they have been completed by Mr. Hopkins, we can only make a partial use of them in forming an opinion of the thickness of the earth's solid crust. Judging, however, alone from the greater conductivity of the igneous rocks, we may calculate that the thickness cannot possibly be less than nearly three times as great as that calculated on the usual suppositions of the conductive power of the terrestrial mass at enormous depths being no greater than that of the superficial sedimentary beds. Other modes of investigation which Mr. Hopkins has brought to bear on this question appear to lead to the conclusion that the thickness of the earth's crust is much greater even than that above stated. This would require us to assume that a part of the

heat in the crust is due to superficial and external rather than central causes. This does not bear directly against the doctrine of central heat, but shows that only a part of the increase of temperature observed in mines and deep wells is due to the outward flow of that heat.

Touching those highly interesting branches of science, Botany and Zoology, it may be considered presumptuous in me to offer any remarks. I have, however, not entirely neglected, in my earlier days, to inform myself of certain portions of natural history, which cannot but be attractive to all who delight in the wonderful beauties of natural objects. How interesting is the organization of animals and plants! how admirably adapted to their different functions and spheres of life! They want nothing, yet have nothing superfluous. Every organ is adapted perfectly to its functions; and the researches of Owen, Agassiz, Darwin, Hooker, Daubeny, Babington, and Jardine, fully illustrate the perfection of the animal and vegetable economy of nature.

Two other important branches of scientific research, Geography and Ethnology, have for some years been united, in this Association, in one section, and that probably the most attractive and popular of them all. We are much indebted to Sir Roderick Murchison, among other members of the Association, for its continued prosperity, and the high position it has attained in public estimation. The spirit of enterprise, courage and perseverance displayed by our travellers in all parts of the world have been powerfully stimulated and well supported by the Royal Geographical Society; and the prominence and rapid publicity given to discoveries by that body have largely promoted geographical research. In Physical Geography, the late Baron von Humboldt has been one of the largest contributors, and we are chiefly indebted to his personal researches and numerous writings for the elevated position it now holds among the sciences. To Humboldt we owe our knowledge of the physical features of Central and Southern America. To Parry, Sir James Ross and Scoresby we are indebted for discoveries in the Arctic and Antarctic regions. Geography has also been advanced by the first voyage of Franklin down the Copper Mine River, and along the inhospitable shores of the Northern Seas, as far as Point Turnagain; as also by that ill-fated expedition in search of a North-West passage, followed by others in search of the unfortunate men who perished in their attempt to reach those ice-bound regions, so often stimulated by the untiring energy of a high-minded woman. In addition to these, the discoveries of Dr. Livingstone in Africa have opened to us a wide field of future enterprise along the banks of the Zambesi and its tributaries. To these, we may add the explorations of Captain Burton in the same continent, and those also by Captain Speke and Captain Grant of a hitherto unknown region, in which it has been suggested that the White Nile has its source, flowing from one of two immense lakes, upwards of 300 miles long by 100 broad, and situated at an elevation of 4000 feet above the sea. To these remarkable discoveries I ought to add an honourable mention of the sagacious and perilous exploration of Central and Northern Australia by Mr. McDouall Stuart.

Having glanced, however imperfectly, at some of the most important branches of science which engage the attention of Members of this Association, I would now invite attention to the Mechanical Sciences, with which I am more familiarly acquainted. They may be divided into Theoretical Mechanics and Dynamics, comprising the conditions of equilibrium and the laws of motion; and Applied Mechanics, relating to the construction of machines. I have already observed that Practice and Theory are twin sisters, and must work together to ensure a steady progress in mechanical art. Let us, then, maintain this union as the best and safest basis of national progress, and, moreover, let us recognise it as one of the distinctive aims of the annual *réunions* of this Association. During the last century, the science of Applied Mechanics has made strides which astonish us by their magnitude; but even these, it may reasonably be hoped, are but the promises of future and more wonderful enlargements. I, therefore, propose to offer a succinct history of these improvements, as an instance of the influence of scientific progress on the well-being of society. I shall take in review the three chief aids which engineering science has afforded to national progress, namely, canals, steam navigation and railways; each of which has promoted an incalculable extension of the industrial resources of the country. One hundred years ago, the only means for the conveyance of inland merchandise, were the packhorses and waggons on the then imperfect

highways. It was reserved for Brindley, Smeaton and others to introduce a system of canals, which opened up facilities for an interchange of commodities at a cheap rate over almost every part of the country. The impetus given to industrial operations by this new system of conveyance induced capitalists to embark in trade, in mining, and in the extension of manufactures in almost every district. These improvements continued for a series of years, until the whole country was intersected by canals, requisite to meet the demands of a greatly extended industry. But canals, however well adapted for the transport of minerals and merchandise, were less suited for the conveyance of passengers. The speed of the canal boats seldom exceeded from 2½ to 3 miles an hour; and, in addition to this, the projectors of canals sometimes sought to take an unfair advantage of the Act of Parliament, which fixed the tariff at so much per ton per mile, by adopting circuitous routes, under the erroneous impression that mileage was a consideration of great importance in the success of such undertakings. It is in consequence of short-sighted views and imperfect legislation that we inherit the numerous curves and distortions of our canal system. These defects in construction rendered canals almost useless for the conveyance of passengers, and led to the improvement of the common roads and the system of stage coaches so that before the year 1830 the chief public highways of the country had attained a remarkable smoothness and perfection; and the lightness of our carriages, and the celerity with which they were driven, still excite the admiration of those who remember them. These days of an efficiently worked system, which tasked the power and speed of the horse to the utmost, have now been succeeded by changes more wonderful than any that previously occurred in the history of the human race.

Scarcely had the canal system been fully developed when a new means of propulsion was adopted—namely, steam. I need not recount to you the enterprise, skill, and labour that have been exerted in connexion with steam navigation. You have seen its results on every river and every sea; results we owe to the fruitful minds of Miller, Symington, Fulton, and Henry Bell, who were the pioneers in the great march of progress. Viewing the past, with a knowledge of the present and a prospect of the future, it is difficult to estimate sufficiently the benefits that have been conferred by this application of mechanical science to the purposes of navigation. Power, speed, and certainty of action have been attained on the most gigantic scale. The celerity with which a modern steamer, with a thousand tons of merchandise and some hundreds of human beings on board, cleaves the water and pursues her course, far surpasses the most sanguine expectations of a quarter of a century ago, and indeed almost rivals the speed of the locomotive itself. Previous to 1812 our intercourse with foreign countries and with our colonial possessions depended entirely upon the state of the weather. It was only in favourable seasons that a passage was open, and we had often to wait days, or even a week, before Dublin could be reached from Holyhead. Now this distance of sixty-three miles is accomplished in all weathers in little more than three hours. The passage to America used to occupy six weeks or two months; now it is accomplished in eight or nine days. The passage round the Cape to India is reduced from nearly half a year to less than a third of that time, while that country may be reached by the overland route in less than a month.

These are a few of the benefits derived from steam navigation, and, as it is yet far from perfect, we may reasonably calculate on still greater advantages in our intercourse with distant nations. I will not here enter on the subject of the numerous improvements which have so rapidly advanced the progress of this important service. Suffice it to observe that the paddle-wheel system of propulsion has maintained its superiority over every other method yet adopted for the attainment of speed, as by it the best results are obtained with the least expenditure of power. In ships of war the screw is indispensable, on account of the security it affords to the engines and machinery, from their position in the hold below the water-line, and because of the facility it offers in the use of sails, when the screw is raised from its position in the well to a recess in the stern prepared for that purpose. It is also preferable in ships which require auxiliary power in calms and adverse winds, so as to expedite the voyage and effect a considerable saving upon the freight.

The public mind had scarcely recovered itself from the changes which steam navigation had caused, and the impulse

it had given to commerce, when a new and even more gigantic power of locomotion was inaugurated. Less than a quarter of a century had elapsed since the first steamboats floated on the waters of the Hudson and the Clyde, when the achievements thence resulting were followed by the application of the same agency to the almost superhuman flight of the locomotive and its attendant train. I well remember the competition at Rainhill in 1825, and the incredulity everywhere evinced at the proposal to run locomotives at twenty miles an hour. Neither George Stephenson himself, nor anyone else, had at that time the most distant idea of the capabilities of the railway system. On the contrary, it was generally considered impossible to exceed ten or twelve miles an hour; and our present high velocities, due to high-pressure steam and the tubular system of boilers, have surpassed the most sanguine expectations of engineers. The sagacity of George Stephenson at once seized upon the suggestions of Henry Booth, to employ tubular boilers; and that, united to the blast-pipe, previously known, has been the means of effecting all the wonders we now witness in a system that has done more for the development of practical science, and the civilization of man, than any discovery since the days of Adam.

From a consideration of the changes which have been effected in the means for the interchange of commodities, I pass on to examine the progress which has been made in their production. And, as the steam-engine has been the basis of all our modern manufacturing industry, I shall glance at the steps by which it has been perfected. Passing over the somewhat mythical fame of the Marquis of Worcester and the labours of Savery, Beighton, and Newcomen, we come at once to discuss the state of mechanical art at the time when James Watt brought his gigantic powers to the improvement of the steam-engine. At that time the tools were of the rudest construction, nearly everything being done by hand, and, in consequence, wood was much more extensively employed than iron. Under these circumstances Watt invented separate condensation, rendered the engine double-acting, and converted its rectilinear motion into a circular one, suitable for the purposes of manufacture. But the discovery at first made little way, the public did not understand it, and a series of years elapsed before the difficulties, commercial and mechanical, which opposed its application, could be overcome. When the certainty of success had been demonstrated, Watt was harassed by infringements of his patent, and lawsuits for the maintenance of his rights. Inventors, and pretended inventors, set up claims, and entered into combination with manufacturers, miners, and others, to destroy the patent and deprive him of the just fruits of his labour and genius. Such is the selfish heartlessness of mankind in dealing with discoveries not their own, but from which they expect to derive benefit. The steam-engine, since it was introduced by Watt, has changed our habits in almost every condition of life. Things which were luxuries have become necessities, and it has given to the poor man in all countries in which it exists a degree of comfort and independence and a participation in intellectual culture unknown before its introduction. It has increased our manufactures tenfold, and has lessened the barriers which time and space interpose. It ploughs the land and winnows and grinds the corn. It spins and weaves our textile fabrics. In mining it pumps, winds, and crushes the ore. It performs these things with powers so great and so energetic as to astonish us at their immensity, while they are at the same time perfectly docile, and completely under human control. In war it furnishes the means of aggression, as in peace it affords the bonds of conciliation, and, in fact, places within reach a power which, properly applied, produces harmony and good will among men, and leads to the happiest results in every condition of human existence. We may, therefore, well be proud of the honour conferred on this country as the cradle of its origin, and as having fostered its development from its earliest applications to its present high state of perfection. I cannot conclude this notice of the steam-engine without observing the changes it is destined to effect in the cultivation of the soil. It is but a short time since it was thought inapplicable to agricultural purposes from its great weight and expense. But more recent experience has proved this to be a mistake, and already in most districts we find that it has been pressed into the service of the farm. The small locomotive, mounted on a frame with four wheels, travels from village to village with its attendant, the thrashing machine,

performing the operations of thrashing, winnowing, and cleansing, at less than one-half the cost by the old and tedious process of hand-labour. Its application to ploughing and tillage on a large scale is, in my opinion, still in its infancy, and I doubt not that many members of this Association will live to see the steam-plough in operation over the whole length and breadth of the land. Much has to be done before this important change can be successfully accomplished; but, with the aid of the agriculturist preparing the land so as to meet the requirements of steam machinery, we may reasonably look forward to a new era in the cultivation of the soil. The extraordinary developments of practical science in our system of textile manufacture are, however, not entirely due to the steam-engine, although they are now in a great measure dependent on it. The machinery of these manufactures had its origin before the steam-engine had been applied, except for mining purposes; and the inventions of Arkwright, Hargreaves, and Crompton were not conceived under the impression that steam would be their moving power. On the contrary, they depended upon water; and the cotton machinery of this district had attained considerable perfection before steam came to the aid of the manufacturer, and ultimately enabled him to increase the production to its present enormous extent.

I shall not attempt a description of the machinery of the textile manufactures, because ocular inspection will be far more acceptable. I can only refer you to a list of establishments in which you may examine their operations on a large scale, and which I earnestly recommend to your attention. I may, however, advert to a few of the improvements which have marked the progress of the manufacturing system in this country. When Arkwright patented his water-frames in 1767, the annual consumption of cotton was about 4,000,000lb. weight. Now it is 1,200,000,000lb. weight—300 times as much. Within half a century the number of spindles at work, spinning cotton alone, has increased ten-fold; while, by superior mechanism, each spindle produces fifty per cent. more yarn than on the old system. Hence the importance to which the cotton trade has risen, equalling at the present time the whole revenue of the three kingdoms, or 70,000,000*l.* sterling per annum. As late as 1820 the power-loom was not in existence; now it produces about 14,000,000 yards of cloth, or, in more familiar terms, nearly 8000 miles of cloth per diem. I give these particulars to show the immense power of production of this country, and to afford some conception of the number and quality of the machines which effect such wonderful results. Mule spinning was introduced by Crompton, in 1787, with about twenty spindles to each machine. The powers of the machine were, however, rapidly increased; and now it has been so perfected that 2000 or even 3000 spindles are directed by a single person. At first the winding on, or forming the shape of the cop, was performed by hand; but this has been superseded by rendering the machine automatic, so that it now performs the whole operation of drawing, stretching, and twisting the thread, and winding it on to the exact form, ready for the reel or shuttle as may be required. These and other improvements in carding, roving, combing, spinning, and weaving, have established in this country an entirely new system of industry; it has given employment to greatly increased numbers, and a more intelligent class of work-people. Similarly important improvements have been applied to the machinery employed in the manufacture of silk, flax, and wool; and we have only to watch the processes in these different departments to be convinced that they owe much to the development of the cotton manufacture. In the manufacture of worsted, the spinning-jenny was not employed at Bradford until 1790, nor the power-loom until about 1825. The production of fancy or mixed goods from alpaca and mohair wool, introduced to this country in 1836, is, perhaps, the most striking example of a new creation in the art of manufacture, and is chiefly due to Mr. Titus Salt, in whose immense palace of industry, at Saltaire, it may be seen in the greatest perfection. In flax machinery, the late Sir Peter Fairbairn was one of the most successful inventors, and his improvements have contributed to the rapid extension of this manufacture.

I might greatly extend this description of our manufacturing industry, but I must for the present be brief, in order to point out the dependence of all these improvements on the iron and coal so widely distributed among the mineral treasures of our island. We are highly favoured in the abundance of these minerals, deposited with an un-

sparing hand by the great Author of Nature, under so slight a covering as to bring them within reach of the miner's art. To them we owe our present high state of perfection in the useful arts; and to their extended application we may safely attribute our national progress and wealth. So that, looking to the many blessings which we daily and hourly receive from these sources alone, we are impressed with devotional feelings of gratitude to the Almighty for the manifold bounties He has bestowed on us. Previously to the inventions of Henry Cort, the manufacture of wrought iron was of the most crude and primitive description. A hearth and a pair of bellows was all that was employed. But since the introduction of puddling, the iron-masters have increased the production to an extraordinary extent, down to the present time, when processes for the direct conversion of wrought iron on a large scale are being attempted. A consecutive series of chemical researches into the different processes, from the calcining of the ore to the production of the bar, carried on by Dr. Percy and others, has led to a revolution in the manufacture of iron; and although it is at the present moment in a state of transition, it nevertheless requires no very great discernment to perceive that steel and iron of any required tenacity will be made in the same furnace, with a facility and certainty never before attained. This has been effected to some extent by improvements in puddling; but the process of Mr. Bessemer—first made known at the meeting of this Association at Cheltenham—affords the highest promise of certainty and perfection in the operation of converting the melted pig direct into steel or iron, and as likely to lead to the most important developments in this manufacture. These improvements in the production of the material must, in their turn, stimulate its application on a large scale and lead to new constructions.

In iron shipbuilding an immense field is opening before us. Our wooden walls have to all appearance seen their last days; and as one of the early pioneers in iron construction, as applied to shipbuilding, I am highly gratified to witness a change of opinion that augurs well for the security of the liberties of the country. From the commencement of iron shipbuilding in 1830 to the present time, there could only be one opinion among those best acquainted with the subject, namely, that iron must eventually supersede timber in every form of naval construction. The large ocean steamers, the *Himalaya*, the *Persia*, and the *Great Eastern*, abundantly show what can be done with iron, and we have only to look at the new system of casing ships with armour plates, to be convinced that we can no longer build wooden vessels of war with safety to our naval superiority and the best interests of the country. I give no opinion as to the details of the reconstruction of the Navy—that is reserved for another place—but I may state that I am fully persuaded that the whole of our ships of war must be rebuilt of iron, and defended with iron armour calculated to resist projectiles of the heaviest description at high velocities. In the earlier stages of iron shipbuilding, I believe I was the first to show, by a long series of experiments, the superiority of wrought iron over every other description of material in security and strength, when judiciously applied in the construction of ships of every class. Other considerations, however, affect the question of vessels of war; and although numerous experiments were made, yet none of the targets were on a scale sufficient to resist more than a six-pounder shot. It was reserved for our scientific neighbours, the French, to introduce thick iron plates as a defensive armour for ships. The success which has attended the adoption of this new system of defence affords the prospect of invulnerable ships of war, and hence the desire of the Government to remodel the Navy on an entirely new principle of construction, in order that we may retain its superiority as the great bulwark of the nation. A committee has been appointed by the War-office and the Admiralty for the purpose of carrying out a scientific investigation of the subject, so as to determine, first, the best description of material to resist projectiles; secondly, the best method of fastening and applying that material to the sides of ships and land fortifications; and, lastly, the thickness necessary to resist the different descriptions of ordnance.

It is asserted, probably with truth, that whatever thickness of plates is adopted for casing ships, guns will be constructed capable of destroying them. But their destruction will even then be a work of time, and I believe, from what I have seen in recent experiments, that with proper armour it would require, not only the most powerful ordnance, but also a great

concentration of fire, before fracture will ensue. If this be the case, a well-constructed iron ship, covered with sound plates of the proper thickness, firmly attached to its sides, will, for a considerable time, resist the heaviest guns which can be brought to bear against it, and be practically shot-proof. But our present means are inadequate for the production of large masses of iron, and we may trust that, with new tools and machinery, and the skill, energy, and perseverance of our manufacturers, every difficulty will be overcome, and armour plates produced which will resist the heaviest existing ordnance. The rifling of heavy ordnance, the introduction of wrought iron, and the new principle of construction with strained hoops, have given to all countries the means of increasing enormously the destructive power of their ordnance. One of the results of this introduction of wrought iron and correct principles of manufacture, is the reduction of the weight of the new guns to about two-thirds the weight of the older cast-iron ordnance. Hence follows the facility with which guns of much greater power can be worked, while the range and precision of fire are at the same time increased. But these improvements cannot be confined to ourselves. Other nations are increasing the power and range of their artillery in a similar degree, and the energies of the nation must, therefore, be directed to maintain the superiority of our Navy in armour as well as in armament.

We have already seen a new era in the history of the construction of bridges, resulting from the use of iron; and we have only to examine those of the tubular form over the Conway and Menai Straits to be convinced of the durability, strength, and lightness of tubular constructions applied to the support of railways or common roads in spans which ten years ago were considered beyond the reach of human skill. When it is considered that stone bridges do not exceed 150 feet in span, nor cast-iron bridges 250 feet, we can estimate the progress which has been made in crossing rivers 400 or 500 feet in width, without any support at the middle of the stream. Even spans greatly in excess of this may be bridged over with safety, provided we do not exceed 1800 to 2000 feet, when the structure would be destroyed by its own weight.

It is to the exactitude and accuracy of our machine tools that our machinery of the present time owes its smoothness of motion and certainty of action. When I first entered this city the whole of the machinery was executed by hand. There were neither planing, slotting, nor shaping-machines, and, with the exception of very imperfect lathes and a few drills, the preparatory operations of construction were effected entirely by the hands of the workmen. Now everything is done by machine tools with a degree of accuracy which the unaided hand could never accomplish. The automaton, or self-acting machine tool, has within itself an almost creative power; in fact, so great are its powers of adaptation, that there is no operation of the human hand that it does not imitate. For many of these improvements the country is indebted to the genius of our townsmen, Mr. Richard Roberts and Mr. Joseph Whitworth. The importance of these constructive machines is, moreover, strikingly exemplified in the Government Works at Woolwich and at Enfield, chiefly arranged under the direction of Mr. Anderson, the present inspector of machinery, to whose skill and ingenuity the country is greatly indebted for the efficient state of those great arsenals.

Among the changes which have largely contributed to the comfort and enjoyment of life, are the improvements in the sanitary condition of towns. These belong probably to the province of social rather than mechanical science; but I cannot omit to notice some of the great works that have of late years been constructed for the supply of water and for the drainage of towns. In former days 10 gallons of water to each person per day was considered an ample allowance. Now, 30 gallons is much nearer the rate of consumption. I may instance the water-works of this city and of Liverpool, each of which yield a supply of from 20 to 30 gallons of water to each inhabitant. In the former case the water is collected from the Cheshire and Derbyshire hills, and, after being conveyed in tunnels and aqueducts a distance of 10 miles to a reservoir, where it is strained and purified, it is ultimately taken a further distance of 8 miles in pipes, in a perfectly pure state, ready for distribution. The greatest undertaking of this kind, however, yet accomplished is that by which the pure waters of Loch Katrine are distributed to the city of Glasgow. This work, recently completed by Mr. Bateman, who was also the constructor of the waterworks of this city, is of the most gigantic character, the water being

conveyed in a covered tunnel a distance of 27 miles through an almost impassable country to the service reservoir, about 8 miles from Glasgow. By this means 40 million gallons of water per day are conveyed through the hills which flank Ben Lomond, and after traversing the sides of Loch Chon and Loch Aird, are finally discharged into the Mudgock Basin, where the water is impounded for distribution. We may reasonably look forward to an extension of similar benefits to the metropolis, by the same engineer, whose energies are now directed to an examination of the pure fountains of Wales, from whence the future supply of water to the great city is likely to be derived. A work of so gigantic a character may be looked upon as problematical, but when it is known that six or seven millions of money would be sufficient for its execution, I can see no reason why an undertaking of so much consequence to the health of London should not ultimately be accomplished.

In leaving this subject, I cannot refrain from an expression of deep regret at the loss which science has sustained through the death of one of our Vice-Presidents, the late Professor Hodgkinson. For a long series of years he and I worked together in the same field of scientific research, and our labours are recorded in the *Transactions* of this and other Associations. To Mr. Hodgkinson we owe the determination of the true form of cast iron beams, or section of greatest strength; the law of the elasticity of iron under tensile and compressive forces; and the laws of resistance of columns to compression. I look back to the days of our joint labour with unalloyed pleasure and satisfaction.

I regret to say that another of our Vice-Presidents, my friend, Mr. Joseph Whitworth, is unable to be present with us through serious, but I hope not dangerous, illness. To Mr. Whitworth mechanical science is indebted for some of the most accurate and delicate pieces of mechanism ever executed; and the exactitude he has introduced into every mechanical operation will long continue to be the admiration of posterity. His system of screw threads and gauges is now in general use throughout Europe. We owe to him a machine for measuring with accuracy to the millionth of an inch, employed in the production of standard gauges; and his laborious and interesting experiments on rifled ordnance have resulted in the production of a rifled small arm and gun which have never been surpassed for range and precision of fire. It is with pain that I have to refer to the cause which deprives me of his presence and support at this meeting.

A brief allusion must be made to that marvellous discovery which has given to the present generation the power to turn the spark of heaven to the uses of speech, to transmit along the slender wire for a thousand miles a current of electricity that renders intelligible words and thoughts. This wonderful discovery, so familiar to us, and so useful in our communications to every part of the globe, we owe to Wheatstone, Thomson, De la Rive, and others. In land telegraphy the chief difficulties have been surmounted, but in submarine telegraphy much remains to be accomplished. Failures have been repeated so often as to call for a Commission on the part of the Government to inquire into the causes, and the best means of overcoming the difficulties which present themselves. I had the honour to serve on that Commission, and I believe that from the report, and mass of evidence and experimental research accumulated, the public will derive very important information. It is well known that three conditions are essential to success in the construction of ocean telegraphs—perfect insulation, external protection, and appropriate apparatus for laying the cable safely on its ocean bed. That we are far from having succeeded in fulfilling these conditions is evident from the fact that out of 12,000 miles of submarine cable which have been laid since 1851, only 3000 miles are actually in working order; so that three-fourths may be considered as a failure and loss to the country. The insulators hitherto employed are subject to deterioration from mechanical violence, from chemical decomposition or decay, and from the absorption of water; but the last circumstance does not appear to influence seriously the durability of cables. Electrically, india-rubber possesses high advantages, and, next to it, Wray's compound and pure gutta percha far surpass the commercial gutta percha hitherto employed; but it remains to be seen whether the mechanical and commercial difficulties in the employment of these new materials can be successfully overcome. The external protecting covering is still a subject of anxious consideration. The objections to iron wire are its weight and liability to corrosion. Hemp has been sub-

stituted, but at present with no satisfactory result. All these difficulties, together with those connected with the coiling and paying out of the cable, will no doubt yield to careful experiment and the employment of proper instruments in its construction, and its final deposit on the bed of the ocean. Irrespective of inland and international telegraphy, a new system of communication has been introduced by Professor Wheatstone, whereby intercourse can be carried on between private families, public offices, and the works of merchants and manufacturers. This application of electric currents cannot be too highly appreciated, from its great efficiency and comparatively small expense. To show to what an extent this improvement has been carried, I may state that 1000 wires in a perfect state of insulation, may be formed into a rope not exceeding half-an-inch in diameter.

I must not sit down without directing attention to a subject of deep importance to all classes,—namely, the amount of protection inventors should receive from the laws of the country. It is the opinion of many that patent laws are injurious rather than beneficial, and that no legal protection of this kind ought to be granted: in fact, that a free trade in inventions, as in everything else should be established. I confess I am not of that opinion. Doubtless there are abuses in the working of the patent law as it at present exists, and protection is often granted to pirates and impostors, to the detriment of real inventors. This, however, does not contravene the principle of protection, but rather calls for reform and amendment. It is asserted by those who have done the least to benefit their country by inventions, that a monopoly is injurious, and that if the patent laws are defended, it should be, not on the ground of their benefit to the inventor, but on that of their utility to the nation. I believe this to be a dangerous doctrine, and I hope it will never be acted upon. I cannot see the right of the nation to appropriate the labours of a lifetime, without awarding any remuneration. The nation, in this case, receives a benefit; and assuredly the labourer is worthy of his hire. I am no friend of monopoly, but neither am I a friend of injustice; and I think that before the public are benefited by an invention, the inventor should be rewarded either by a fourteen years' monopoly, or in some other way. Our patent laws are defective, so far as they protect pretended inventions; but they are essential to the best interests of the State in stimulating the exertions of a class of eminent men, such as Arkwright, Watt, and Crompton, whose inventions have entailed upon all countries invaluable benefits, and have done honour to the human race. To this Association is committed the task of correcting the abuses of the present system, and establishing such legal provisions as shall deal out equal justice to the inventor and the nation at large.

I must not forget that we owe very much to an entirely new and most attractive method of diffusing knowledge, admirably exemplified in the Great Exhibition of 1851, and its successors in France, Ireland and America. Most of us remember the gems of Art which were accumulated in this city during the summer of 1857, and the wonderful results they produced on all classes of the community. The improvement of taste, and the increase of practical knowledge which followed these Exhibitions have been deeply felt; and hence the prospects which are now opening before us in regard to the Exhibition of the next year cannot be too highly appreciated. That Exhibition will embrace the whole circle of the sciences, and is likely to elevate the general culture of the public to a higher standard than we have ever before attained. There will be unfolded almost every known production of art, every ingenious contrivance in machinery, and the results of discoveries in science from the earliest period. The fine arts, which constituted no part of the Exhibition of 1851, and which were only partially represented at Paris and Dublin, will be illustrated by new creations from the most distinguished masters of the modern school. Looking forwards, I venture to hope for a great success and a further development of the principle advocated by this Association—the union of Science and Art.

In conclusion, my apologies are due to you for the length of this address, and I thank you sincerely for the patient attention with which you have listened to the remarks I have had the honour to lay before you. As the President of the British Association, I feel that far beyond the consideration of merely personal qualifications, my election was intended as a compliment to practical science, and to this great and influential metropolis of manufacture; where those

who cultivate the theory of science may witness, on its grandest scale, its application to the industrial arts. As a citizen of Manchester, I venture to assure the Association that its intentions are appreciated; and to its members, as well as to the strangers who have been attracted here by this meeting, I offer a most cordial welcome.

On Thursday, the 5th inst., the Sectional Meetings commenced. The Sections are the following:—

A.—Mathematical and Physical Science.—*President*: G. B. Airey, Esq., D.C.L., F.R.S., Astronomer Royal.
B.—Chemical Science.—*President*: W. A. Miller, M.D., F.R.S., Professor of Chemistry, King's College, London.

C.—Geology.—*President*: Sir R. I. Murchison, G.C.St.S., D.C.L., F.R.S., Director-General of the Geological Survey of the United Kingdom.

D.—Zoology and Botany.—*President*: C. C. Babington, Esq., M.A., F.R.S., Professor of Botany, Cambridge.

E.—Sub-Section Physiology.

F.—Economic Science.—*President*: John Craufurd, Esq., F.R.S., President of the Ethnological Society.

G.—Mechanical Science.—*President*: John F. Bateman, Esq., C.E., F.R.S.

NOTICES OF BOOKS.

A Guide to Croft Spa. By Miss D. WILKINSON. Darlington: Times' Office. 1861.

At this season of the year the very word Spa has a refreshing look and sound to the overtaken inhabitant of a crowded city. It speaks of shady walks by bubbling streams, through leafy woods just tinged and crisped by autumn winds; and rest and solace for a jaded mind, with fresh health and vigour for a hard-worked frame. But, as chemists and reviewers, we must avoid sentiment, and proceed to business.

Croft deserves to be better known. It may be placed at the head of the list of English *Thiopege*s; and, indeed, in respect of the amount of sulphuretted hydrogen contained in the water of one spring, it may rank before all the Continental spas except Schwyznach in Switzerland.

The village lies in a sheltered valley on the south bank of the Tees at the very edge of Yorkshire. On a gravelly soil, in the midst of a rich, luxuriant, well-cultivated country, and defended on all sides from inclement breezes, the air is dry, and pure, and mild, while the scenery around the village, if wanting in the grander aspects of Nature, presents that charming combination of hill and dale with winding stream, wood, pasture-land, and corn-field, which makes up the "quiet, delicate beauty" of an English landscape.

But the mineral springs are, of course, the principal attraction at Croft. There are four of them, three containing sulphuretted hydrogen, and another, from the absence of this gas, denominated the Sweet Well. The water of the New Spa contains a larger quantity of sulphuretted hydrogen than any other medicinal spring in England. According to the analysis of Dr. Cossar, quoted by Miss Wilkinson, it amounts to 20.56 cubic inches in the gallon; but Mr. Walker's analysis gives as much as 22.2 cubic inches. This is considerably more than is found in Harrogate water, or in that of Weilbach, the most frequented sulphur spring on the Continent. For the various hepatic, cutaneous, and other disorders for which sulphurous waters are recommended, the Croft Springs are therefore to be preferred.

With regard to the other matters, however, our authorities differ considerably; and we place the analyses of the New Spa, by Dr. Cossar and Mr. Walker, in juxtaposition with the hint that any person interested in the prosperity of Croft had better employ a competent chemist to make an exact determination of the ingredients.

position with the hint that any person interested in the prosperity of Croft had better employ a competent chemist to make an exact determination of the ingredients.

	Cossar.	Walker.
Water of New Spa. In one gallon.	Cubic inches.	Cubic inches.
Sulphuretted hydrogen	20.56	22.2
Carbonic acid	40.216	15.2
Nitrogen and carburetted hydrogen	..	4.8
Gaseous contents	60.776	42.2
	Grains.	Grains.
Carbonate of lime	23.60	64.0
Carbonate of magnesia	4.56	..
Sulphate of lime	141.68	8.0
Sulphate of magnesia	13.72	67.2
Sulphate of soda	18.82	..
Chloride of sodium	5.92	..
Chloride of calcium	..	19.2
Saline matter	208.30	158.4

Miss Wilkinson only gives Dr. Cossar's analysis, but without Mr. Walker's we should have been greatly puzzled by one sentence in her little work. She writes:—"The sulphur water is often drunk warm with a spoonful of common salt in it, which tends to strengthen the stomach, enabling it to bear the otherwise raw potency of the chloride of calcium." None of this latter salt appears in the analyses our authoress quotes, and only a very small amount of chloride of sodium, so one or the other of the chemists we have named must have made some mistake in the estimation of the chlorine. The other differences are remarkable, and, as we have said, it would be well to have them settled.

There is nothing more to notice in the chemical, and the archaeological and topographical information which Miss Wilkinson gives does not concern us here. We may assure our readers, however, that they will find a good account of all that is interesting in and around the neighbourhood. There is a good hotel at Croft, a respectable inn, and plenty of private lodgings, the last having "a character of aristocratic rural comfort rarely found in the more crowded watering-places." Altogether it seems to be a very desirable residence for invalids. We advise all who are recommended to a sulphur spring to consult Miss Wilkinson's little book; and let those who have no wish to mix with the fashion and gaieties of Harrogate, who have no knowledge of French or German, whose appetites fail before foreign cookery, or whose spirits rebel against Continental police regulations, go down to Croft, and there get all the benefit to be derived from drinking or bathing in sulphur-water, with pure air and healthful exercise in quiet scenery; and then let them thank Miss Wilkinson for having brought the knowledge of the place home to them.

CORRESPONDENCE.

White Gunpowder.

To the Editor of the CHEMICAL NEWS.

SIR,—In reply to the letter of "J. M. C." in your Journal of August 31, respecting the grinding of white gunpowder prepared by the moist process, I beg to state that the plan now adopted is that of crushing the dry mass between wooden rollers, and then sieving; by this means it is reduced to the condition of a powder, the fineness of which will depend upon the number of meshes to the square inch of the sieve employed. I do not find that the powder has a tendency to cake, or form hard lumps when exposed to the air. I have now some samples by me, prepared upwards of a month ago, that

have been exposed to air, which have not caked to any appreciable extent.

I send a small sample of white gunpowder prepared in this way; the proportions chlorate of potash 49 parts, yellow prussiate of potash 28 parts, and cane sugar 23 parts, are those given by Dr. Pohl in your Journal. If "J. M. C." will try the experiments of grinding a portion with a tolerable amount of pressure in a stone or Wedgewood mortar, he will have no longer any doubts respecting the possibility or impossibility of its exploding (or igniting) by trituration between stones, or when struck a smart blow with a hammer, upon a stone.

I shall be happy to forward "J. M. C." a sample of white gunpowder prepared by the above-mentioned process if he will send me his address.—I am, &c.

F. HUDSON.

Laboratory, 68, Corporation Street, Manchester.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Specific Gravity of Ammonia.—Jolly has made some experiments (*Annal. der Chem. und Pharm.* Bd. cxvii. s. 181), and obtained numbers which differ slightly from the determinations of others. The specific gravity of pure liquid ammonia at 0° and in relation to water at the same temperature he found to equal

I.	II.	III.
0.6239	0.6261	0.6193.

The specific gravity of gaseous ammonia relatively to that of atmospheric air at 0° and pressure 715 mm., he found to equal

I.	II.	III.
0.558	0.576	0.565

The above numbers differ a little from those given by Faraday, Biot, and Arago.

II. ANALYTICAL CHEMISTRY.

Influence of Free Ammonia and Ammoniacal Salts on the Precipitation of Nickel, Cobalt, Zinc, Manganese, Iron, and Uranium, by Sulphide of Ammonium.—Fresenius publishes (*Journ. für Prakt. Chem.* Bd. lxxxii. s. 257) the result of a long investigation undertaken to test the truth of Terrell's assertion that the presence of ammonia and ammoniacal salts hindered the complete precipitation of manganese, cobalt, nickel, and zinc by sulphide of ammonium. The author gives copious details of his experiments which sometimes confirm, but generally contradict, Terrell. We select and condense some of the results:—

Nickel.—Colourless or yellow sulphide of ammonium precipitates very slowly a salt of nickel oxide, and traces of nickel remain in solution. Sal ammoniac greatly assists the precipitation after some time. Free ammonia is most prejudicial. The precipitation is most complete with sal ammoniac and a slight excess of sulphide of ammonium.

Cobalt.—Very dilute solutions of salts of cobaltous oxide are not precipitated by sulphide of ammonium alone, even on long standing. Sal ammoniac assists the complete precipitation after a time. Ammonia hinders the complete precipitation, but only in a slight degree. Whether colourless or yellow sulphide is used, the results are the same. Under the most favourable conditions the sulphide will precipitate a solution of cobalt containing only $\frac{1}{100000}$ th part.

Zinc.—Colourless sulphide of ammonium will precipitate slowly a pure neutral zinc solution. Sal ammoniac assists the complete precipitation. Free ammonia causes the precipitate to remain suspended a longer time, but beyond this has no injurious influence. With sal ammoniac the yellow sulphide answers as well as the colourless. Zinc may be precipitated from a solution containing only $\frac{1}{100000}$ th part.

Manganese.—Pure sulphide of ammonium will not pre-

cipitate a very dilute manganese solution. Sal ammoniac favours the precipitation after some time. Ammonia in the presence of sal ammoniac does not appear either to assist or hinder the precipitation. Yellow and colourless sulphide appear to act very much alike; but the latter is, perhaps, to be preferred. The reagent is not so sensitive with manganese as with zinc, nickel, and cobalt, but a solution containing from $\frac{1}{100000}$ to $\frac{1}{1000000}$ th part is soon precipitated and in the course of time as little as $\frac{1}{1000000}$ th. When a manganeous oxide solution was precipitated by the sulphide in the presence of sal ammoniac, the author observed that the flesh-coloured precipitate changed after some days to a green, an appearance which had never before been remarked. This change of colour is obviously caused by the passage of the hydrated sulphide of manganese into the anhydrous. The change of colour begins at one spot and gradually spreads over the whole precipitate.

Iron.—Ferrous Oxide.—Pure sulphide of ammonium, colourless or yellow, precipitates a solution of ferrous oxide slowly but completely. Sal ammoniac greatly assists the precipitation after a time. Free ammonia is not appreciably detrimental. Colourless or clear yellow sulphide has the same effect.

Ferric Oxide.—Pure sulphide of ammonium, colourless or yellow, precipitates a dilute solution of ferric oxide slowly and imperfectly, or not at all. Sal ammoniac assists the precipitation. Free ammonia is no disadvantage: a small quantity may be of some advantage in hastening the precipitation. Colourless or yellow sulphide answers equally well.

Uranium.—The precipitation of uranic oxide from very dilute solutions requires a long time. Colourless sulphide appears to act somewhat quicker than yellow. Sal ammoniac promotes the precipitation after some time, and free ammonia may assist. The colour of the precipitates depends on the presence and amount of sal ammoniac, ammonia, and sulphide of ammonium.

III. ORGANIC CHEMISTRY.

The Fixation of Ammonia.—M. Schützenberger (*Bulletin de la Société Chimique de Paris*, i., p. 16) has also made experiments similar to those of M. Thenard, with analogous results. He extended his investigations to the vegetable colouring matters, and found that when these were heated with ammonia in a sealed tube, that they also fixed the gas which was not removed by the strong acids nor disengaged by hydrate of lime.

Luteoline.—The colouring matter of the woad according to Schützenberger and Paraf (*Ibid.* ii., p. 18), is easily obtained pure by extracting in a displacement apparatus with boiling alcohol. The concentrated alcoholic solution is then precipitated by water, the precipitate dried, and the luteoline separated from the resinous matter by sublimation, in which way it is obtained in beautiful golden yellow acicular crystals. The analysis of the crystals led to the formula $C_{24}H_8O_{10}$. Luteoline dissolves when heated in a sealed tube with caustic ammonia, and the solution evaporated to dryness, leaves a residue which evolves ammonia when treated with caustic potash.

MISCELLANEOUS.

THE WATER BAROMETER AT THE CRYSTAL PALACE.

THE scientific visitor to the Crystal Palace will find a new object of interest in the Water Barometer which has recently been erected within the building at Sydenham. The instrument is remarkable not merely by reason of its unique character in point of construction, but bespeaks an additional claim to our notice from the circumstance of its having been originally fitted up, by Professor Daniell, against the staircase wall of the apartments occupied until

lately by the Royal Society at Somerset House, and where for nearly thirty years it has registered in a very conspicuous manner the variations of barometric pressure.

The Council of the Royal Society having placed the instrument at the disposal of Mr. Glaisher, of Greenwich Observatory, that gentleman in turn negotiated its transfer to the Directors of the Crystal Palace, who at once took steps to provide at Sydenham a convenient place for its reception and exhibition, and within the last few days the reconstruction of the instrument has been successfully accomplished by Messrs. Negretti and Zambra, of Hatton Garden. The preliminary operation of removing the barometer from Somerset House was, after much difficulty, effected by them some months since, all arriving in safety at the Palace, with the exception of the breakage of about eighteen inches from the lower end of the barometer tube. Originally the tube was filled by means of a copper steam boiler, set in brickwork over a fire-place immediately under the barometer itself, so that the tube could be lowered into the boiler which has since formed its cistern.

The original plan of erection did not appear to be that most readily available for reconstructing the instrument at the Palace; firstly, because there was doubt as to the boiler being sufficiently strong to bear the necessary pressure, so that the water might be boiled almost throughout the tube and maintained for some time in a state of ebullition, and, secondly, on account of the existing regulations at the Crystal Palace not permitting fire to be lighted within the building. Under these circumstances it was suggested by Mr. Negretti to use an auxiliary boiler outside the building, and his object was first to boil the water in the cistern, allowing the steam to escape by a cock for some time, and then, by closing the cock, to drive steam through the tube until it escaped in a strong jet from its upper extremity, and then to boil the water in the tube till it issued from the top. By this means all the air would be expelled from the tube itself, and when this operation had continued long enough to temporarily seal the top while the water was still flowing out, and ultimately, by means of a judicious use of the blowpipe, to seal hermetically the top of the tube. Mr. Negretti was engaged for three days at the Palace preparing everything, and with great dexterity joined on a new piece of glass to the lower end of the barometer tube which had been broken, and another piece of glass likewise on to the upper extremity terminating in a small aperture.

The operation of filling and sealing the tube is thus described by Mr. Glaisher:—"On Thursday, August 29, everything was ready for the experiment, an auxiliary boiler was planted outside the building for generating steam, a pipe from which led to the boiler under the barometer itself into which distilled water in sufficient quantity had been placed, precautions having been taken to connect the pipe from the outer boiler with that in the inner boiler, with a portion of flexible tubing which would first yield if anything went wrong. After the connection was made, in about half an hour a current of steam issued from the cock. When this had continued for some time, the cock was closed, and the steam ascended into the tube, being at first condensed by the upper part of the tube, and ultimately rushing out at the top with considerable velocity. Water then ascended into the tube; and it was an anxious moment to watch the boiling water reach and pass where the tube had been joined, which, however, had been so admirably performed that all anxiety on this head soon ceased. After a time the upper extremity, while steam was rushing out, was closed, when the water ascended the tube and soon reached a point 32 feet from the surface of water in the boiler below. After this it was driven up, by steam, to very nearly the top of the continuation of the tube, but it did not reach quite up to the sealing point. A consultation now took place; it was almost resolved to let well alone, but it was ultimately determined to go through the same process again, as some

practical points had been suggested which promised to make the experiment yet more successful.

"The top of the tube was therefore opened, the water fell, and the operation repeated. It would be tedious to detail all the steps taken in the reboiling; the operation was eminently successful, and the tube was finally closed by the blow-pipe, and the column of water reached nearly 33 feet, without the slightest speck of air-bubble being perceptible.

"On Friday afternoon Mr. Negretti and I found the column of water 32 feet 9 inches in length, as measured from the surface in the cistern. This is equivalent to a column of 28.84 inches of mercury, considering the specific gravity of mercury as 13.624. The reading of a mercurial barometer at the same time was 29.86 inches; the difference between these two readings is 0.98 inch, and this difference should indicate the elastic force of aqueous vapour at the temperature at the time—viz., 78° if both instruments be correct. The elastic force of aqueous vapour at the temperature of 78° is 0.96 inch, differing by two hundredths of an inch only from that indicated by the water barometer, and completely satisfied me as to the accuracy of the operation, thus proving that the space above the water was free from air.

"I cannot close without expressing my sense of the ready resource of Mr. Negretti in overcoming every difficulty as it arose, and of the facility with which he performed and conducted every process connected with the operation."

The instrument is fixed in an angle of the Tropical Department, and near the great tree. The top of the column of water can be seen from the first gallery, and as that which causes a variation of an inch of mercury will cause a variation of more than a foot in the column of water, so the changes in the latter will be more than twelve times as great as in the former. Many oscillations, therefore, may be seen by the water which cannot be seen by the mercurial barometer, and in gales of wind or heavy storms it will be highly interesting to watch its action.

The principle of construction which renders this form of the barometer somewhat liable to get out of repair will not we trust materially falsify its indications for some time to come. The fact of water being a solvent for the gaseous components of the atmosphere must introduce a slight source of error, which will make itself apparent by a gradual diminution in the height of the water-column. The precautions taken against frost by the selection of the site appear fully sufficient to combat this formidable antagonist of the Water Barometer.

We may remark, in conclusion, that the Crystal Palace, from its offering such rare facilities in regard to height, appears peculiarly adapted for the exhibition of a gigantic Foucault's pendulum, which, like the philosophical instrument just now erected, would probably have an interest for the majority of the numerous visitors.

The City Gas Companies.—A joint circular has been issued by the City Gas Companies, notifying that, in consequence of the recent Act for Regulating the Supply of Gas to the Metropolis requiring, under heavy penalties, a greatly increased standard of illuminating power and purity, which will necessarily involve an increased cost of production, the Companies supplying the City of London will in future charge 4s. 6d. per 1000 ft. for the ordinary coal gas.

Phosphuretted Hydrogen.—Professor Graham has shown that phosphuretted hydrogen, although not spontaneously inflammable, becomes so when brought in contact with a small quantity of nitrous acid. M. Landolt has furnished an elegant mode of showing this fact. Some phosphuretted hydrogen, not spontaneously inflammable, prepared by heating some phosphorus with a concentrated solution of potash with double its volume

of alcohol, is directed towards a layer of nitric acid in a capsule. If the acid be about the density of 1.34, and if it is deprived of its hyponitric acid by boiling, the bubbles of phosphuretted hydrogen will traverse it without ignition. But if only a few drops of the fuming acid be added, each bubble of the gas will immediately inflame. Too much of the fuming acid will suspend the action, by decomposing the gas.

Oil Springs in America.—We have several times recently called attention to the oil springs of the United States and Canada. These springs have already become a new and increasing source of wealth. In Canada there are nearly 100 wells yielding oil. The land which contains the springs is leased in patches of acres or half acres. The lessors generally give 300 dollars for the privilege and one third of the oil drawn from the wells. A correspondent says: "The wells are sunk and cribbed to a depth of from 40 to 60 feet till the rock is reached. In many cases surface oil is found before reaching the rock, but it is of rather inferior quality and doubtful yield. After arriving at the rock, the wells through the earth being from 4 to 7 feet square, they drill to a depth of from 40 to 70 feet, between which distances oil is almost sure to be discovered. Wooden tanks, varying from 500 to 2,000 gallons capacity, are constructed close to each well. The oil is pumped into these, and afterwards drawn off into barrels to be sent to market. The cost of a well is very small. Parties are in the district making contracts for sinking wells. Their charge is, for sinking through the earth 2½ dols. per foot, and for drilling through the rock 2½ dols. per foot. A well after being commenced usually yields oil in less than 30 days. At these prices a well can be sunk, a tank for 100 gallons made, and a pump and all necessary appliances provided for a sum equal to about 100l. sterling. When oil is found, one man at 4s. a day can readily pump 100 barrels, or 4,000 gallons a day. The cost of the oil, including every expense, and calculating the reimbursement of every outlay incurred in one year, and also calculating a yield of 15 barrels, or 600 gallons, a day, is, it is alleged, absolutely less than a half-penny sterling a gallon delivered into the tank alongside the well. Very few attempts have yet been made to pump by steam at the place where the oil is most extensively found, nearly all the labour being by hand. This is partly to be attributed to the badness of the roads, preventing the possibility of getting heavy loads into the district, and partly to the yield of the wells being on an average about equal to the quantity a man can easily pump in a day. A moderate average yield for all the wells now in operation (100) is 15 barrels, or 600 gallons, a day. At the rate at which privileges are being disposed of and parties are preparing to sink wells, there will be 500 in operation before Christmas. Taking the present 100 wells only, however, the result is extraordinary. Thus—100 wells yielding 600 gallons give 60,000 gallons per day, 360,000 per week, or 18,720,000 per annum. A trustworthy observer remarks:—"Such a yield seems almost fabulous, and yet I believe, from personal observation and enquiry on the spot, that it is within the truth."

Diamonds.—In the official statistical report of the value of produce exported from Bahia, Brazil, an amount is included not exceeding 150,000l., whereas an amount superior to 500,000l., worth of this precious stone is clandestinely sent out of the country yearly. The discovery of the diamond mines in the district of Paraguassú, in the year 1845, has effected great and beneficial results to the population and commerce. What was in that year but a desert, is now one of the most flourishing districts, inhabited by a laborious and daily-increasing population. The town of Isabel de Paraguassú and neighbourhood contains 60,000 souls; that of Lencóas, 30,000; and Andarahy, 20,000—all the result of this discovery; and it may truly be said that those mines have realised for the poorer population of the interior, the same good we have

observed in California and Australia. It has opened a large field to all without dependence on professional skill or capital, of which the majority were not possessed; and in a moral sense has operated wonders in reclaiming the vicious from habits of indolence and crime. These men, finding in the extraction of diamonds an advantageous profit, naturally became large consumers of the produce of the country, which, for the want of market, was not cultivated; and altering their habits of life, increased their well-being, and eventually made them consumers of the produce of foreign countries. The town of St. Isabel de Paraguassú is now the chief depôt of the trade of the north-west of this province, and supplies all the interior populations with the comforts of European civilisation, a population calculated at no less than 700,000 souls. In the neighbourhood of St. Isabel the cotton plant flourishes, and nitrate of soda is found—but the great difficulty to the exportation of these valuable products is the want of a good road to this port. Last year I used all my influence and endeavours with the late President to promote the passing of a law in the Assembly, guaranteeing five per cent. on the capital expended in the construction of a good carriage road. The law was passed and duly sanctioned by the late President, but, unfortunately, that law has been revoked. To give an idea of the importance of the road in question, it is enough to state that goods (foreign) to the value of 600,000l. to 1,000,000l. sterling are annually sent to the diamond district from this city, and that its exports in diamonds average between 600,000l. to 800,000l. per annum; no calculation being possible of the value of cotton, nitrate of soda, hides, skins, cattle, iron, &c., &c., that would be sent down, had the road I had so strongly recommended been carried into execution.—*Extract from the Report of H. B. M. Consul.*

ANSWERS TO CORRESPONDENTS.

Kafes and Adolphe.—Received.

J. S. Blockey.—Your suggestion is received with thanks, and will be attended to if practicable.

Applied Chemistry, by M. Barrowwill.—Will the correspondent who last week left at our office a short paper with the above heading, favour us with the second part?

Tri-ethyl phosphine.—A correspondent wishes to be informed of the best method of preparing this new reagent for bisulphide of carbon in coal gas.

T. King.—1. Professor Johnstone has written an excellent little work on "The Analysis of Soils and Manures," and we believe Mr. Nesbit, of Kennington, has also published a similar volume. 2. At the Government School of Mines, Jermyn Street, or at Chapman and Hall's.

Brvata.—In the article "Researches on the Assay of Silver," in our last No., p. 125, fourth line, for "double salt KO +," read "SO₄KO +." The last three lines (in p. 127) should read thus:—"A little chlorine soon prohibits its further formation. This, also, is an interesting instance of the influence of quantity on chemical affinity."

Ventilation.—A correspondent has drawn our attention to the subjoined extract from the *Scientific American* of the 21st ult.:—"An English journal says that 'the new system of ventilation, patented by the National Ventilation Company, consists in a nice adaptation of wire-gauze to open windows, so that the air is allowed to steal gently on and out, while violent draughts, and those enemies to ventilation, the blacks, are excluded.' We should like to know what on earth is new about this mode of ventilation that it should attract the attention of the National Ventilation Company. The same thing is in extensive use in this country, and has been for years—used for the double purpose of ventilation and to keep out noxious insects."

Nitro-Prusside of Sodium.—*Theophilus.*—This reagent is prepared by treating 2 parts of powdered ferrocyanide of potassium with 5 parts of common nitric acid previously diluted with its own volume of water. The solution, after the evolution of gas has ceased, is digested on the water-bath until salts of protoxide of iron no longer yield a blue but a slate-coloured precipitate. The liquid is now allowed to cool, when much nitrate of potash and occasionally oxamide is deposited; it is filtered and neutralised with carbonate of soda which yields a green or brown precipitate and furnishes a ruby-coloured filtrate. This, on evaporation, gives a crystallization of nitrate of potash and soda together with the new salt. The crystals of the latter are selected and purified by crystallization; they are rhombic, and of a splendid ruby colour.

THE CHEMICAL NEWS.

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SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Transformation of Monobromated Propylene into a New Hydrocarbide of the Composition, C_3H_4 , by M. SAWITSCH.

In a paper on the preparation of acetylene by bromated ethylene, I ventured the opinion that by applying the same method to the superior homologues of this body, the series of hydrocarbides C_nH_{2n-2} would be obtained. Experience has fully confirmed my opinions, at least in that which concerns the second part of the series. In fact, by submitting monobromated propylene to the action of soda alcohols, one equivalent of hydrobromic acid may be taken away, transforming it into a carbide, C_3H_4 , to which I have given the name of *allylene*.¹

The details of the operation are identical with those I have already pointed out with respect to acetylene, with this difference, that ethylate was substituted for acetylate of soda. When the balloon, previously cooled, in which the reaction took place was opened, a considerable quantity of a gaseous mixture was disengaged,² which was several times passed through a solution of ammoniacal oxide of copper, in which it deposited a large, flaky, canary-yellow precipitate, possessing the following properties:—Heated, it decomposes with deflagration and the production of a reddish flame; thrown into bromine, it is destroyed, occasioning a rustling sound, accompanied by a red flame; finally, cold, concentrated acids immediately disengage gas from it, which is set free under the influence of diluted acid, particularly of hydrochloric acid, but only by the application of heat.

This last reaction is utilised in the preparation of pure allylene. This gas is colourless, with a strong and disagreeable odour, though less so than that of acetylene. It burns with a brilliant, fuliginous flame. It precipitates mercurous and argentine solutions,—the former dark gray, the latter white. These two combinations having a composition probably analogous to cuprous compounds, are, like them, unstable; they decompose by heat, the former without detonation, the latter with an explosion and a reddish flame.

The endiometric analysis of the gas gives the following results:—

Temperature . . .	13°
Barometer . . .	0.763m'
Volume of gas . . .	2.69 c. c.
Volume of oxygen added .	21.92 "
Total volume . . .	24.61 "
After . . .	19.10 "
After potassa . . .	11.02 "

¹ This name was previously proposed by M. Berthelot in his work on Organic Chemistry, vol. II., p. 161.

² Twenty-five grammes of monobromated propylene have yielded about three litres of gas. To extract the whole, the balloon must be gradually heated to the boiling point of alcohol, in which allylene is soluble.

	Experiment.	Theory.
	c. c.	c. c.
Disappeared . . .	5.51	5.38
Carbonic acid formed .	8.08	8.07
Oxygen consumed . .	10.90	10.76

or, by comparing the results of experiment with one volume of gas:—

Contraction . . .	2.04	2.00
Carbonic acid . . .	3.00	3.00
Oxygen consumed . .	4.05	4.00

I had not sufficient gas at my disposal to enable me to determine its density, and thus to corroborate the formula, $C_3H_4 = 2$ vols., which, nevertheless, seems to me incontestable, looking to the mode of formation of this body, to its composition, and to the similarity of its properties with those of acetylene, respecting whose formula no doubt can be entertained. The curious property of precipitating ammoniacal solutions of copper, which has enabled me to isolate allylene, seems to be characteristic of the hydrocarbides of this series, at least, judging from the two first terms. There is good reason to believe that it will lead to the discovery of higher terms.

I propose soon to return to the study of the physical and chemical properties of allylene and its derivatives. At present I confine myself to the mention of bromide of allylene, a limpid and colourless liquid, smelling like tribromide of allyle, and which I obtained by dropping allylene into bromine.

It may not be needless to remark, that, in order to obtain every desirable guarantee of the purity of the gas, I employed monobromated propylene, prepared by means of propylene gas, proceeding from the reduction of iodide of allyle by mercury and hydrochloric acid. In my opinion, this is the only way to obtain pure bromated propylene.

These researches were made in M. Wurtz's laboratory. — *Comptes-Rendus*.

On the Presence of Nitrates in Mineral Waters, and on the Production of Saltpetre, by R. HAINES, M.B., Professor of Materia Medica, Grant Medical College, Bombay.

In September, 1851, the Medical Board sent me for analysis five bottles of well-water from Aden, each from a different well. These waters were all clear and colourless, neutral to test-paper, and of a more or less bitter, nauseous, saline taste, like a dilute solution of Epsom salts.

The analysis was conducted in the usual way; but from the small quantity of each water available, it was necessary to economise the supply as much as possible, and any attempt to estimate the less abundant constituents, as bromine, iodine, fluorine, &c., was out of the question.

The first process in these analyses is usually to evaporate

a portion of the water to dryness, with the view of ascertaining the total amount of solid matter.

The object of this is merely to give a check upon the total of separate constituents. Afterwards each constituent is estimated by itself, a fresh portion of water being used, if possible, for each. In the present case, as soon as the evaporation had proceeded so far as to cause the commencement of crystallisation, pungent fumes of hydrochloric acid began to be given off in abundance. This was evidently owing to the presence of chloride of magnesium, which, like chloride of aluminium, is decomposed on evaporating its solution into the base and hydrochloric acid. As, from the small quantity of water sent me, I was obliged to employ the dry saline mass for the purpose of estimating some of the constituents, I did not dry the mass completely enough (that is, at a high temperature) to make its weight an efficient control on the subsequent analysis, which, if I could have foreseen what afterwards occurred, I should certainly have done.

On summing up the amounts of acids and bases obtained,—hydrochloric and sulphuric acids, soda, lime, and magnesia,—I was surprised to find that in four out of the five cases the acids were quite insufficient to neutralise the bases; in the fifth case the difference was unimportant. I naturally suspected some error in my analysis, and repeated a few of the processes with such small quantities of water as I could spare, without finding any errors of importance. I was driven, therefore, to seek for some other acid which must have escaped detection. Phosphoric acid was sought for in vain; but on applying the tests for nitric acid, it at once appeared that, in the very four cases mentioned, it was present in abundance, while in the fifth mere traces of it were found.

There are some substances which, from peculiar circumstances, it is difficult to estimate quantitatively in a direct manner, and it has, therefore, been usual in compounds containing them to weigh all the other ingredients, and then set down the difference which remains unaccounted for as their weight. Boracic and nitric acids are of this class. The only method recommended for directly estimating the nitric acid of mineral waters was to precipitate the chlorine, iodine, and bromine by sulphate of silver, and then to distil the concentrated liquid with an excess of sulphuric acid. Nitric acid with a little sulphuric acid comes over into the receiver in which a solution of baryta is placed, with which the acids combine. By then slowly evaporating the solution to dryness in the air, the free baryta is converted into carbonate; water leaves the carbonate, and sulphuric acid dissolves the nitrate, in which the baryta being estimated by sulphuric acid in the usual way, the quantity of nitric acid equivalent to it is found. This is a very round-about process, and the distillation especially is most objectionable. The retort is pretty sure to break, unless a sand-bath is used, which retards the operation, and it is quite impossible to bring over the whole of the acid, which remains hanging about the body and neck of the retort. However, with the very small quantity of water that remained, I tried the process, and in one case succeeded in obtaining five-eighths of the whole quantity of nitric acid to be accounted for; in the other cases the loss was much greater. I was obliged, therefore, to rest contented with estimating the nitric acid by difference.¹

¹ Far superior modes of estimating nitric acid have been introduced within the last ten or twelve years, but I was not then acquainted with them.

The waters from Aden were named as follows:—1, Bantan well; 2, Parsee well; 3, Turkish well; 4, Jaffron well; and 5, Lower Tawella. Their composition is given in the accompanying tables; the specific gravities were taken at the temperature of the atmosphere, and reduced by calculation to 60°, the expansion being assumed to be the same as that of pure water. The quantity to which the contents are referred is the imperial pint of 8750 grain measures, or one-eighth of an imperial gallon.

Names of Wells.	Banian.	Parsee.	Turkish Well.	Jaffron.	Lower Tawella.
Specific gravity at 60°.	1007.2	1005.2	1005.1	1007.9	1004.4
Constituents found:—	Grains.	Grains.	Grains.	Grains.	Grains.
Chlorine	32.078	30.507	29.459	41.170	23.247
Sulphuric acid	6.771	3.746	4.130	7.466	2.765
Nitric acid	8.081	3.010	2.577	5.766	..
Magnesia	3.802	4.326	4.159	5.427	2.389
Lime	10.833	7.381	6.315	7.645	7.110
Soda	20.503	16.816	17.755	28.773	11.313
Carbonates of lime and magnesia	0.303	0.275	0.349	0.397	0.443
Silica	0.466	0.271	0.393	0.333	0.585
Arranged as:—					
Chloride of sodium ..	28.815	26.071	27.019	41.033	17.178
Sulphate of soda ..	11.980	6.670	7.353	13.345	4.923
Chloride of magnesium ..	8.442	10.060	9.595	12.622	5.558
Chloride of calcium ..	12.909	11.627	9.671	8.324	13.567
Nitrate of lime ..	12.332	4.576	3.935	8.657	..
Carbonates of lime and magnesia	0.303	0.275	0.349	0.397	0.443
Silica	0.466	0.271	0.393	0.333	0.585
Total solids	75.647	59.550	58.315	86.710	42.594
Carbonic acid, cu. in. ..	0.481	1.190	0.961	0.481	0.744

I was until lately under the impression that this was the first time the water of the Aden wells had been analysed. I find, however, that some years previously Dr. Giraud had cursorily examined seven specimens of such waters, at the instance of the Medical Board, with the following result, which he has kindly allowed me to communicate. I have added the third and fifth columns for the sake of comparison with my analyses:—

Name.	Specific gravity of distilled water = 1000.		Saline contents.	
	At 80° F.	At 60° F.	In 1000 grains.	In an Imperial pint.
1. Bantan Well ..	999	1002	1.16	5.08
2. Bir ud Dowla ..	999	1002	1.50	6.08
3. Bir Zawool ..	1000	1003	2.75	12.07
4. Gaol Well ..	1001	1004	5.58	24.50
5. Bir Senar ..	1001	1004	8.41	36.94
6. Bir Zaffran ..	1002	1005	10.04	44.15
7. Un-named ..	1000	1003	8.70	38.18
8. Ras Ma' (contained bromine) ..	..	..	59.00	258.12

No quantitative analysis was then made, but Dr. Giraud remarks that No. 7 contained a large quantity of salts of lime.

Amongst these waters, there are only two which bear the same name as those in my list, the Banian well and the Zaffran well. Whether the Zawool is the same as

² From a well experimentally sunk on the Isthmus, very near the sea, at the south-eastern extremity of the inner line of fortification, at the foot of the rocks, the water was at first sweet, but after a time the sea appeared to penetrate into it, and it became useless. (See Lieut. Wilkins' Report.)

the Lower Tawella of my list is doubtful. But the difference in character of those two, as they appear in the two lists, is so great as not to be easily reconciled; either there must be some confusion of names, or the water of the wells is liable to very great variation in quality. Perhaps some of the officers at Aden may be able to clear up this point. A list of the various wells in Aden, with an exact description of their localities, and other particulars, is much wanted.

The discovery of so large a quantity of nitric acid in well water was at the time, I believe, unparalleled; but a few years after other cases of a similar nature were found to occur, and on this side of India. In 1854 some water was sent for analysis from Nassick, drawn from a certain well in the gaol, which water was so impregnated with salts as to be unfit for any purpose but that of washing. Dr. Giraud found it to contain so much nitric acid that "on evaporating the water to dryness, and igniting the residue, copious red fumes of nitrous and hypo-nitrous acids were disengaged from the decomposition of the nitric acid of the nitrates, by the organic matter previously held in solution in the water." Again, in the following year, four samples of water, from different wells in the town of Nassick, being sent for analysis, three of them proved to be ordinary spring water, of tolerable purity; but the fourth, from the "Bhunder Kallee" well, was found to be even more strongly impregnated with nitrates than that from the gaol. In this case, also, copious red fumes were given off in heating the saline residue. The proportion of silica, too, appears most unusually large; perhaps the sulphate of lime was not thoroughly extracted from it. The composition of these waters is given below. I have added a column with a view to facilitate comparison with my analysis of the waters from Aden. The nitric acid is estimated by difference. The water from the Nassick wells is said to vary very much according to the season of the year. These waters were drawn in October and November.

Specific gravity .. {	Gaol Well.		Bhunder Kallee Well.	
	At 80° At 60°	1002° 1004°5	At 80° At 60°	1004° 1006°5
	In 10,000 grs.	In Impl. pint.	In 10,000 grs.	In Impl. pint.
Constituents found:—	Grains.	Grains.	Grains.	Grains.
Chlorine (total) ..	5.6000	4.922	12.054	10.14
Sulphuric acid ..	0.9974	0.877	4.021	3.541
Nitric acid ..	..	(13.190)	22.655	19.951
Chloride sodium ..	8.4150	7.395	..	..
Soda ..	..	..	10.618	9.367
Lime ..	4.0215	3.518	7.305	6.432
Magnesia ..	3.7000	3.262	5.429	4.7.0
Silica ..	..	..	3.260	2.871
Arranged as:—	8.4150	7.395	19.972	17.590
Chloride sodium ..	1.7064	1.499	6.879	6.058
Sulphate lime ..	9.5992	8.430	12.896	11.356
Nitrate lime ..	12.2810	10.802	19.635	17.291
Nitrate magnesia ..	0.6940	0.609	..	..
Chloride magnesium ..	..	..	3.260	2.871
Silica ..	..	..	..	..
Total solids ..	32.6936	28.741	62.643	55.166

(To be continued.)

On Perchloric Acid and its Hydrates,¹
by Professor Roscoe.

ALL the knowledge we possess of the quantitative relations of perchloric acid is the determination of the composition of the potassium salt, first analysed by Stadion, in 1816, and afterwards by many other chemists. The perchloric anhydride has not been isolated, and no analysis of the aqueous acid has ever been made. We can only account for the neglect with which chemists have treated the highest and yet the most stable of the oxides of chlorine, by the fact that the preparation of the acid in large quantities has been attended with great difficulties. The best method for preparing aqueous perchloric acid is to decompose chlorate of potassium with hydrofluosilicic acid, to boil the chloric acid thus obtained down, which splits up into lower oxides of chlorine, which escape in the gaseous state, impure perchloric acid being left behind, which is purified by distillation. The acid thus obtained is in appearance not to be distinguished from oil of vitriol, being a colourless, heavy, thick, oily, corrosive liquid, giving off by heating dense white fumes. By heating the aqueous perchloric acid with four times its volume of concentrated sulphuric acid, the latter takes water from the first, dense white fumes are evolved, a yellow mobile liquid distils over, afterwards thick oily drops appear, which, when coming in contact with the yellow liquid, form white crystals, previously obtained by Serullas, but in such small quantities that he was unable to analyse that substance, which, prepared in this way, contains always sulphuric acid, and is therefore not fit for analysis, and requires redistillation. Heated, however, to 110° C., the crystals decompose, and split up again in the yellow liquid, which distils over at a low temperature, and the thick oily liquid remains in the retort. The yellow liquid thus obtained is pure perchloric acid, a body not known before, and can be obtained also by distilling one atom of perchlorate of potassium with four atoms of sulphuric acid. In the pure state it is perfectly colourless, but commonly slightly yellow, owing to the presence of lower oxides of chlorine. Perchloric acid is one of the most powerful oxidising agents known; a single drop brought into contact with charcoal, paper, wood, alcohol, &c., immediately causes explosive combustion, falling not short in violence of that of chloride of nitrogen, and brought on the skin fearful wounds are produced, which do not heal for months. Like nitric acid, it cannot be distilled without decomposition, but it darkens, and heated stronger, decomposes with explosion. It cannot be kept for any length of time; even sealed up in glass bulbs, which are placed in the dark, it decomposes suddenly after some time, smashing the vessel containing it. It mixes with water with a hissing noise and evolution of heat, uniting and forming the same crystals which were mentioned before, and were used for preparing the pure acid. These crystals are the monohydrated perchloric acid. They melt at 50° C., and heated to 110° C., split up in pure perchloric acid, which distils over, and an oily liquid boiling at 200°, which is also obtained by boiling aqueous perchloric acid till dense white fumes are given off. That oily acid has a constant composition containing 72.3 per cent. pure perchloric acid and 27.7 per cent. of water. This per-centage corresponds, however, to no definite hydrate of simple atomic composition, but follows the same general relations respecting composition and boiling point, which,

¹ Might not the red fumes rather be partly owing to the reaction of the hydrochloric acid disengaged from the hydrated chloride of magnesium, on the nitrates producing chlorine and chloro-nitric gases?

¹ Read before the British Association.

as I have shown previously, hold good for so many aqueous acids, namely, that the phenomena of constant boiling point and constant composition depend chiefly upon physical, and not on chemical attractions.

TECHNICAL CHEMISTRY.

On the Chemical Manufactures of South Lancashire, by Drs. SCHUNCK, ROSCOE, and SMITH.

Sulphuric Acid.—This is an important manufacture, and quite indispensable in the production of many other articles, as well as in the manufacturing processes. To produce it economically on a large scale was of considerable importance, and a great degree of economy was practised. Few manufacturers employed sulphur, most of them using pyrites. In burning the pyrites from eight to ten of sulphur remained behind and was lost. Mr. Spence, of Manchester, had, however, reduced this loss to two. The introduction of Spanish and Portuguese pyrites had caused the rise of a new branch of industry in the extraction of a small quantity of copper which these ores contain. In Hill's process of purifying gas, hydrated peroxide of iron was used instead of lime. When the material could be no longer used for purification, it was found to contain sulphur, and Mr. Spence used this sulphur for the production of sulphuric acid. From one ton of the material he obtained about one and a-quarter ton of hydrated sulphuric acid. Mr. Harrison Blair's improved sulphur-burner was especially valuable, as economising space in the chambers, by preventing the sulphurous acid from being diluted with too large an excess of air, as is the case in the ordinary burners. The tendency in this district has been to increase the size of the sulphuric acid chambers, the largest being 112,000 cubic feet. Many manufacturers employ Gay-Lussac's method, invented sixteen or seventeen years ago, for economising nitric acid. By this means a saving of more than 50 per cent. of nitrate of soda is effected; but others do not employ this method, as with the low price of nitrate of soda, at 12l. per ton, it does not pay to collect and absorb the waste oxides of nitrogen. The use of platina stills for the rectification of sulphuric acid has now been almost entirely abandoned, and their place supplied by glass retorts, which are made larger and of better quality than formerly. The weekly production of sulphuric acid of specific gravity 1.85, exclusive of what is used in the manufacture of soda ash, is 700 tons.

Soda.—This is a most important manufacture. Few changes in the principle of manufacture have taken place during the last ten years, the essential points of the original method of Leblanc (1798) being still adhered to. The extent of the manufacture has largely increased since the year 1851. The value of alkali made annually in England is 2,000,000l.; of this half is made in South Lancashire and half in the Newcastle district. The following statistics apply to the South Lancashire alkali trade, per week, in 1861:—Common salt decomposed per week, 2600 tons; sulphuric acid used, 3110 tons; hydrochloric acid produced, 3400 tons; soda ash sold per week, 1800 tons; salt cake sold per week, 180 tons; soda crystals ditto, 170 tons; bicarbonate soda ditto, 225 tons; and caustic soda ditto, 90 tons. Since 1852 the alkali trade of South Lancashire had more than tripled. The large quantity now produced was manufactured in about twenty-five works, chiefly situated at St. Helen's, Runcorn Gap, Widnes Dock, near Warrington, the

neighbourhood of Bolton, and Newton Heath. None of the patents for improving the manufacture of alkali had changed the process materially. The improvements of detail since 1851 have been the following:—1. Greater attention to economical working in all the branches. 2. The process of lixiviation of black ash is more completely accomplished than formerly by the arrangement of Mr. Shanks. 3. In some works the black ash is now made by machinery. 4. The soda is now in many alkali works packed in casks by machinery. Since 1851, a new branch of the manufacture has been introduced by the preparation of solid caustic soda, which is largely exported to America and other places. The proposition recently made by Kuhlmann for the employment of the alkali waste as a cement is a very old idea, Mr. Deacon, of Widnes, having used it for making floors for twelve years. In conclusion, the report recognised the important additions to our knowledge of the theory of soda manufacture made by Mr. Gossage.

Bleaching Powder.—155 tons of this powder is made in this district. The only remarkable points in the manufacture is an ingenious process of preparing chlorine without the use of biniodide of manganese, patented in 1858 by Mr. Shanks, of St. Helen's, and the regeneration of peroxide of manganese from the waste liquors containing chloride of manganese.

Chlorate of Potash.—From four to five tons of this salt are made weekly in this district, and it is used for match making.

Hypo-sulphite of Soda.—Three tons weekly are made, and are used by paper makers, photographers, and by bleachers as an antichlor.

Silicate of Soda.—This compound is used as a substitute for cow-dung in calico printing, and by soap manufacturers in place of the resins formerly in use. About ten tons weekly are produced in this neighbourhood.

Arseniate of Soda.—This salt is used as a substitute for cow-dung in calico printing, for which purpose it is better adapted than the silicate of soda, as it does not so much attach the alumina mordants. Ten or twelve tons are produced weekly in this neighbourhood.

Bichromate of Potash.—There is nothing new in the manufacture of this salt. About fourteen tons are produced weekly in the district.

Prussiate of Potash.—From four to five tons of yellow prussiate of potash and one ton of red prussiate are produced in this district weekly.

Superphosphate of Lime.—The weekly production in this district is 5600 tons.

Alum.—Before 1845 the alum manufactured in this district was confined to a small quantity from pipeclay, our chief supplies being derived from Whitby. By the old process 60 tons of the oolitic shale of Yorkshire was required to produce one ton of potash alum and one ton of Epsom salts. By Mr. Spence's process 50 tons of shale yield 65 tons of ammonia alum. Mr. Spence employs the shale found underlying the seams of coal in this district. In 1850-51, Mr. Spence made about 20 tons per week. The quantity now made by him amounts to 110 tons. Fully half of the total quantity manufactured in England (300 tons per week) is made by this process.

Protosulphate of Iron.—This salt is manufactured in large quantities in this district, principally for the use of dyers, the amount being about eighty tons per week.

Pyroligneous Acid.—The only improvement introduced into the manufacture of this acid during the last

few years consists in the use of sawdust instead of wood in the process of destructive distillation. The quantity of acid manufactured weekly in Manchester is 12,000 gallons, containing about 4 per cent. of glacial acid. The value of the acid is 3*l.* per ton, whilst that of the tar is 4*l.* to 4*l.* 10*s.*

Compounds of Tin—Chlorides of Tin.—The quantity of these compounds (estimated as crystallised protochloride of tin) manufactured in this district amounts to about sixteen and a-half tons per week.

Stannate of Soda.—This compound has for some time been extensively used for the purpose of preparing calicoes which are intended to be printed with so-called steam colours.

Sulphate of Baryta.—Of this salt, which is usually sold under the name of *Blanc fixe*, about two tons are made in this district by precipitation.

Nitric Acid.—About forty-eight tons of nitrate of soda per week are used in this district for making nitric acid. The salt yields its own weight of acid. Nitric acid is used here for making the nitrates of copper, lead, alumina, and iron, for oxidising tin, for etching, and, latterly, for making aniline.

Oxalic Acid.—One of the most important and interesting of the new manufacturing processes is one for the preparation of oxalic acid, invented and patented by Messrs. Roberts, Dale, and Co., gentlemen to whom we owe a number of highly ingenious and useful practical processes. The quantity of acid manufactured by Messrs. Roberts and Dale amounts to nine tons per week, with capabilities of extension to upwards of fifteen tons. To give an idea of the effect which the introduction of this process has had on the market, it may be mentioned that the selling price at this time is 8*d.* to 9*d.* per lb., whereas in 1851 it was 15*d.* to 16*d.* per lb. Oxalic acid is used extensively in calico printing, woollen dyeing, woollen printing, silk dyeing with wood colours, in straw bleaching, and for making binoxalate of potash, so-called "salt of lemons."

Starch and Artificial Gums.—About twenty tons of starch and thirty-four tons of gum substitutes, made by roasting farina and other kinds of starch, are produced in this district per week.

Purification of Resin.—Several very interesting and successful processes have lately been patented by Messrs. Hunt and Poehin, of Salford, for the purification of resin. The aim of these gentlemen, who have devoted a large amount of time and attention to this subject, is to produce a bright, nearly colourless, solid, and brittle resin, from the common, dark, and impure commercial article. About sixty tons per week of this clarified resin is now manufactured in this district under this patent.

Organic Colouring Matters.—There are few substances of more importance to the manufacturers of this district than those which are employed in imparting colours to the various fabrics, especially those of cotton, produced here. Of these substances, the majority are derived from the animal or vegetable kingdom. Indeed, with the exception of oxide of iron and chromate of lead, very few mineral substances are at the present time made use of alone by the dyer or printer. The great intensity, beauty, and variety of the dyes which are wholly or in part composed of organic matter, causes them to be preferred, and the increase of skill and knowledge of scientific principles on the part of dyers and printers have also led to their more exclusive employment, when it is stated that the quantity of dye-

woods (logwood, peachwood, saffronwood, barwood, fustic, quercitron bark) consumed weekly by the dyers of this district amounts to 300 or 400 tons; that the weekly consumption of the same by printers is about 60 tons; that from 150 to 200 tons are in the same time converted into extracts; that 150 tons per week of madder are used up, exclusive of what is used for garancine, &c., some idea of the magnitude of the interests depending on the employment of the materials may be formed. With the exception of a new method of reducing indigo by means of finely-divided metals, patented by Leonard, we do not suppose that any important improvement has been introduced in connection with this dye-stuff. Of less importance in the art of dyeing is madder, the material with which the most permanent reds, purples, and blacks are produced. The methods which have been proposed for more effectually utilising this important dye-stuff are very numerous indeed, though exceedingly few of them have been found to be of practical value. They may be divided into two classes,—viz., those having for their object to render available the greatest amount of colouring matter, and those which tend to produce more permanent or more beautiful colours. The first object seems to be perfectly attained by converting the madder by the action of acid into garancine. This preparation is becoming more and more extensively used.

Aniline Colours.—The artificial colouring matters from aniline and other bases have of late attracted much attention, and various plans have been devised for producing them. The usual method of obtaining aniline purple, the so-called "mauve," consists in submitting salts of aniline in watery solution to the action of oxidising agents, such as chromates, permanganates, or the peroxides of manganese and lead.

Disinfectants.—The manufacture of disinfectants has become a regular and constant one, and since the inquiries instituted on the subject by Mr. M'Dougall, of this city, the use of those made in this district has been enormously increased. Mr. M'Dougall manufactures, near Oldham, a disinfecting powder, in which the properties of carbolic and sulphuric acid are taken advantage of. This is used to prevent decomposition in stables, cow-houses, and among accumulations of putrescible matter, and generally for the prevention of decomposition in manures. A liquid is also prepared with carbolic acid and lime-water, which is applied for the purpose of preventing decomposition in sewers, according to the idea entertained by one of us, of purifying whole cities by preventing the generation of gases in sewer water or among accumulations of animal refuse. The liquid is also used to prevent the decomposition of animal matter when it cannot at once be made use of, especially in the case of meat brought to market, or animals that have died in the field. The solution of the powder has also been used, to some extent, in dissecting-rooms, where it immediately destroys any noxious smell, and at once liberates the fingers of the operator from the peculiarly nauseous odour which so often attaches to them. It has also been found useful in the treatment of sores, as well as of dysentery. Mr. M'Dougall has also applied carbolic acid to the destruction of parasitic insects on sheep, and has in many districts entirely driven out the arsenical preparations by the use of this acid united with fatty substances. Sheep dipped in it are not liable to be attacked by tick, even when left for some months among other sheep infected with it. Foot-rot, and other diseases of sheep, are also said to be prevented and cured by its use.

PHARMACY, TOXICOLOGY, &c.

On Phosphate of Sesquioxide of Iron,
by EMILE HEYDENREICH.

FOR several years various compounds of phosphorus, oxygen and iron, either by themselves dissolved in acids, or combined with some alkaline salts have been used to a considerable extent. Among these, the compound syrup of the phosphates of iron, soda and lime, (the chemical food,) the so-called syrup of superphosphate of iron, and the glycerol and syrup of hypophosphites of iron have been most generally used. The pyrophosphate of the sesquioxide of iron has also been brought forward by some French chemists, but did not come into use till it was offered by the late M. E. Robiquet, of Paris, in combination with the citrate of ammonia, with which it forms a soluble salt. The extended use which this latter salt has obtained, and the little notice which had been taken of the other compound of phosphoric acid and sesquioxide of iron, led me to make some experiments in this direction. The varied basic character of the phosphate of soda, from which corresponding metallic salts may be usually obtained, would naturally lead one to suppose that similar compounds of the sesquioxide of iron might be obtained by adding to a solution of them solutions of a sesquisalt of iron. This has been verified in the case of the pyrophosphate of iron $2\text{Fe}_2\text{O}_3 \cdot 3\text{PO}_5$: the reaction being $3(2\text{NaO} \cdot \text{PO}_5) + 2(\text{Fe}_2\text{O}_3 \cdot 3\text{SO}_3) = 2(\text{Fe}_2\text{O}_3) \cdot 3\text{PO}_5 + 6(\text{NaOSO}_3)$. According to this, by using the tribasic phosphate of soda, instead of the pyrophosphate, we might expect the reaction to be $3(2\text{NaO} \cdot \text{HO} \cdot \text{PO}_5) + 2(\text{Fe}_2\text{O}_3 \cdot 3\text{SO}_3) = (2\text{Fe}_2\text{O}_3 \cdot 3\text{HO} \cdot 3\text{PO}_5) + 6(\text{NaO} \cdot \text{SO}_3)$.

On carefully adding to a solution of tribasic phosphate of soda a solution of tersulphate of sesquioxide of iron to saturation, a bulky white precipitate was obtained; the supernatant liquid was found to be decidedly acid to litmus paper. The precipitate was washed by decantation several times, thrown on a filter, allowed to drain, and finally pressed between bibulous paper. It was afterwards dried on a sand bath till anhydrous, when it was analysed to ascertain its composition; during the washing and drying, the precipitate assumed gradually a yellow tint.

One hundred grains were fused with some carbonate of soda, the mass left after ignition was removed from the crucible, introduced into a dish with some water, and every portion of oxide of iron carefully washed from the crucible; this mixture was allowed to boil, and sufficient acetic acid added to neutralise the excess of carbonate of soda. By this means, all the sesquioxide of iron was precipitated, while the clear liquid contained the phosphate of soda with some acetate of soda. The sesquioxide of iron was collected on a filter, washed and ignited; to the solution of phosphate and acetate of soda a solution of chloride of ammonium, of sulphate of magnesia and of ammonia was added; by this means the double phosphate of magnesia and ammonia was precipitated, which by ignition furnished the pyrophosphate of magnesia, from which the phosphoric acid was calculated. The result of two analyses carefully performed was:—

First Experiment.	Second Experiment.
Fe_2O_3 52.50	Fe_2O_3 52.00
$2\text{MgO} \cdot \text{PO}_5$ 73.00	$2\text{MgO} \cdot \text{PO}_5$ 74.00

112 parts $2\text{MgO} \cdot \text{PO}_5$ being equal to 72 parts of PO_5 , the following is the result:—

First Experiment.	Second Experiment.
Fe_2O_3 52.50	Fe_2O_3 52.00
PO_5 46.92 } 99.42	PO_5 47.57 } 99.57

These results correspond in composition with the phosphate of sesquioxide of iron $\text{Fe}_2\text{O}_3 \cdot \text{PO}_5$, mentioned by Will in his "Outlines of Chemical Analysis," and by Dr. Graham, in his "Elements of Chemistry." Thereaction, therefore, must be between one equivalent of phosphate of soda, and one of tersulphate of sesquioxide of iron, by which are formed: one eq. phosphate of sesquioxide of iron, two eq. of sulphate of soda, and one of sulphuric acid, which remains in the water: $2\text{NaO} \cdot \text{HO} \cdot \text{PO}_5 + \text{Fe}_2\text{O}_3 \cdot 3\text{SO}_3 = \text{Fe}_2\text{O}_3 \cdot \text{PO}_5 + 2(\text{NaO} \cdot \text{SO}_3) + \text{SO}_3 \cdot \text{HO}$.

Some further experiments were now tried to ascertain its solubility in some solvent, which might render it useful in medicine.

In hydrochloric and nitric acids, it is readily soluble; in phosphoric acid it is insoluble; in citric and tartaric acids it is slightly soluble, more so on the application of heat, forming green solutions. In phosphate of soda it is insoluble, even when heated; in tartrate of ammonia it is slightly soluble, forming a green solution, which, when heated to boiling, is decomposed, forming a gelatinous mass.

Citrate of soda dissolves it in considerable quantity, but the best solvent for it is the neutral citrate of ammonia proposed by M. Robiquet, as a solvent for the pyrophosphate of iron. In this salt it dissolves readily, forming a green solution with a brown shade; when evaporated and spread on glass, and dried, it forms scales of a brownish-green colour, which much resemble those of the citro-ammoniacal pyrophosphate of iron. The dry salt dissolves more rapidly in cold water than the pyrophosphate, and has a pleasant saline taste; a solution of it on the addition of ammonia, remains clear, but turns to a reddish-brown colour, while the addition of a portion of citric acid turns it to a lighter green colour.

100 parts of this salt on examination, were found to lose 10 parts of water, and furnish 23 parts of Fe_2O_3 , which correspond to 44 parts of $\text{Fe}_2\text{O}_3 \cdot \text{PO}_5$, the remaining 46 parts being the citrate of ammonia. We have thus a citro-ammoniacal sesquiphosphate of iron, which is a reliable preparation, containing 23 per cent. of sesquioxide of iron, and possessing decided advantages over most ferruginous preparations, on account of its pleasant taste, while it will prove more active than the citro-ammoniacal pyrophosphate of iron, as it contains more iron. We may, therefore, hope that a fair trial of this new salt by the medical profession will gain for it the place to which its composition would seem to entitle it. —*American Journal of Pharmacy.*

PHYSICAL SCIENCE.

On the Effect of Great Pressures combined with Cold on the Six Non-Condensable Gases, by Dr. ANDREWS.

IN this communication the author gave an account of some results already obtained in a research with which he is still occupied on the changes of physical state which occur when the non-condensable gases are exposed to the combined action of great pressures and low temperatures. The gases when compressed were always obtained in the capillary end of thick glass tubes, so that any change they might undergo could be observed. In his earlier experiments the author employed the

¹ Read before the British Association.

elastic force of the gases evolved in the electrolysis of water as the compressing agent, and in this way he actually succeeded in reducing oxygen gas to $\frac{1}{300}$ th of its volume at the ordinary pressure of the atmosphere. He afterwards succeeded in effecting the same object by mechanical means, and exhibited to the Section an apparatus by means of which he had been able to apply pressures, which were only limited by the capability of the capillary glass tubes to resist them; and while thus compressed the gases were exposed to the cold attained by the carbonic acid and ether bath. Atmospheric air was compressed by pressure alone to $\frac{1}{371}$ th of its original volume, and by the united action of pressure and a cold of -106° F. to $\frac{1}{575}$ th; in which state its density was little inferior to that of water. Oxygen gas was reduced by pressure to $\frac{1}{324}$ th of its volume, and by pressure and cold to $\frac{1}{544}$ th; hydrogen by the united action of cold and pressure to $\frac{1}{500}$ th; carbonic oxide by pressure to $\frac{1}{278}$ th, by pressure and cold to $\frac{1}{278}$ th; nitric oxide, by pressure to $\frac{1}{310}$ th, by pressure and a cold of -160° F. to $\frac{1}{380}$ th. None of the gases exhibited any appearance of liquefaction, even in these high states of condensation. The amount of contraction was nearly proportional to the force employed, till the gases were reduced to from about $\frac{1}{300}$ th to $\frac{1}{310}$ th of their volume; but, beyond that point, they underwent little further diminution of volume from increase of pressure. Hydrogen and carbonic oxide appear to resist the action of pressure better than oxygen or nitric oxide.

PROCEEDINGS OF SOCIETIES.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

SECTION B.—CHEMICAL SCIENCE.

THE President, Dr. W. A. Miller, F.R.S., &c., Professor of Chemistry, King's College, delivered the opening address.

THE PRESIDENT said that, in the home of Dalton, in the focus of applied chemistry, very few words would be necessary. They could not but remember that, on the last occasion, when the meeting of the British Association was held in Manchester, that illustrious philosopher was still amongst them; and he trusted that the same spirit which actuated Dalton still remained in Manchester to enlighten his native country. Without saying more by way of introduction, he would call their attention to one or two points of progress during the past year. In calling attention to these subjects, he must necessarily refer to debatable ground in science,—but it was in debatable land that progress was necessarily made. He would only touch upon two or three practical applications of chemistry, and two or three theoretical ideas since they last met. The Professor then alluded to the new methods of preparing oxygen and hydrogen, proposed by Deville, which admit of application on such a scale as to allow of the generation of oxygen for manufacturing purposes, and the employment of the oxyhydrogen blast, as a source of heat, in metallurgical operations. The novelty in the preparation of oxygen consists in decomposing by heat the vapour of sulphuric acid, and by a further process, storing up the oxygen in gasholders. The preparation of hydrogen required more care. The metallurgy of platinum had already experienced a remarkable modification, owing to the application of the intense but manageable source of heat obtained by the combustion of these gases.

In connection with oxygen might be mentioned a singular circumstance regarding ozone, which, according to the observation of Schrötter, had been found in a

peculiar species of fluor spar, from Wolsendorf, which, when rubbed or broken, emitted a peculiar odour of ozone.

The active chemist Deville, in following his researches, had discovered a variety of means of obtaining, artificially, crystallised minerals of great regularity and beauty. The methods adopted were chiefly by heating the amorphous substances in a slow current of some gas, such as hydrochloric acid, which was not an unfrequent natural product in volcanic districts.

No discovery, however, had made a greater impression upon the popular mind than that of the remarkable alkaline metals, cesium and rubidium, by Kirchhoff and Bunsen. These eminent men, in investigating the appearances presented by flames coloured by various metallic salts when analysed by the prism, were led, from the appearance of certain bright lines in the spectra, produced whilst they were examining a saline residuum from the waters of the Dürckheim spring, to infer the existence of a substance hitherto unknown. It was found that cesium was present in such minute quantity that a ton of that water, which was the most abundant source of cesium yet known, contained only three grains of its chloride. Taking into account the minuteness of its quantity, and its striking resemblance to potassium, it was not too much to say that the discovery of cesium would have been impossible by any other known method than that which was actually employed. The other metal, rubidium, was somewhat more plentiful; but rubidia also so closely resembled potash, that it would not have been discovered but for the peculiarity of its spectrum.

Referring to the revision of the atomic weights of sulphur, silver, nitrogen, potassium, sodium, and lead by Stas, Professor Miller said that chemist had come to the conclusion that it was not proved that the elementary bodies were multiples of the unit of hydrogen, and in opposition to the opinion of Dumas he had pronounced the law of Prout as imaginary.

Every chemist would read with interest the paper by Graham upon the application of liquid diffusion to analysis. The remarkable conclusion to which the author arrived was that the process of diffusion separated all substances into one or other of two classes, which he distinguished as crystalloids and colloids.

The rapid improvement in the methods of analysis, though not admitting on that occasion of detailed mention, must not be overlooked. A variety of bodies, formerly supposed to be of rare occurrence, were now found in minute quantities, unexpectedly, but widely diffused. The discovery of these small substances was by no means unimportant, for they might aid in solving problems of great interest.

Glancing only for a moment at the important practical subject of the formation of steel, Professor Miller referred to the activity displayed in the pursuit of the organic department of Chemical Science, remarking upon two lines of research as important from their theoretical bearings,—namely, the investigation of polyatomic compounds, and the process of oxidation and of reduction, applied by various chemists, and by Kolbe in particular, to the investigation of the organic acids. The labours of Hofmann upon the polyatomic bases showed completely the principle upon which these bodies might be formed, and he had been enabled to group an unlimited number of atoms of ammonia into one compound molecule. Great progress had also been made in our knowledge of the relations of the organic acids. In conclusion, the President said that Drs. Schunck, Roscoe, and Smith had prepared a Report on the Manufactures of the South Lancashire District, and perhaps few were aware of the large extent to which the science was applied and used in the neighbourhood of Manchester.

In the evening the President gave a lecture in the Concert Hall on "*The New Method of Spectrum Analysis.*"

Professor MILLER said that the subject which the

Council of the Association had desired him to bring before the meeting was one which, amongst the scientific questions of the day, had attracted as large a share of popular attention as any that could be named. It appealed to the imagination so powerfully,—it revealed to us matters of investigation much in the same way as the telescope made known to us what was passing in distant worlds, bringing before us not only those particular changes which appeared to be going on in the bodies which compose our solar system, but it appeared, also, further to reveal to us something of the nature of those distant solar orbs which stud the firmament. Not only, however, did it make known those wonders at a distance, but, like the microscope, it revealed to us also quantities of substances so minute that they would defy any other mode of investigation; it made known to us bodies which not only would defy, but up to the present time had defied, all the sources of analysis and chemical skill. He thought he should best discharge the duty which devolved upon him if he endeavoured to bring before them in an exceedingly brief manner the successive steps by which this discovery had been made. Like all other great discoveries, it was not the work of one individual. It was a work in which, he was proud to say, their own countrymen had taken a prominent part, and many of the members of the British Association had laid the foundations of our knowledge of this point. Newton communicated the first information upon this subject in 1701. We knew nothing of the solar spectrum till Newton revealed it in his great work on Optics in that year, his researches having occupied him many years previously. He would first bring before the audience the spectrum as seen by Newton (not that Newton had the means of producing that artificial light which on that occasion he should employ), gradually bringing them up to the present time. The spectrum was perhaps the most glorious sight the eye could behold.

(Professor Miller then directed a beam of white light upon the wall from an electric lamp, and afterwards directed it, through a prism, to a screen on the platform prepared for the purpose. The beautiful prismatic colours on the screen drew forth indications of approbation from the audience.)

The Professor then went on to say that in the solar spectrum dark lines or bands were discovered by Wollaston in 1801; and the distinguished German philosopher, Fraunhofer, discovered dark lines crossing the spectrum at right angles to its length. Those lines were seen whatever was the material used in the prisms. He would for a moment anticipate his subject by thirty years, in order to show a photographed spectrum on a surface of collodion. This image, it would be seen, was not continuous, but was intersected by bands more or less bright. This he showed in order that his audience might understand what he meant when he referred to Fraunhofer's black lines in the solar spectrum, upon the nature of which much speculation had been exercised. The point next to remember was that other lights than that of the sun also exhibited these lines. For instance, if a solid substance were heated till it gave out light, as all solid bodies would when sufficiently heated, these solid bodies—whether the ball of lime in the oxy-hydrogen light, or the charcoal in the electric light—exhibited broken spectrum, being crossed by bright lines. This led him to several distinct lines of research followed since Fraunhofer. First, the lines in the solar spectra, which he might call cosmical. Next, lines produced by the process of absorption. For the discovery of these lines we were indebted to one of our most illustrious philosophers, who was with them on that occasion—Sir David Brewster. (Applause.) Then there were a series of lights produced by coloured flames. Lastly, there was a series of lines for the knowledge of which they were mostly indebted to Professor Wheatstone—(renewed applause)—the lines produced by the electric spark. Wollaston and Fraunhofer noticed that the electric spark produced spec-

trum, but they went no further. Professor Wheatstone had, however, done so. He had no opportunity of presenting to the audience any of the cosmical lines beyond that he had already shown in the photograph. Sir David Brewster was the first person to throw a light on these lines. He observed that under certain circumstances the sun's rays were absorbed. It appeared certain that particular rays were deepened by the absorbing action of the earth's atmosphere. Then came the beautiful discovery of Becquerell's, in 1842, by which it was made known that there was the same interruption, or black lines, in phosphorogenic rays; and Stokes a few years afterwards showed that the rays which produced fluorescence were interrupted precisely in the same way. But no one had satisfactorily explained the cause of these lines.

He would now lead his audience a second step, for which they were also indebted to Sir David Brewster. He discovered that the dark orange-coloured gas, nitric oxide, had a very remarkable absorptive power upon the sun's rays. This brownish-red gas, placed in a ray of light, and afterwards examined by the prism, showed a series of black bands much more distinctly than those occurring in the sun's light. The moment he put this gas into the course of the ray it produced the black lines. A good many other gases had similar effects. The next point of interest was observed by M. Foucault, who found that by sending the sun's rays through the electric light, we got a black band where the electric light showed a bright one. This led to the discovery which formed the subject of the present discourse. The next step was the discovery of the spectra of coloured flames. In 1822, Sir John Herschel made a series of observations upon coloured lights; and it appeared to Talbot that these observations might be turned to account for chemical analysis. Professor Miller then showed by experiment the observations made by Mr. Talbot upon lithium and strontium, the flames of which were so closely allied in colour that when viewed by the unaided eye but little could be seen to distinguish them one from the other; but the moment they were viewed through the prism there was a marked and striking difference in the bands. Mr. Talbot pointed out that soda and potash gave out very peculiar spectra. It did not matter what preparation of potash was used, the result was the same. In the case of the spectra of the alkaline earths, the lines were very numerous. A map of the spectra [here pointed to] was one published by Bunsen and Kirchhoff; another, going much more into detail, had been published by himself. Passing on to another point of great importance, Mr. Swan, he said, was the first person who endeavoured to estimate the delicacy of these reactions, and he showed that so minute a quantity as the one two millionth and a-half part of a grain of a salt of sodium could be detected by the colour of its flame. Other points also were investigated by Mr. Swan.

The next step was with reference to the spectra of the electric light; and here Mr. Wheatstone gave the first impulse to discovery. In the year 1835, he showed, at Dublin, a map of various spectra produced by the electric sparks passed between different metals, and he discovered the very important principle that each metal had its own peculiar spark, alike whether the electricity employed were voltaic, magnetic, or from the common electrical machine. He further showed these phenomena were not due to the burning of the metals, for he performed his experiments in an air-exhausted vessel, and he put carbonic acid in the vessel and still found the same light. He also examined metals in their melted state, and the spectra of one or two of these the lecturer here showed. The barium spectrum having been exhibited parenthetically, Professor Miller went on to display Mr. Wheatstone's spectra of lead and tin; and he stated that Mr. Wheatstone's investigations had led to the offer of a prize for further investigation, Mr. Wheatstone having been at that time engaged with his electric telegraph. This prize was won by M. Mason,

who published from 1851 to 1855 some important papers on this matter, with engravings of the spectra, giving much more complicated lines than those given by Mr. Wheatstone. Having displayed the spectrum of silver, the learned lecturer proceeded to refer to further experiments made by an American discoverer, and others whom he named. Amongst these were a number of curious observations on the spectra of gases in tubes first filled with these gases, and then as much as possible exhausted. On the table in front of the platform stood an apparatus, consisting mainly of two such tubes, the one containing hydrogen, the other nitrogen, both in imponderable quantities. Electric light passed through these tubes gave, in the one case, a vivid red; in the other, a brilliant blue. It was also found that if two pieces of wire having on them the least trace of lime, or baryta, or soda, were subjected to the electric spark passing between them, the characteristic spectra of those metals were produced. Nobody up to that time had explained the relation of these different facts with the other.

M. Kirchhoff, and simultaneously, Mr. Stewart, investigated the nature of radiant heat, the vibration produced by different degrees of temperature; and both concluded that when a substance was heated it gave out a certain set of rays, which, when cold, it absorbed; or *vice versa*. Having exhibited the light of sodium under the two conditions, to show the contrast, next Professor Miller remarked that the black line in the spectrum before them was the key-note to the whole of Kirchhoff's explanation. The light absorbed there was not lost; it was radiated in all directions instead of being cast on the screen. Kirchhoff proved by theoretical deductions that the dark bands in the solar spectrum were the reverse of the bright bands, and found he could reverse the bright bands, not only of sodium, but of lithium, potassium, barium, and strontium, by putting their flames in the course of the sun's rays. Turning to stellar matters, Kirchhoff had said with great probability that the spectra of Fraunhofer were merely the bright lines which we should see if there were not an intensely heated body in the atmosphere of the sun producing these dark lines. His opinion was that in the luminous atmosphere of the sun, vapours of various metals were present; and that the solid body of the sun behind the luminous atmosphere, emitting rays of all degrees of refrangibility, caused these lines to be observed. Kirchhoff had arranged an extremely sensitive apparatus, by means of which he had been enabled to magnify the solar spectrum in such a way that he could get a spectrum eighteen feet in length, and view every part of it with a microscope. Sir David Brewster and Dr. Gladstone had recently made known the existence of at least two thousand black lines in the solar spectrum, but Kirchhoff estimated them as very much more numerous. Showing them a map of the solar spectrum, Professor Miller said that amongst the lines thus exhibited were a large number corresponding exactly to the lines produced by known metals; and he pointed to and described the drawing of apparatus whereby at once the lines of the solar spectrum could be seen simultaneously and compared with the lines characteristic of mundane metals. Every star had its own series of lines, some of them seeming to show sodium. The sun appeared to contain sodium, potassium, iron, chromium, nickel, and magnesium.

Bunsen, in observing the residue of the water of a certain spring, found spectrum lines which he had never seen before, and supposed them to be due to the presence of an unknown metal, which he afterwards was able to isolate, finding that three grains (only) of it were extractable from a ton of the water. From the beautiful blue lines in the spectrum produced by this metal he named it Cæsium. By the kindness of Professor Bunsen, who had complied with Professor Roscoe's request for a specimen, the lecturer was enabled to exhibit the spectrum lines of this new metal; and Professor Roscoe had also supplied him

with a sample of another new metal thus discovered, and named Rubidium, the spectrum of which he also threw on the screen.

Dr. ROBINSON said he had been deputed to propose a vote of thanks to Professor Miller for the lucid and instructive exposition which he had given of one of the most remarkable discoveries of modern times. As a humble labourer in the same field, he had come forward to express his sense of the high privilege which he had shared with others in hearing those great and magnificent truths so splendidly explained. Familiar as he was himself, from his own researches, with many of those truths, he had never seen them so fully and so beautifully illustrated as they had been that evening. What a wonderful triumph of human intellect it was even to measure the dimensions of the sun; to pass over that vast field that separated him from us, and to weigh that great orb in the balance and to say that it was of such a specific gravity—what a wonderful triumph it was! By the marvellous powers of sight, those powers which linked together for us distant portions of space, it might be conceived that we could pass over that enormous void; but what a triumph it was to say that we could travel there with the understanding—to say that the sun was of such a magnitude, of such a gravity, contained such elements, was composed of such substances, and was a part of our system bound to us by a community of elements! (Hear.) They had that night seen exhibited on the screen those luminous bands which indicated that in the atmosphere of the sun there existed the metals magnesium, sodium, chromium, cobalt, and iron. These were the metals which they knew from other facts pervaded the planetary system. They were found on earth in those mysterious strangers which, under the name of *aeolites*, came down from the planetary space and gave us indications of the common links which bound us together with the other bodies of our system. (Applause.) He concluded by proposing the resolution.

Mr. HOPKINS, the newly-appointed Secretary to the Association, seconded the resolution, which was carried with acclamation, and the interesting proceedings terminated about ten o'clock.

NOTICES OF PATENTS.

Improvements in the Treatment of Vegetable Substances, and in the Preparation of Beverages Therefrom. J. H. JOHNSON. A Communication. (Not proceeded with.)

THE inventor proposes to subject to a kind of desiccating process the leaves, shoots, and other portions of trees, shrubs, and plants, containing tannin or other bitter vegetable principle. When dried, these are to be coarsely pulverized, and used either alone or in admixture with tea, coffee, or any similar substance for the preparation of drinks by infusion or decoction.

In reference to this specification, it is almost needless to remark that, besides having a tolerably comprehensive aim, the novelty of the process is questionable, since the treatment directed is precisely similar to that by which the tea-plant has always been prepared. The production of counterfeit tea from *sloe* leaves, Paraguay tea, and even the old well-known doses of camomile and senna might be advanced as arguments contradictory to the novelty of the suggestion. For ourselves, we should much prefer to accept the cup of tea prepared as a wholesome beverage without the admixture of any sort of medicinal nastiness which might originate with a proposal such as that under discussion—a proposal which fortunately has in the present instance failed in taking practical effect.

Certain Applications of Chapapote and its Products. J. J. DE ARRIETA.

THIS invention relates to the employment, for various use-

ful purposes, of a certain natural matter called, in Cuba, *chapaote*, whether in its concrete, viscous, or more or less fluid condition, and whether alone or combined with other substances. It may be applied as a coating to wood, metal, stone, paper, leather, textile fabrics, fibrous materials, and to a variety of other substances, to all of which it communicates an additional degree of protection not only from wear and tear, and the ravages of insects, but from the destructive action of damp and oxidising influences generally. It may also be employed as a waterproofing material; as an impervious varnish or coating in the construction of air-tight vessels; it serves to strengthen and likewise to ornament wood and other porous and fibrous materials. The *chapaote* is for any of these purposes, applied in its natural condition or in combination with gums, resins, wax, and fatty matters, essential oils, including those produced by the destructive distillation of the substance itself; with gutta percha and India-rubber, spirit and naphtha, hydrocarbons, varnishes, or conjointly with any one of the numerous classes of mineral substances, individually specified. Also with agents for resisting combustion, and other agents and matters.

The comprehensive nature of the specification, of which the above is but an outline sketch, reminds us of the early patents relating to the applications of gutta percha. The significant finale, "and other agents and matters," surely leaves nothing out of enumeration. The West Indian natural product here described, seems to bear a general resemblance both to caoutchouc and to gutta percha, and apparently to be capable of similar application.

Mounting of Tablets or Show-bills, &c. B. G. GEORGE, Hatton Garden.

THE object of the patentee is to produce, at a small cost, an embossed show-card or tablet that will retain its shape notwithstanding the variable temperature and humidity of the atmosphere. By means of a metal die and a counter-die of the pattern or device required, and a printing surface also of the same pattern, the inventor takes first a printed impression upon paper or card; this, thoroughly wetted, is then laid face downwards on the embossing die, and the counter-die then brought down with pressure upon the back of the sheet produces a representation which may be either in relief or *intaglio* at pleasure. A plaster-cast is also prepared from one or other of these dies, which is intended to be used as a backing to preserve the shape of the card, the surfaces being permanently fastened by any suitable adhesive composition, and, if needful, again dried under pressure to ensure perfect contact and accuracy of form.

The mode of proceeding thus minutely described, displays certainly a high degree of ingenuity, and would be calculated to produce a kind of advertising card well adapted for exhibition in our treacherous climate. The additional precautions of varnishing the face of the tablet and of closing, by an after treatment, the pores of the plaster, would no doubt further contribute to the permanent character of the result.

Improvements in the Manufacture of Plate Glass, and in Furnaces employed therein. R. W. SWINBURNE, South Shields, Durham.

THE first improvement refers to the construction of furnaces for melting or firing the materials for the manufacture of plate glass, which furnaces are constructed after the fashion of reverberatory furnaces; but the fire is so placed that the crucible or melting-pot is entirely surrounded with flame. The second improvement consists in a new mode of casting plate glass. For this purpose the patentee constructs a furnace capable of receiving the large pots usually employed in the manufacture of crown or sheet glass. He places these melting-pots at such a distance from each other as will admit one or more

claspers or movable pots to stand between them, and at such an elevation that the brims or edges of all the pots in the furnace are at the same level. The "metal" or melted glass is ladled out of the larger pots into those adjoining, which when filled are removed to the casting-table, whereon their contents are thrown to be rolled into plate glass. The mode of removing them and emptying their contents on the casting-table, is the particular object of this part of the invention. Without entering further into the mechanical details of the specification, it is sufficient to state that the crane or lifting apparatus usually placed above the casting-table is dispensed with, and that cutters or knives for trimming off the edges of the newly-cast plate are superadded to the ordinary equipment.

Improvements in the Manufacture of Manure when Urine is Used. H. PHILLIPS and J. BANNEUR.

IN manufacturing manure when employing urine, it is first collected and stored in vessels, where it is allowed to become decomposed. The construction of these collecting vessels is described as having traps with water-joints, and all the air-outlets provided with an arrangement for holding charcoal or other deodorising material, so that no offensive vapours can make their escape into the atmosphere. It is proposed to convert the stale urine into manure by saturating therein any dry vegetable matter, by preference the ground spent bark of tanners; the materials are allowed to stand in a heap for some weeks, then spread out to dry, and again moistened with the same liquid. This treatment is repeated until the fertilising materials are sufficiently concentrated, and towards the end of the operation it is preferred to employ a small proportion of sulphuric acid in admixture to prevent the escape of ammonia.

There is no doubt that a very valuable manure would be furnished by the process here described; but whether the economy of the process will enable it to compete with other simple means frequently practised on the farm, is a little questionable. The priority of the claim for the employment of charcoal in the air-passages is undoubtedly due to Dr. Stenhouse.

Colouring Matters for Dyeing, &c. M. J. STARK, Norwich.

THIS invention refers to the production of purple colouring matters from aniline, toluidine, cumidine, or any other analogous substances. The patentee converts either of these bases, or any mixture of them, into an organic salt by the addition of an acid, the use of hydrochloric alone, or with nitric acid being preferred. The saline product resulting from the admixture of acid and base is to be diluted with about an equal bulk of water, and gradually mixed with any soluble ferri-cyanide, that of potassium being used by preference. The proportion in which this latter salt is recommended to be employed, amounts to a weight equal to that of the aniline originally taken. After the whole of the acid solution of aniline has been added, the temperature of the liquid is raised to ebullition, and maintained at a boiling heat for a period of one or more hours, varying with the bulk of material operated upon. When the whole has been sufficiently boiled it is allowed to cool, and the greyish-blue precipitate which will have formed separated from the liquid by filtration or subsidence and decantation. This is collected and washed, and while still moist boiled for an hour or so with tartaric or other acid—one part of crystallised tartaric acid to two parts of water—which treatment will have the effect of extracting the brilliant colouring matter from the bulky precipitate; but for the perfect removal of the same, this boiling with acid may require to be repeated. The filtered solution may at once be employed for dyeing purposes, but the inventor prefers to treat it as follows:—When cold it is mixed with ammonia (or with any alkali or alkaline earth) in slight excess by which the purple

dye is precipitated and may be collected by filtration. It should finally be re-dissolved in alcohol or methylated spirit to furnish a dye of a very permanent character, and of a brilliant purple colour, to which the inventor has given the name "chyraline."

This process differs from that originally described by the discoverer, Mr. Perkin, only in respect to the substitution of *ferri-cyanide* for *bichromate* of potassium. Both are oxidising agents, precisely similar in their behaviour as regards their action upon organic bases—thus, in the colour-test for strychnine, these salts are, in conjunction with sulphuric acid, employed almost indiscriminately—so will the products of these patented processes be identical, whether "mauve" or "chyraline," be the name under which the purple dye is introduced to the public.

Grants of Provisional Protection for Six Months.

2010. John Lancaster, Princes Street, Bedford Row, London, "A new method of producing sand."

2024. Edmund Edwards, Beaufort Street, Strand, London, "Improvements in machinery or apparatus for separating mineral ores, coal, and other substances from impurities."—Petitions recorded August 14, 1861.

2034. Francis Allen Kain, Redhill, Reigate, Surrey, "An improved manufacture of artificial stone or earthenware applicable for bricks, tiles, retorts, railway sleepers, and other articles."

2059. William Gossage, Widnes, Lancashire, "Improvements in the manufacture of certain kinds of soap and in the construction of apparatus to be used in such manufacture."

2096. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in the preparation of pulp for paper."—A communication from Mademoiselle Jeanne Françaises Marie de Cassan and Monsieur Jean Baptiste Meyer, Paris.

2100. Louis Marino Casella, Hatton Garden, London, "Improvements in mercurial thermometers."

Notices to Proceed.

1039. Samuel Fox, Stockbridge Works, Deepcar, near Sheffield, Yorkshire, "Improvements in hardening and tempering steel."

1055. John Marshall, Liverpool Road, London, "Improvements in preventing the fracture of metals from crystallisation."—Petitions recorded April 26, 1861.

1079. Jacques Meyer, Berlin, Prussia, "Certain new chemical combinations, and for the application thereof to fixing aniline and pigment colours in printing and dyeing, to tanning, waterproofing, and other industrial purposes." A communication from Dr. Francis Leopold Sonnenschein, Berlin, Prussia."—Petitions recorded April 30, 1861.

1095. John Charles Wilson, Bucklersbury, London, "Improvements in machinery or apparatus for the manufacture of sugar."

1646. John Cass Smart and Adam Aitchison, Scarborough, Yorkshire, "Improvements in the manufacture of charcoal."—Petition recorded June 27, 1861.

1726. Alfred Noble, Bristol, "Improvements in obtaining products from alkali waste and gas lime refuse."—Petition recorded July 6, 1861.

1817. Robert Mushet, Coleford, Gloucestershire, "Improvements in the manufacture of cast steel."—Petition recorded July 19, 1861.

2059. William Gossage, Widnes, Lancashire, "Improvements in the manufacture of certain kinds of soap, and in the construction of apparatus to be used in such manufacture."—Petition recorded August 19, 1861.

A Solution of Starch for Volumetric Analysis is prepared by Dr. F. Mohr, by trituration, with concentrated chloride of zinc, and diluting with water; the solution is not clear, but after precipitating the zinc by carbonate of soda, the filtrate is transparent.—*Ann. d. Chem. und Ph.* xxxix. 231—233.

CORRESPONDENCE.

New Source of Rubidium and Cesium.

To the Editor of the CHEMICAL NEWS.

SIR,—The enclosed letter from Professor Bunsen may be interesting to your readers, as it shows in what way lithium is associated with the two new alkali metals in its chief sources, lepidolite and triphyllyne.—I am, &c.

W. STEVENS. SQUIRE.

37, Acacia Road, N.W.

Dear Dr. Squire, — I only returned last week from Switzerland, and, unfortunately, with inflammation of the eyes, which has confined me to a dark room for the last eight days. This is the reason I have not written sooner to thank you for the triphyllyne residues which you kindly brought me, and to express my regret that I was, by my absence from home, deprived of the pleasure of seeing you when in Heidelberg. The residues are comparatively very rich in rubidium and cesium, and contain much more of the latter than of the former, while the residues from lepidolite contain scarcely any cesium. To judge from the reaction in the spectrum apparatus, we ought to be able to prepare eight or ten grammes of chloride of cesium from the whole quantity. Thanking you once more for your kind gift, by which you have put into my possession so rich a source of cesium,—Believe me yours very truly,

R. W. BUNSEN.

Heidelberg, September 16.

Printing Prussian Blue.

To the Editor of the CHEMICAL NEWS.

SIR,—Some time last year I was annoyed to find that, in endeavouring to prepare flame-proof fabrics by the use of a mixture of starch and sulphate of ammonia, the condemnation of the use of this salt by Verreman and Oppenheim was not undeserved, as the iron is acted upon in the subsequent stages of the laundry process. This is not unaccountable; for I have before observed that copper foil, in presence of air heated with sulphate of ammonia, liberates ammonia. Iron does the same. I have seen a beautiful crystalline compound, white, and in appearance like citric acid, containing iron and ammonia and sulphuric acid. Now, I have found that by printing on to cloth, in any pattern, sulphate of ammonia and yellow prussiate of potash it will remain white for any length of time; but upon passing the piece over a hot surface of iron, the printed parts instantly strike blue, especially if slightly dampened. Would not this be available in printing blues and browns on the large scale by iron and copper rollers?

I am, &c. JOHN S. BLOCKET.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

The Chlorides and Bromides of Tungsten.—C. Blomstrand describes (*Journ. für prakt. Chem.*, Bd. lxxxii. s. 408) four chlorine compounds of tungsten.

1. *Perchloride of Tungsten*, WCl_6 , remains as the final product of the action of chlorine (when no oxygen is present) on metallic tungsten or the sulphide, at a strong heat. The vapour is orange coloured. It crystallizes indistinctly, decrepitates on cooling, and is slowly decomposed by water forming the coloured tungstic acid.

2. *Chloride of Perchloride of Tungsten*, W_2Cl_8 or $(WCl_4)_2$, formed probably in small quantity in the preparation of the foregoing compound, and obtained pure by the reduction of that salt with hydrogen. Dark grey like the above, vapour greenish yellow, fuses with difficulty, crystallizes easily, cools without decrepitating, is quickly

decomposed by water with the separation of the blue oxide of tungsten.

3. *Monozchloride of Tungsten*, WCl_2O , or $(2WCl_3 + WO_3)$ is best prepared by gently heating the sulphide of tungsten made in the wet way in chlorine. It is of a beautiful red colour, and crystallizes in transparent needles. It is very fusible; the vapour is orange-coloured. Decomposes in water like the former.

4. *Binozchloride of Tungsten*, WCl_3O , or $(WCl_3 + 2WO_3)$ is formed simultaneously with 1 and 3 in the first stage of the action of chlorine on pure oxide of tungsten. It has a yellowish colour, is vapourised into a colourless gas without fusing, crystallizes in scales, and is decomposed, like the others, by water.

The author also describes three bromides:—

1. *Bromide of Perbromide of Tungsten*, W_2Br_5 , formed by the direct action of bromine on metallic tungsten. It is black, gives dark purple brown vapours, crystallizes easily in distinct needles, decomposes in water, depositing the black oxide.

2. *Monozbromide of Tungsten*, WBr_2O , or $(2WBr_3 + WO_3)$ formed under the same circumstances as the corresponding chlorine compound. Colour resembles that of the preceding salt, but is lighter, vapours brown red, usually crystallizes in perfect needles, decomposes in water.

3. *Binozchloride of Tungsten*, also formed like the corresponding chlorine compound. Ordinarily brass or mosaic gold coloured crystalline scales, infusible, decomposing in water.

The author believes in the existence of a *perbromide*, WBr_3 , but thinks Bonnet's oxybromide— $WBr_3 + W_2O_5$ —apocryphal.

II. ORGANIC CHEMISTRY.

Introduction of Hydrogen into Organic Compounds.—Zinin (*Bulletin de St. Petersburg*, T. iii. p. 529), and Kolbe (*Annal. der Chem. und Pharm.* Bd. cxviii. s. 122), have experimented on the direct addition of hydrogen to organic compounds. The latter digested a hot saturated solution of benzoic acid and a little hydrochloric acid with sodium amalgam, and in this way obtained bitter almond oil, another oil which becomes a crystalline solid on cooling, and a volatile acid. When the action takes place in an alkaline solution, the changes are different. No bitter almond oil is obtained nor the crystalline oil, but more of the new acid is formed, which Kolbe intends to investigate further.

Zinin continues the researches on benzile, some account of which was given at page 223 of our last volume. He there showed that benzile might be converted into benzoïn by the direct addition of hydrogen. He now shows that by continuing the action longer, new bodies may be formed containing more hydrogen than benzoïn. The author made a boiling solution of one part benzoïn and three or four parts alcohol of 75 per cent., and to this he added one part of strong alcohol saturated with hydrochloric acid gas, and then half a part of finely-granulated zinc was slowly introduced into the mixture. As soon as the violent reaction ceased, another half a part of the alcoholic solution of hydrochloric acid gas was added, and the mixture boiled down to half. It was then poured off from the undissolved zinc and mixed with water, whereupon an oily body separated which soon cooled into a crystalline mass which was purified by re-crystallization from alcohol. It was then obtained in rhombic tables, which fused at 55° . This new body is more hydrogenated than benzoïn; but the elementary analysis, the author says, presents unusual difficulties. By the action of nitric acid and of bromine on this new body, other crystallizable bodies are formed.

Bitter almond oil dissolved in the alcoholic solution of hydrochloric acid and boiled with zinc, forms a thick oily body which sticks to the sides of the flask, and on cooling becomes solid and resinous. It is freely soluble in ether,

and from the solution part crystallizes out, the remainder separates as an oily mass, in which, after a time, other crystals form.

MISCELLANEOUS.

Smelting Spanish Iron Ore in England.—Mr. Morrison, of Ferryhill, has just imported a cargo of very fine quality iron ore from Santander, which is said to have proved very valuable in the South Wales district for improving the quality of iron produced from British ores. The ore exists in enormous deposits within a quarter of a mile from the river, and is worked most inexpensively. Many hundreds of tons per week could easily be raised and shipped, and whilst the present low freight continues a profit can be realised upon shipment either to Wales or the North.—*Mining Journal*.

The Oil Wells of Pennsylvania—Curious Spouting Phenomena.—A correspondent of the *United States Railroad and Mining Register*, writing from Oil Creek, Pa., gives some very interesting information respecting the oil wells and the nature of the rocks in which they are found. He states that the yield from the wells has greatly decreased, and that the oil well region, for practical working, is of very limited extent. He says:—"Along the banks of the Allegheny, for five or six miles above the mouth of Oil Creek, and at a point some three miles below Franklin, operations have been and continue quite successful, so that these may be counted among the reliable sources of supply. Beyond them, the Allegheny river, for profitable oil production may be considered dry." The oil in most of the wells is found in a series of sandstone rocks, which are usually met with at about 190 feet below the surface, and the pockets of oil are located in fractures. In many borings, no oil has been found; while in adjacent drillings, which have been pierced into vertical fractures, large quantities have been obtained. Many wells which once yielded twenty barrels per day are now dry. The usual depth of working the wells is 200 feet, but the oil is obtained at various depths. After drilling down 200 feet, a mass of shales 100 feet in thickness is encountered, in which no oil of any account is obtained. At about 325 feet in depth, there is about 40 feet of white sandstone, near the top of which "spouting wells" are found. This is the second oil yielding strata. After passing through this rock, another mass of shales and fire clay is met with; and at 450 feet in depth, another stratum of sandstone, which forms the third oil-bearing series. Here the "big spouting wells" are found, the yield of which is from ten to two hundred barrels per day. These spouting wells are curiosities; they are genuine land whales, and twice as oily as their rival sea monsters. The spouting is caused by compressed gas, and the flow is periodical. One well will flow for five minutes with intervals of ten. Its neighbour will flow once an hour; while one flows four times, and another six times, in twenty-four hours. Again, the ten-minute well will change to a regular flow of three or four hours' duration. Most of them grow gradually less vigorous, and diminish in yield until they finally cease; but some of them stop suddenly.

ANSWERS TO CORRESPONDENTS.

G. F. Rodwell.—Received, with thanks.

R. S.—See Dr. Roscoe's paper read before the British Association, reported in the present number.

John S. Blockey.—The patents referred to in the *CHEMICAL NEWS* for September 7 (page 135), bear the following dates and numbers:—C. H. G. Williams, No. 67, dated January 10, 1861; C. A. Girard, No. 97, dated January 12, 1861; V. Hall, No. 101, dated January 14, 1861. With a view to identification, we propose, in future, to furnish these particulars in connection with every patent coming under our notice.

THE CHEMICAL NEWS.

VOL. IV. No. 95.—September 28, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Presence of Nitrates in Mineral Waters, and on the Production of Saltpetre, by R. HAINES, M.B., Professor of Materia Medica, Grant Medical College, Bombay.

(Continued from page 155.)

THE existence of nitrates in mineral waters is no new discovery. Berzelius, writing in 1826, gives directions

for the estimation of the nitric acid in them;¹ and H. Rose's "Manual of Analytical Chemistry," published in 1830, describes the method for estimating it, which I attempted to apply.² The proportion, however, that is present is usually very minute indeed, and in the greater number of published analyses it has escaped detection altogether. I have collected together, in the following table, all the analyses I can find on record in which nitric acid occurred in any notable quantity. The number is very small. The quantity of water referred to has been reduced, for the sake of comparison, to the British imperial pint.

Locality.	Saldschütz ³ in ?	Saldschütz ⁴ (Hauptbrunnen).	Flushing, ⁵ in Zealand, Netherlands.	Zwolle in Overijssel, Netherlands.	Svanda ⁶ in Banat, Hungary.	Highgate, ⁷ near old Churchyard.
Well or spring . . .	Spring	Spring	Spring	Spring	Spring	Well
Age or character of strata	Volcanic	Volcanic	Alluvium on Eocene	Alluvium on Eocene	?	Eocene (gravel and clay)
Name of Analyst . . .	Struve	Steinmann	Gunning	Gunning	Ragsky	Noad
Grains of nitric acid in imperial pint . . .	0.781	16.459	1.463	1.120	2.379	4.238

I was struck with the circumstance that the springs which contained by far the largest quantity, arose in a volcanic district.⁸ The similarity in this respect of Aden, actually an extinct volcanic crater, is at once suggested; the trappean formation of the Deccan, in which Nassik stands, is no less of volcanic origin, though under altered circumstances. May we surmise, then, that the volcanic nature of the superficial strata is in some way favourable to the occurrence of nitric acid? Before proceeding to this question, let us dispose of the other instances recorded above.

The occurrence of nitric acid in the Highgate well explains itself. The well is close to the old and crowded churchyard, the organic matter from which, undergoing putrefaction, evolves ammonia, and the nascent ammonia, oxidised by the atmospheric air in the loose earth, is converted into nitric acid. It is a notorious fact that while the deep wells of London, which are sunk through

the London clay, yield an exceedingly pure, though often alkaline water, the water of the shallower wells is generally contaminated with organic matter and nitrates. In the water from the parish pump of St. Clement Danes, in the Strand, which, in accordance with a venerable, but somewhat disgusting ancient practice, was placed close to the churchyard railings, the same thing was observable in a very marked degree. In the springs in the Netherlands, and the Banat mentioned in the table, the nitric acid is not present in any large quantity, and I have no data on which to explain its occurrence.⁹

Nitric acid is not a constituent of any rock masses or strata; it is found in minute quantity in rain water, but, with this exception, there is no doubt that it is exclusively formed at or near the surface of the ground (the production of nitre in the so-called nitre plantations is a notable instance of its mode of formation); hence, the nitrates must be among the first matters dissolved by the rain water as it percolates downwards, while the other salts are more gradually dissolved out at a greater depth. Considering the abundance in which alkaline and earthy nitrates are found on the surface of the ground in India, it is a matter of considerable surprise that they have not been observed more frequently in the water of springs and wells; but this, apart from the limited number and accuracy of observations that have been recorded, may be explained, I think, by the circumstance of their formation at the surface. The fact

¹ "Analysis of Inorganic Bodies," translated by G. O. Rees, 1833, p. 88.

² Griffin's "Translation," 1831, p. 374.

³ Brande's "Manual of Chemistry," Fourth Edition, 1212.

⁴ Ure's "Dictionary of Arts," &c., 1851, Suppt., p. 266.

⁵ Liebig and Kopp's *Jahresbericht*, 1854, p. 767.

⁶ *Ibid.*, 1853, p. 712.

⁷ *Quarterly Journal of Chemical Society*, iv. 20; Pereira's "Materia Medica," Fourth Edition, i. 299.

⁸ In the list given, in Ure's "Dictionary of Arts," &c., Saldschütz is said to be situated in a volcanic region; I have not been able to discover its precise locality. The most marked volcanic region in Germany is the Elfel district on the Rhine, although a line of volcanic action of older date extends from thence eastward as far as Silosia. (See "Daubeny on Volcanoes.")

⁹ Lyell mentions the existence of no less than "five distinct groups of volcanic rocks" in Hungary, and also that "pumice occurs in great quantity." ("Manual of Elom. Geol.," Fourth Edition, 421.)

is, that, unless the ground is unusually porous, or the fall of rain excessive, these surface salts are not carried down to any great depth; and as the upper surface dries, the saline solution continually remounts upwards by capillary attraction, leaving, by its own desiccation, the saline efflorescence which we observe so abundantly in the drier parts of India; thus the nitrates, for the most part, merely follow this alternating march from a few feet in depth to the surface, and *vice versa*.

But if the concomitance of a large amount of nitric acid with volcanic formations be anything more than accidental, how are we to explain it? Let us enumerate the circumstances which are essential to the formation of nitric acid; they are:—1st, ammonia, or substances capable of producing it, as nitrogenous organic matters; 2nd, oxygen; 3rd, an alkali or alkaline earth, which, by its so-called disposing affinity for the acid, determines the combination. There is a fourth and scarcely less essential condition, the presence of a body which is capable of acting as a medium of combination between the oxygen and ammonia. Now, volcanic products are very rich in alkalies, and, what is more, they are in many instances exceedingly prone to decomposition and disintegration by meteorological agencies, whereby the alkalies become set free. Oxygen is, of course, always at hand. It is not so evident at first sight how volcanic formations can meet the remaining two conditions. Organic matter, as such, they can hardly contain; and, if ammonia is there, it is clear from their origin that it cannot be in the solid substance, but only in the interstices of the fragments or on their surfaces; and I see no reason why this should not be the case, but, on the contrary, much to make it probable. A great number of solids possess, in different degrees, the power of condensing gases upon their surface, ammonia in particular; and when the surface is multiplied by porosity of structure, this condensing power becomes perfectly enormous. The property is most marked in platinum finely divided, as in platinum black, less so in spongy platinum; less again, but still in a very marked degree, in charcoal, and by a decreasing gradation in pumice stone, oxide of iron, alumina, common clay, and sand. Has pumice, then, or pumice-like scoria, such as the loose cindery mass upon which the cantonment of Aden is built, the power of condensing and oxidating ammonia, in presence of the alkali furnished by its own disintegration? The question might be solved by an examination of the well water of pumice-yielding districts. If Saidechütz be, as I suspect, in the Eifel district, this may well be the origin of the nitric acid in the water, as pumice is found in great abundance in the neighbourhood, especially of Neuwied and Andernach, being used as a building stone at the latter place. I am in doubt how far the occurrence of nitric acid in the wells at Nassik can be explained in this way, as it is scarcely probable that porous scoræ are found there. Perhaps, considering how very local the distribution of the nitrates is in these wells at Nassik, and the circumstance alluded to of the quality of the waters being liable to great and sudden variation, the nitric water may find its way from near the surface through open fissures in the rock, which allow it to pass at times with great freedom and rapidity.

Dr. Bradley notices the existence of scoræ on the edge of the crater of the Loonar lake in the Dekkan¹⁰; does the water of the lake contain any nitrate? Mr. Malcolmson states the solid constituents in 1000 grains of the water to be:—

	Grains.
Muriate of soda . . .	29
Sesquicarbonate of soda . . .	4.2
Sulphate of soda . . .	0.1
Potassa . . .	a little
	33.3 ¹¹

But nitric acid may easily have been overlooked. A fresh analysis of this water is much to be desired.

The question of the formation of nitric acid¹² and of the circumstances which favour it, appears to be one of peculiar interest in this country, where the production of saltpetre is so abundant as to give India an almost complete monopoly of the article in the commerce of the world. What connection is there between the occurrence of carbonate of soda in the soil (*Sujee muttee*, or Dhobees' earth) and that of nitrate of potassa? Does carbonate of potassa ever occur in the soil, as carbonate of soda does? I strongly suspect that the greater part of the nitric acid is, in fact, in combination with soda, and that in the rude native methods of extracting the saltpetre, the nitrate of soda is left behind in the mother-liquor, or drains away from the moist salts in the process of carriage. I would urgently recommend a more careful study of the nitre-yielding earths, as, if my conjectures are correct, an immense saving might be the result of the application of a little scientific knowledge; for I am persuaded that the productive power of India, in regard to the nitrates at least, is capable of being extended to many times its present amount.

(To be continued.)

On Atmospheric Ozone,¹³

by Dr. T. MOFFATT, F.R.A.S., F.G.S., &c.

THE mean daily quantity of ozone is greater with readings of the barometer below than above the mean, and greater when the range of the barometer and the number of its oscillations are above the mean. It is greater when the mean-daily and dew-point temperatures are above the mean, but greater when the degree of humidity is below than when above the mean. When the wind

¹¹ *Ibid.*, p. 30.

¹² On the formation of nitric acid, see Liebig's "Chemistry of Agriculture," Part II., chap. viii. and Knapp's "Chemical Technology," I., 356 seq.

¹³ Kuhlmann supposes that, in the formation of saltpetre, when the mixture contains no carbonate of potassa, but only carbonates of lime and magnesia, ammonia is first formed from the nitrogen of the decomposing organic matter; that this forms, with the oxygen of the atmosphere, nitrate of ammonia, which, with the carbonates of lime and magnesia, is decomposed into nitrates of lime and magnesia, and carbonate of ammonia, so that the ammonia is at once the agent in the formation of nitric acid, and of its transfer to the fixed bases. The same observer invariably found ammonia in the crude saltpetre ley, combined partly with carbonic acid and partly with fixed acids. This is also in accordance with the experiments of Collard de Martigny, who found that when milk of lime was kept in a closed and partly-filled vessel, with a little ammonia, and frequently shaken, for six months, nitric acid was formed. Longchamp (*Ann. Chim. Phys.*, xxxiii. 5; xxxiv. 215) maintains that nitrogenous organic compounds contribute nothing to the formation of saltpetre; but that the water contained in porous substances, such as chalk, absorbs oxygen and nitrogen from the air, and condenses them into nitric acid. The experiments of Gaultier de Chanbry (*Ann. Chim. Phys.*, lii. 24), according to which great quantities of nitrates are found in limestone hills, which contain but a mere trace of organic matter, favour this view. It may, however, be admitted that, under certain circumstances, as in electrical discharges in the air, the nitrogen, the nitric acid, and the carbonate of ammonia of the atmosphere, contribute to the formation of saltpetre, without denying that, in ordinary cases, the nitrogen of the organic matter plays the principal part. (Gay Lussac.—*Ann. Chim. Phys.*, xxxiv. 56; compare Fontenelle, *Journ. Pharm.*, 14), Gmelin, "Handbuch der Chemie," 4te Aufl. I. 302.

¹³ Read before the British Association.

¹⁰ Carter's "Geological Papers of Western India," p. 766.

is from points of the compass between N.W. and S.E. by N. above the mean number of times, ozone is at its minimum, and when it is in point S.S. of these above the mean number, it is at its maximum. It is also at its maximum when the wind is above its mean force. When rain is above the mean quantity ozone is also at its maximum, and also with hail; but the quantity is smaller on days with snow and sleet than on days without them. With fog it is below the mean quantity. It is above it with cirri, halos, aurora, and the zodiacal light, but below it with thunder, thunder and lightning, and thunder storms; and it is at its maximum with negative, and minimum with positive electricity. The mean daily quantity is greater with decreasing than with increasing readings of the barometer, and it is three times greater with wind in points S. of E. and W., than it is in points N. of these,—the greatest quantity (3.5) with S.W., and the smallest (0.8) in the N.E. points. Ozone periods may be said to commence invariably with decreasing readings of the barometer and increase of temperature, and with winds from S. points of the compass; and to terminate with increasing barometer readings, decrease of temperature, and wind from N. points of the compass. These results are from 296 periods. By far the greatest number of ozone periods commence in S.E., and a great majority terminate in N.W. points. Indeed, although the commencement of ozone periods in N.W. points is not uncommon, the S.E. may be called the points of their commencement, and the N.W. the points of their termination. The quantity of ozone is greater in the night than in the day. It is greater with new and full moons than with the first and last quarters, and it also varies with the seasons. It is greater in January, February, and March, than in April, May, and June; but greater in the latter months than in July, August, and September. In these it is at its minimum, and it again increases in October, November, and December. The greatest quantity is in April (2.3) and December (2.4), and the smallest in July (1.3) and August (1.3). The greatest number of ozone days is in April (25), and the smallest in August (29) and November (17). Whatever tends to a deflection in the direction of the wind leads to a corresponding result in ozone observations, and a town, chemical works, drains, and cesspools, in fact, at any locality, in which the products of combustion and decomposition are in sufficient quantity to decompose ozone, the air will be de-oxygenated, and the wind or current passing over it will be non-oxygenated. The quantity of ozone increases with increase of elevation above the level of the sea. The following results were obtained from observations taken at fifteen stations along a tidal river and its estuary. The stations varied in height from three feet to six hundred feet; and in distance from the river, from its bank to eight miles inland. Some of the stations were in towns and villages; from these we find that ozone, as a rule, increases with increase of elevation, and decreases with increase of distance from the river; and that it is invariably in greater quantity in the open country than in towns and villages. This, I believe, is a universal law. It is the same on the Alps as at lower elevations. The following results I obtained from observations forwarded to me by a member of the Alpine Club. The observations were taken at elevations varying from 740 to 8000 feet. The mean daily quantity of ozone from 740 to 2000 feet is 15; from 2000 to 4000, it is 33; from 4000 to 8000, it is 77. [The degree of humidity at the lowest elevation was 58; at the next, 83; and at the last, 83 also.] Ozone is a highly oxidized body, and it is easily

decomposed by oxidable substances. If a test-paper prepared with iodide of potassium be freely exposed to the air in a locality where the quantity of these substances is at a minimum, it will in time become deeply coloured brown, and ozone will be said to be at its maximum. If a similar paper be placed in a locality where the quantity of oxidable substances is at a maximum, as over or in the neighbourhood of drains and cesspools, the paper will remain uncoloured, and ozone will be at its minimum; and if the slip that becomes brown be placed in the latter place, it will lose its colour. In the first condition the ozone oxidizes the potassium and sets the iodine free. In the second, the oxygenated air meets with incompletely oxidized substances, which are more easily oxidable than the potassium; and in the third instance the brown colour of the paper is removed by sulphuretted hydrogen. The ocean, with the wind that blows over it, is represented by the first condition, and the land, with its wind bearing the products of combustion and putrefaction, by the second and third conditions. The conditions of an ozone period are undoubtedly those of the south or equatorial or ocean current of the atmosphere, and those of a no-ozone period are as clearly those of the north or Polar or land current. With the former we have low readings of the barometer, with the maximum of its range and oscillations increasing mean-daily and dew-point temperature, maximum of rain, prevalence of cirri, halos, and high winds, with negative electricity; with the latter we have high readings of the barometer, minimum of range and oscillations, decreasing temperature, no halos, no cirri, low winds, settled weather, and positive electricity. The atmospheric conditions, with ozone and those with no ozone, are so invariable, and ozone periods so frequently commence and terminate with S.E. and N.W. winds, that the points of the compass might not inaptly be arranged into four sets—viz., the equatorial or ozone points, the Polar or no ozone points, and the transition points, the south-east, the points of transition from the north or Polar to the south or equatorial, while the north-west are those of change from the latter to the former. In a medico-meteorological sense I am not prepared to state that atmospheric ozone produces any form of disease, but I have no hesitation in saying that it prevents diseases of the epidemic character, by removing their causes. The maximum of diseases undoubtedly takes place while the oxygeniferous current has the ascendancy, but they are of a sporadic nature, and are caused by the vicissitudes of weather, electrical influences no doubt playing an important part. The maximum of deaths takes place while the wind is in N. points. By far the greatest number of diseases, however, take place in the N.W. and S.E. points, or at the commencement of ozone periods, and these are chiefly affections of the nervous and muscular systems, from which it would appear that derangements in the nervous and muscular forces take place at the time of transition from the no-ozone and positively electric to the ozone and negatively electric current of the air. The diseases peculiar to the calm, which is also a no-ozone condition, are of an epidemic nature, such as cholera and choleraic diarrhoea. These results are deduced from 2727 cases of disease and 1149 deaths which occurred at Hawarden in a period of ten years. As the S. is the higher and oxygeniferous current, and the N. the lower and no-ozone current of the atmosphere, in a medico-meteorological sense there ought to be some analogy between the higher strata of the atmosphere and the S. wind and between the lower strata and the N. wind, and observation shows that as

regards ozone and deaths they are similar. All who have paid any attention to the atmospheric conditions of a cholera period must have observed that the readings of the barometer are remarkably high, that they slowly attain their maximum, and that when they begin to decrease they as slowly approach their minimum. While the barometer is increasing, the wind continues to veer from N. to N.E. and E., until there is a perfect calm. At first the air is clear and the sky cloudless. The air becomes less clear, it thickens to haze, and the sky can no longer be perceived. There is no ozone, and the brown test papers rapidly lose their colour. At the commencement of the calm there are a few cases of diarrhoea, and as it continues cases take the character of choleraic diarrhoea, and at last a few cases of cholera occur. The calm continues, the haze thickens, and the cases of cholera *pari passu* increase in number and severity. The haze becomes a dry fog, things communicate a gluey sensation to the touch, insects fly about in swarms, and the epidemic reaches its height. The barometer having reached its maximum, begins gradually to drop, the haze becomes more of the character of fog, and if it can be seen through cirri will be seen hovering in the higher regions of the air, or moving slowly northwards. The south current is now approaching, the barometer continues to fall, a gentle motion of the air is perceived from S.E. Ozone is detected; there may be rain; there may be a thunderstorm. The wind increases in power and ozone in quantity, and cholera disappears. The rationale of this medico-meteorological process is this: The first part of the process was the north current gaining the ascendancy; and, as it is the land current, bearing the products of decomposition, ozone is reduced to its minimum. While the air keeps in motion these products do not accumulate in great quantity. The barometer begins to drop. The north current falls back, and is succeeded by the calm. The products of putrefaction go on accumulating; there is no renewal of air, and sulphuretted hydrogen can now be detected. Cholera cases go on increasing as the poisonous substances accumulate. As the barometer decreases slowly, the south current slowly advances; but that it is advancing is shown by the cirri in the higher strata of the air. The air becomes more moist, because the moist current is approaching. The south or ozoniferous current at last gains the ascendancy, and cholera vanishes, because the incompletely oxidised bodies, the poisonous substances—perhaps hydrocarbons—are rendered innocuous by the ozonised air affording them oxygen. These views are supported by the facts that diarrhoea and choleraic diarrhoea are most common in the autumn months, and then ozone is at its minimum, and that ozone is invariably absent during cholera periods. Cholera is also observed to advance from east to west; so do the Polar current and the calm. I may state, also, in support of these views, that during the cholera epidemic at Newcastle, in 1853, the calm prevailed and ozone was at its minimum. From the 24th of August to the 11th of September, 1854, ozone was only once perceived, and then in a minimum quantity. Cholera was then at its height in London. On the 10th of September I wrote to a friend, stating that the S., or ozoniferous current was approaching, and requested him to watch its effects upon the epidemic. On the 11th we had a S. wind, with ozone, and from that day the number of cases of cholera diminished. In conclusion, I have to observe that in making ozone observations the test paper ought to be kept in the dark; that sulphuretted hydrogen, ammonia, and moisture cause loss of colour.

Dr. ANDREWS said that in order to determine the nature of the body said to be ozone, it should be ascertained that it possessed all the qualities of ozone. He had found many difficulties in his researches on the subject; but so far as his own opinion went, he was inclined to believe that what Dr. Moffatt spoke of was ozone. A very simple method of discovering it was to expose a piece of paper, moistened in a solution of the hydrate of potassium, to the atmosphere; if there was any ozone in the air the paper would soon become coloured. It was very interesting to expose these papers at different places for the same length of time, and the depth of discoloration would give the relative quantity of ozone in the air. He thought it was important to find out the causes of the destruction of ozone in the atmosphere, because it was quite clear that the power of destruction was as actively at work as the power of its production. Of all the tests of ozone, he thought the most delicate was that of metallic mercury.

Dr. R. ANGUS SMITH said he had occasionally made experiments with ozone, and he could confirm the observations of Dr. Moffatt as to its absence from large towns. He had never yet found that ozone approached Manchester within half-a-mile. The test which had been mentioned was a very valuable one. He did not mean to say that the effects were caused by the existence of ozone, but it was very important as an indication of the state of the atmosphere. He therefore hoped Dr. Moffatt would continue these observations, as he was sure he was on the right track. There had been several objections to these observations on ozone made by chemists, who contended that the indications were referable to the acidity of the atmosphere. From the absolute non-existence of these indications in Manchester, he could not refer them to acidity, because the rain of Manchester was nothing less than diluted vitriol, many hundred tons of vitriol falling upon our heads in Manchester in the course of a year. (Laughter.)

Mr. J. GLAISHER, F.R.S., said this was a question to which he had paid particular attention since 1848. He obtained test-papers from Dr. Moffatt, and he placed them at Greenwich Hospital, on the banks of the Thames; at the Greenwich Royal Observatory; and at Blackheath. The results were, that the papers were never coloured at the Hospital, whilst they were frequently coloured at the Observatory, and always coloured on Blackheath. From his investigations into ozone, he had learned to be careful in his deductions. He pursued his investigations at different hospitals, and placed the papers over several of the cholera patients, and when the papers were returned to him, he found several were deeply coloured, whilst others were uncoloured. He found, however, that where there was no colouration there was the greatest number of deaths. There was no indication of ozone whatever at St. Thomas's Hospital, or, in fact, near the Thames, except on occasions when the wind was moving there would be a slight tinge on the paper. His next discovery was that there was more ozone by the sea than inland,—about four times as much. In towns there were seldom any indications of ozone. As to height, ozone increased with elevation, or the purity of the air. Ozone was destroyed by the exhalation of animal matter in these districts. He agreed with Dr. Moffatt in all respects but one, and that was with regard to the barometer; for he had never yet been able to trace any connection between the variation of the barometer and the health of man. He agreed with Dr. Moffatt that a wind off the sea produced a maximum of ozone, but he did not think it was produced by any particular wind blowing

always in one direction. Taking Dr. Moffatt's researches altogether, they had been a great assistance to him (Mr. Glaisher). He was sure Dr. Moffatt was in the right path, and he hoped he would continue untiring in following out his present results.

Dr. DAUBENY said that the observation that there was a great deal more oxygen in the neighbourhood of the sea than in the interior of our island, was quite in accordance with the theory that the ozone was destroyed by animal and vegetable exhalations. He might take this opportunity to produce a specimen of fluor spar from Bavaria, which, if rubbed, appeared to contain ozone. He should like to know if it was a fact that there was an entire want of ozone on the top of the Alps.

Dr. MOFFATT said that, with regard to the barometer, he did not know that he could attribute disease either to the high pressure or low pressure of the atmosphere; but he believed it was the change from one pressure to the other that caused disease. That might be electric. Both the animal and vegetable kingdoms could adapt themselves to several inches of pressure, but it was the change from one pressure to the other which caused the disease,—whether it was electric or moisture he did not know.—In reply to a question as to what the test-papers were made of, Dr. Moffatt explained that they were made of iodide of potassium, starch, and water. It was difficult to make two packets alike, and the best way would be to purchase them of Negretti and Zambra.

TECHNICAL CHEMISTRY.

*On Bleaching,*¹ by Mr. HENRY ASHWORTH.

THE art of bleaching may be considered to have taken date from the earliest ages, probably from the time when human knowledge had become extended to the fabrication of clothing; and the inventor may have been the first housewife whose personal feelings induced the desire to enjoy the comeliness and the comfort of clean linen. Primarily, the operation has been accomplished by soap and water, the friction of the human hands, and exposure to the whitening effect of the atmosphere. Theoretically speaking, the bleaching of the present day, in all its enlarged proportions, is accomplished out of the same elements, although in their application they may have been altered or modified to suit the economy of modern chemistry, and the mechanical appliances which now supersede the manipulations of a by-gone age. The interesting character of the progress of these changes it will be our province to endeavour to trace. The ancient writers do not appear to have left us any details on the subject, and the knowledge we possess extends only as far back as the middle of the last century. Previous to that time, the cotton manufacture was scarcely known, and the linen manufacturers of the North, who knew nothing of bleaching, used to send their brown fabrics to be bleached in Holland, and they were received again in the course of eight months, and sold under the definition of brown Holland. In the year 1749, the practice of lime bleaching was introduced, and the cloth was then steeped in alkaline lye, which was called "bucking." The subsequent process of bleaching was done by exposure on the grass, which was called "crofting," and these operations were repeated five or six times, extending over a period of eight or ten

weeks. The first advance upon this tedious process was made by Dr. Home, of Edinburgh, who substituted a very weak solution of sulphuric acid for the buttermilk, the rye meal, and the bran operations previously employed, and by this means he accomplished the same result, and in a shorter time than by the buttermilk process, which had required two months. In the year 1787 an important change took place in consequence of the discovery of M. Scheele of Sweden, of a chemical compound, such as is now designated chlorine, and this was used as a substitute for exposure to the atmosphere. The repeated experiments of Berthollet added considerably to the facts already known; but the practical effects of these discoveries were more fully brought out by Mr. Watt and Dr. Henry, and it is to them that the chief merit is due of introducing the new mode of bleaching cotton goods. To those who are familiar with the history of the cotton manufacture, it will be apparent that bleaching, as a distinct pursuit, had by this time become a necessity of the trade, therefore the manufacturer consigned to the bleacher, who was originally called a whitster, his woven fabrics in the state in which they came from the loom, loaded as they were with impurities; and the following is described as the course of operations by which he undertook to whiten them. The loose fibres of cotton adhering to the surface of the cloth were removed by passing the whole length of the piece over a curved plate of iron heated red hot,—an operation that required to be done with great velocity to prevent any injury being sustained to the goods. The cloth was then deposited in a cistern of water, or in a weak alkaline lye, where it would remain, perhaps for some weeks, until the flour paste, used as dressing by the weaver, had become softened by fermentation. The removal of the paste and other matter contained in the web was accomplished by the agitating process of a dash wheel, or cylindrical box revolving on its axis, having four compartments or internal divisions radiating from the centre, in which the bundles of cloth were deposited. A supply of water was being injected at the sides of these compartments to carry off the impurities as they were liberated by the jolting concussions created at every revolution of the wheel. This invention was deemed a great advance upon the previous practice of dragging the pieces by hand through a stream of water in the water pits constructed for the purpose. The cloth was next boiled in lime water, and another operation of the dash wheel succeeded. The lime water had to be expressed from the cloth by passing the whole length of each piece betwixt a pair of revolving rollers called "squeezers." It was then immersed in a composition called "chemical," and when taken out was carefully washed in cold water. These operations, more or less varied in their character and arrangement, were repeated a number of times; the cloth had some weeks of exposure in the grass fields, with an occasional sprinkling of water to aid the process of whitening, and was then considered to have received a bleaching; the process of drying was in long sheds, constructed mostly of wood, having openings at the sides with sliding doors; and the pieces were suspended by hand upon lines of railing until they had become sufficiently dried by the atmosphere. When taken down, every piece was neatly folded and compressed by screw pressure, and this was deemed the finishing process before delivery at the warehouse of the manufacturer.

It is from such a beginning, and within living memory that we shall endeavour to trace the successive changes which, in the course of sixty or seventy years, have led

¹ Read before the British Association.

to the establishing of that important branch of trade, now so intimately connected with the cotton manufacture of this and other countries. The chemical preparations originally adopted were somewhat primitive: the acids were derived, to some extent, from an immersion of the cloth in sour milk, although the use of a weak solution of sulphuric acid was the more prevailing practice. The alkali which is the basis of soap was derived from the ashes of burnt fern, sea-weeds, or other similar products. The oxygen of the atmosphere was employed to whiten the grey fibre of the cloth, and a decoction of sorrel was used to remove the stains and iron moulds. About the year 1795, it had become obvious that the growing demands of commerce required a more efficient agency than could be derived from such rude elements. Severe pressure on the trade was felt from the enormous amount and cost of labour required, the heavy expense and the inferior quality of the drugs then in use, the rental paid by the bleacher for so large an extent of grass land as he found it necessary to occupy with the exposure of his cloth to the atmosphere, together with the unavoidable delay, uncertainty, and pecuniary losses, which were constantly occurring from unfavourable weather and other causes. The manufacturer also suffered greatly from the frequency of his disappointments, and the inconvenience and loss he sustained from the necessity of having his capital in goods lying three months in the bleach grounds. The emergency which had now become so apparent had the effect of bringing out inventions and improved processes. These succeeded one another with great rapidity, and those proprietors of works who possessed intelligence and capital made rapid progress; although there were other instances in which the want of capital or the absence of enterprise prevailed, and in these cases there remained a lingering adherence to those antiquated usages which were being superseded in other concerns. Every one knows that in whatever manner bleaching is done, it must be effected by means of true chemical agency; and therefore it was to the chemist that the bleacher had, in the first instance, to look for the discovery of more potent agents to effect his object.

The first great triumph in this department was the discovery, in 1798, by Mr. Tennant, of Glasgow, of a method of making saturated liquid chloride of lime,—an article which was found to answer quite perfectly all the purposes required by the bleacher. This invention, for which he took out a patent, consisted in the substitution of lime for potash. His patent right was resisted by the bleachers of Lancashire, and was set aside by the verdict of a jury. The grounds of this decision were, that the patent included a mode of "bucking" with quicklime and water, which was not a new invention; and because one part of the patent was not new, the whole of the claim must be set aside; and by this decision the use of liquid chloride of lime in bleaching was thrown open to all. It was deemed an unfortunate circumstance that, in consequence of an error of expression in describing his process, Mr. Tennant should be deprived of the fruits of a laborious investigation, extending over several years. There was a strong feeling of sympathy manifested, and the bleachers of Lancashire presented him with an expression of their grateful acknowledgment by a service of plate, which Mr. Tennant accepted; but it was most in accordance with the character of his original design to press onward with his discoveries, and to bring his first invention to a still more practical issue. He, therefore, adopted a new method,—one which at length he completed and

secured by patent, for impregnating quick lime in a dry state with chlorine, and in this instance his right remained uncontested. He lived to enjoy his pecuniary reward; and the manufactory which he established in Glasgow is at this time the largest in extent of any of the kind in Great Britain. The supply of alkali, which was being derived from the burning of fern, and from the incineration of sea-weed on the coasts of Scotland and Ireland, having become insufficient, it was supplemented by the importation of barilla, an article produced in the same way upon the coasts of Spain, Sicily, and Sardinia. The alkali procured by such means was very crude and very unreliable in its action, and the use of it was at length superseded by potash obtained from the clearing and burning of forests in the United States. At length the consumption of the article had become so greatly enlarged, and the price so enormously high, that the demand for the production of alkali became an object of mercantile attention. Chemical science stepped in, and very quickly responded to this appeal on the part of the bleachers. The consequence was, that an artificial soda was invented, called "soda ash," or British soda, which, in regard to cheapness and efficiency, has caused the bleachers to dispense with the use of potash; and the quality of the article has so successfully commended itself to the consumers, that the manufacturers are now annually exporting British soda of the value of one million sterling over and beyond the requirements of the bleachers, soap makers, and glass manufacturers of our own country. It is worthy of remark that the great bulk of our shipments of British soda are to the United States, from whence, only a short time ago, we were importing very large quantities of potash, the production of their forests. Thus, by an art which half-a-century ago was almost unknown, and by the agency of our coal as fuel, we have succeeded in converting certain products which we dig from under our feet, such as salt, pyrites, and lime, into one of the most important branches of manufacturing chemistry. These discoveries in chemistry may appear extraordinary, although they are not more important in the economy of bleaching than are the mechanical arrangements which have superseded the exposure of labourers, in all states of the weather, to the accustomed drudgery of the "crofters" of old. The "crofters," of whom we have spoken, bore the appearance of remarkably strong men; their working dress was of thick white flannel, called "gladding;" the cut of the coat was peculiar, having a loose, open appearance, and a low, flat collar, on which the shirt collar usually rested. They had their necks uncovered; and, their employment being so much exposed to moisture, they seldom wore stockings. Altogether, they assumed a bearing of unconcern about the state of the weather, and were quite regardless of the splashing of water. Their employment consisted mainly in the handling of wet cloth, and in removing it, either by hand or by wheelbarrows, from one operation to another. Perhaps the most distressing part of their labour was that of carrying upon their shoulders a pile of wet cloth, rising to some height above the head, which they conveyed to some considerable distance in the fields, and spread upon the grass. In the severity of the winter season there would be drippings from the cloth, forming icicles, which would be adhering to the skirts of their clothing. It has been through a succession of mechanical inventions that these laborious operations have been dispensed with, and, one after another, they have been handed over to the power of the steam-engine. The machinery of the dash-wheels are in a great measure

superseded; the use of wheelbarrows for the removal of cloth from one operation to another are not now required; the suspension of wet pieces upon wood railing in an open shed for the purpose of drying has passed away; all this work is now done under cover; the drying process is carried on in heated rooms; and the suspension of the cloth is accomplished by an ingenious application of machinery. Practically speaking, bleaching operations have been revolutionised; and probably the most efficient labour-saving invention has been a mechanical arrangement, or, as it is termed, a continuous system of cloth working. By this method, it is not now necessary to conduct the bleaching of individual pieces singly, one by one, but to have the ends of them sewed together by hundreds in a consecutive string, and have them drawn through a number of operations in their ordinary succession; the regularity of the work being attended to by boys, and the superintendence by men. It will be obvious that, by the various means referred to, the operation of bleaching has been much simplified, and the expense reduced; but, meanwhile there have been great changes, and eventually an increasing demand for what is usually termed "finish," which consists of a skilful application of calendering, pressing, rolling, and otherwise rendering a piece of plain cotton as near as possible in accordance with the design of the manufacturer or the exporter, who may undertake to gratify the taste of the consumers in the various markets of the world. To such an extent has this rivalry or competition by "finish" been carried, that the machinery, labour, skill, and cost of extraneous materials required, have occasioned an expenditure that is said to exceed all the other expenses previously incurred in the bleaching. Dyeing has also been associated with bleaching and finishing; and the class of goods which are embossed and figured by pressure now belong to this department, and afford another variety in the attractions of cotton fabrics. The result has been that the time required for the operation of bleaching is now about as many days as formerly it required weeks to accomplish. Honour to British genius that these advantages have been derived to our country.

The general public will no doubt feel curious to ascertain whether any and what proportion of the money saving thus effected has reached the consumer; some other portion of the public will inquire to what extent the advantages thus achieved by science and art have been shared by the operative class employed? It is not expected that much concern will be manifested about the interests of the proprietor, and it is not unreasonable to suppose that a still more minute inquiry will be raised about the "human machine," more especially, whether, during the progress of these advances in manufacturing art, the material, moral, and intellectual condition of the working class has been made to keep pace with all these improved manipulations, which, amidst the struggle of changes, have destroyed the character of many employments, but have greatly increased the whole number of persons employed? These inquiries it will be our endeavour to satisfy. The advantages shared by the consumer will easily be reckoned. We have before us a printed card, or list of prices for bleaching issued by a leading firm in the year 1803. At that time the charge for bleaching a well-known description of cloth was 7s. 6d. for a piece of 28 yards, and it is now 6d. The case of the labourers employed in bleaching 60 or 70 years ago was, as before stated, a very harassing one; they suffered severely from exposure to wet and cold, and, as a consequence, from rheumatism and asthma. The earnings of a "crofter" would be from 10s. to 15s.

per week. Upon wages so scanty, and with some uncertainty of employment, their mode of living was necessarily inexpensive. Oatmeal was the staple commodity of their food. They used it as porridge; their bread was of oatmeal, either in leavened oat cakes, or baked in the form of a loaf called jannock, which is said to have been introduced by the refugee Flemings; and animal food, with the exception of bacon, was seldom found at the working man's table. Now-a-days, the workmen in bleach works perform all their work indoors, and are, therefore, no longer exposed to the coldness and moisture of the former period. Their wages are increased in a proportion which cannot easily be estimated, and their employment is one of great regularity. They have nearly ceased to consume oatmeal; jannock is unheard of; oat cakes are seldom seen; and their tables are now spread with wheaten bread, animal food from the shambles, and all the other articles which usually enter into the consumption of families in the other grades in life. The social condition of the operative bleacher of early times cannot easily be separated from the rest of the working population of that day, neither could they now be described in any other manner than that which would apply to the operatives around them in other pursuits. We may refer to their mode of pleasure-taking as affording in itself a very appreciable indication of the past and present. The amusements which formerly prevailed were rude and boisterous; now they are more refined and intellectual. Bull-baiting, bear-baiting, and cock-fighting were amongst the common amusements of the day, especially at the wakes and fairs. The game of foot-ball was a very favourite one, so much so that the people of one place would make selection of their combatants and have them pitched against those of some other place, and these would contend in very ardent strife for the renown of mastery. Indeed, so very popular was this game, that a match at foot-ball was, upon one day in the year, tolerated by the inhabitants in the streets of Bolton. The whole of this is now given up. The game of cricket is becoming a popular one, and others equally harmless in their character are being introduced. Their manners have been designated by the historian as "frank and primitive." Therefore, upon such a foundation of industry as that already described, and with elements of character admitted to have been generous and progressive, it will not be matter of surprise to discover that at the present day a commendable position has been attained by this class. Provident Institutions are becoming numerous, and upon holiday occasions the members are fond of the imposing effect of a display of their numbers and respectability. The parade of their flags and other official insignia sometimes disclose a designation of the body which, amongst a manufacturing class, may appear somewhat grotesque, such as Druids, Odd Fellows, Gardeners, Foresters, and the like. There are a great many depositors in the savings-banks; the co-operative societies have absorbed no small amount of their money savings, and the more intelligent have been engaged in the conducting of mercantile pursuits of these concerns. Perhaps there is no other illustration of the spirit of progress so comprehensive or so pregnant with results of a promising nature as the following:—Manchester, which is the industrial centre of the cotton manufacture, is also the centre of 127 Mechanics' Institutions, associated together as the Lancashire and Cheshire Union. The inquiring mind, when put in motion and stimulated by example, is sure to take a direction resulting in intelligence. Hence, among other means, they have

recourse to cheap trips by railway to some distant part of the country. The Lancashire operative does not fail to see that those who are but little above him in station have their autumnal excursions. He works hard, and why should he not shake off, for some short period at least, the burden of wear and tear of body and mind, and transport himself, with other members of his family and friends, over some other portion of the earth's surface which he has not seen, and share the healthful recreations and the pleasures attainable in modern life? Moreover, by an excursion he falls into the way of new scenery, new faces, strange remarks, historical associations, and observations of other trading pursuits to which he has not been accustomed, all tending to enrich the mind, gratify the senses, and create new impressions of delight. In this way the traveller, whose mind is open to profit by what he sees, returns home instructed, and at an expense so slight as to make no conceivable inroad upon his savings fund.

Seventy years ago Sunday-schools had made but slight progress. There were but few persons who could read, still fewer who could write; and when anyone received a letter, he had to carry it away, perhaps a good many miles, to find a scholar who could read it. At the present time, Sunday-schools abound, day-schools are numerous, and the affair of carrying away a letter in search of a scholar may now, with much complacency, be put down as among the reminiscences of seventy years ago.

On the Action of Water on Lead,¹ by Dr. F. CRACE CALVERT, F.R.S.

I WOULD not have troubled this Section with bringing before them a subject which has been noticed by them several times already, if these researches (which were made in connection with a Select Committee of the Sanitary Association of this city) had not presented some interest on several points,—namely, the action on lead of a water supplied to a large population; the determination of the quantity of lead that the same water took up in a given length of time; and, further, with the hope of removing from the minds of some chemists the impression that soft surface water does not act upon lead.

These experiments have extended over a period of more than twelve months, and above 300 samples of water have been tested, and led to the following conclusions:—

First, that the water supplied to Manchester, and passing through a lead pipe used as a supply-pipe, was for four days highly charged with lead compounds, and that the quantity gradually decreased for six weeks, when the water ceased to absorb lead. But if, during that period, the water were allowed to remain in contact with the pipe for twelve hours, its action was still very marked, and it may be stated that, at the end of six weeks, the water still contained $\frac{1}{10}$ th of a grain of lead per gallon, and after three months $\frac{1}{30}$ th of a grain was found.

Secondly, experiments were made on five samples of piping of the following description:—No. 1, extra tinned lead piping; No. 2, ordinary tinned lead piping; No. 3, best or virgin lead piping; No. 4, common lead piping; No. 5, block tin piping. The water was left in contact with these pipes for twelve hours, and, with the exception of the one last mentioned, all were acted on, the

proportion of lead taken up by the water from the lead pipes amounting to from $\frac{1}{10}$ th to $\frac{1}{30}$ ths of a grain per gallon. Waters containing these quantities of lead per gallon are quite sufficiently charged with that compound to prove injurious to health if used as a beverage. I may substantiate this statement by the fact that four samples of water, which I have lately examined as having injured the health of the inhabitants of this city, contained from $\frac{1}{10}$ th to $\frac{1}{30}$ ths of a grain of lead per gallon. Therefore, there can remain no doubt that the water of this city may become sufficiently charged with lead to injure the health of the inhabitants.

Dr. Crace Calvert concluded his remarks by stating that, on allowing water to remain for some time in the pipes, and then again bringing them into constant use, the action of the water on the pipes is greatly increased.

In the discussion which followed,

Dr. SMITH, of Sydney, said, from experiments he had made at Sydney, he found that the action of water upon pipes made of lead and tin was much greater than its action on lead alone.

A Gentleman said that he had found that the water of Sheffield, which came from a similar source as that of Manchester, was strongly contaminated by passing through lead pipes, being to the extent of $\frac{1}{10}$ ths to $\frac{1}{30}$ ths of a grain per gallon.

Dr. ANGUS SMITH related several instances of serious poisonings, and even deaths, through drinking Manchester water. Though the Glasgow water was said not to be injurious to health, he believed as little as $\frac{1}{10}$ th of a grain of lead per gallon would affect many persons, whilst others might not be affected by $\frac{1}{10}$ th.

Dr. MACADAM said the water of Edinburgh was impregnated with lead only to the extent of $\frac{1}{10}$ th of a grain per gallon.

Dr. WALLACE said that he believed the cause of the action of the water upon the lead was oxygen and nitric acid. Lime also exercised a powerful action in permitting the action of the water upon the lead. He had heard of no poisonings in Glasgow by the water.

On the History of the Alkali Manufacture,¹ by Mr. GOSSAGE.

IT was correct, he believed, that the manufacture of soda in Great Britain, by the special decomposition of common salt, had its commencement in Lancashire; at any rate, its largest development had taken place in this country. Previously to the establishment of the French republic, in 1793, soda was obtained almost entirely from the ashes of marine plants growing at Alicante, in Spain, Sicily, Teneriffe, and on the coast of Great Britain. Large quantities of potash were also imported from Russia and America, but now soda was exported to those countries which formerly sent us potash. The importation into France of alkali being stopped by the French Revolution, a committee was appointed by the French Convention to discover means of supplying the article from France alone. The process suggested by Le Blanc was approved of, but it was erroneous to suppose that his process was not invented before the committee was appointed. Having given an account of Le Blanc's invention, Mr. Gossage said that it was very complete, and was the same as now used in both England and France. This invention had done more to promote civilisation than any other chemical manufacture. The

¹ Read before the British Association.

¹ Read before the British Association.

poor inventor, however, met with the too common reward of talent, and after great privations died in an asylum for paupers. Sundry alkali works were erected in France, but the process was not introduced into England for some years afterwards. In 1787 Messrs. Gordon, Barrow and Co., of Aberdeen, applied chlorine, then recently discovered, to the process of bleaching. A large establishment was, in the following year, established at Bolton. At first chlorine was used in the state of solution in water, but the inconvenience of using it in that manner was overcome by the addition of potash to the water. The next step was to substitute lime for potash, producing solution of chloride of lime. This was the invention of Mr. Charles Tennant, of St. Rollox, who afterwards manufactured chloride of lime in the state of powder. This manufacture was carried out to a great extent. A great obstruction to the manufacture, however, was the high excise duty on salt, which operated most injuriously. When Mr. Tennant's patent for manufacturing bleaching powder expired, other parties began the same manufacture. Attention was directed to the utilisation of the mixed sulphate of soda and sulphate of manganese resulting from this manufacture, and carbonate of soda, in crystals was gradually introduced into the market. During the same period, Mr. Losh was making crystals of soda, and might be considered the father of the soda trade in this country. Mr. Losh finished his education on the continent, where he learnt Le Blanc's processes. After his return, he obtained permission of Government to work a spring of weak brine discovered at Walker, on the Tyne, for the manufacture of soda. He there manufactured soda crystals; but notwithstanding these essays, 1833 might be considered the natal year of the soda trade as a special manufacture in Great Britain. In that year common salt being relieved from fiscal impost, Mr. James Muspratt commenced the manufacture of sulphate of soda at Liverpool, to be used for the manufacture of carbonate of soda. Mr. Muspratt adapted Le Blanc's processes in their entirety. He had to contend with many difficulties, but he overcame them all, and reaped a satisfactory reward. Other manufacturers also commenced to make sulphate of soda, by the special decomposition of common salt, for the purpose of making soda; and it had since been found advantageous to adapt this method of working to the production of bleaching powder, by using the hydrochloric acid so obtained to generate chlorine by its action on manganese. In the early days of the soda trade no attempt was made to condense and liberate hydrochloric acid gas. The old apparatus of cylinders and Wolfe's bottles were totally inadequate for the condensing. Many plans were suggested, and amongst others he (Mr. Gossage) obtained a patent in 1836. Having demonstrated the practicability of effecting a complete condensation of hydrochloric acid by the erection and working of a set of apparatus at the soda works with which he was then connected, he introduced the plan to the trade, and it had been subsequently adopted by every manufacturer. The principle of the invention consisted in causing the acid gas to percolate through a deep bed of coke, in small lumps contained in a high tower, at the same time that a supply of water flowed very slowly over the surfaces of the pieces of coke. By this means, an almost unlimited extent of moistened surfaces might be presented to the gas for effecting its absorption, and as the same fluid descended through the tower, it met with more gas and gradually became charged to saturation; whilst, at the upper portion of the tower, any gas

which might otherwise escape was exposed to the absorbing power of unacidulated water. In 1838, a French house, Messrs. Fai and Co., of Marseilles, obtained a monopoly from the King of Sicily for the export of sulphur. This caused an advance in price to 14*l.* per ton from the previous rate of 5*l.* per ton. It was soon found that in our Cornish mines and in those of Wicklow, in Ireland, we possessed an inexhaustible supply of sulphur in the form of pyrites; our practical chemists soon availed themselves of this source, for the manufacture of sulphuric acid. In working with pyrites it was found that this mineral contained sulphide of copper, as well as sulphide of iron, and, at an early period, he commenced to extract the copper from the burned residuum by smelting. At the present time, the products obtained by the soda manufacturers were soda ash, worth 3*l.* per ton; soda crystals, about 4*l.* 10*s.* per ton; bleaching powder, 9*l.* per ton; bicarbonate of soda, 10*l.* per ton; whilst the cost of raw materials, now used in Lancashire, are sulphur, 8*l.* per ton, for which was substituted pyrites at a cost equivalent to 5*l.* per ton; sulphur salt 8*s.*; zincstone, 6*s.* 8*d.*; fuel 6*s.* per ton. Thus, with a reduction in the cost of raw materials not more than equal to 10 per cent., the public was supplied with the products of the soda manufacturer at a reduction of at least 60 per cent. As nearly as he could obtain information, there were 50 establishments in Great Britain, in which soda was manufactured by Le Blanc's process, producing about 3000 tons of soda ash, 2000 tons of soda crystal, 250 tons of bicarbonate of soda, and 400 tons of bleaching powder per week. The total amount of these products might be estimated as exceeding two millions sterling, which was so much entirely added to the annual income of the country, excepting about one hundred thousand pounds paid for materials obtained from other countries. He must not omit to notice the prospect of a new market for British-made soda which had been opened by the successful labours of Mr. Cobden, in negotiating the commercial treaty with the French Government. Many attempts had been made to supersede Le Blanc's process, by some more direct means of operating on salt, so as to eliminate its soda at once. Up to the present time, the result of all these attempts had been the wasteful expenditure of large sums of money. Two-fifths of the total cost for raw materials was incurred for pyrites, from which to produce a supply of sulphur; and it was a well-known fact that more than nine-tenths of this sulphur was retained in the material called "alkali waste," which was thrown away by the manufacturer. Thus was presented a problem which, if it could be solved, would effect a large reduction in the cost of soda. Many chemists, both scientific and practical, had given a great amount of attention to that subject. He had been so unfortunate as to be amongst the number, as he had devoted a great proportion of his time, during a quarter of a century, and a large amount of both money and labour to this hitherto delusive subject. He commenced by demonstrating in 1838, that one equivalent of sulphide of calcium produced monocarbonate of lime and sulphide of hydrogen. This decomposition was contrary to the received views of scientific chemists of that day, as it was held that an excess of carbonic acid was needful to effect the perfect decomposition of sulphides. He was convinced that whenever the utilisation of the sulphur in alkali waste might be effected, it would be by means of this action of carbonic acid. He demonstrated also, at the same time, that one equivalent of carbonic acid would decompose one equivalent of sulphide of sodium, producing monocarbonate of soda

and sulphide of hydrogen. His present impression was, that Le Blanc's process would be modified by the omission of lime, when decomposing sulphate of soda, thus producing sulphide of sodium; and that the carbonic acid evolved by this decomposition would be applied to decompose the sulphide of sodium producing carbonate of soda, and eliminating sulphide of hydrogen, which would be absorbed by peroxide of iron, and the product in the manufacture of sulphuric acid. He had proved the practicability of all those decompositions and actions; but the ideas had still to be worked into a practical operation.

NOTICES OF BOOKS.

A Manual of Chemistry, Descriptive and Theoretical. Part I. By WILLIAM ODLING, M.B., F.R.S., Fellow of the Royal College of Physicians; Secretary to the Chemical Society; and Professor of Practical Chemistry at Guy's Hospital. London: Longman, Green, Longman, and Roberts. 1861.

THE first part of this important and long-expected Manual is announced for publication on October 1st. We propose, in an early number, to devote some space to a critical review of its contents, and, in the meantime, will briefly draw attention to some of the leading features in the book, in most of which it will be found to differ from other chemical manuals of similar scope:—

As a rule, the atomic weights selected for volatile elements represent single volumes, and those for volatile compounds double volumes, of their respective gases or vapours.

The great majority of compound bodies are expressed, as unitary molecules, by unitary formulæ, instead of by additive, or, as they are commonly called, rational formulæ. Throughout, the algebraic sign of addition is never used to express combination.

The equivalent notation, by means of dashes, introduced by the author some years ago, and now constantly used in the original papers of most European chemists, is here employed for the purposes of elementary teaching.

The arrangement of the book is more than ordinarily systematic. The mutual relations of the elements and of their analogous compounds are largely dwelt upon, as are also the mutual relations of the various heterologous compounds of the same element. The properties of classes of bodies, chlorides, oxides, sulphates, &c., are described with greater fulness than is customary in text-books.

The compounds of mineral and organic chemistry are not considered apart in separate sections. Moreover, the doctrines of series, types, and substitutions, are applied indiscriminately to both branches of chemical science.

The technological applications of chemistry are but very briefly referred to, and the physics of chemistry only incidentally discussed; so that a more than usual proportion of space is devoted to chemistry proper, and especially to the description of chemical reactions.

The present part contains an introductory section, and an account of the elements, hydrogen, fluorine, chlorine, bromine, iodine, oxygen, sulphur, selenium, tellurium, nitrogen, phosphorus, arsenic, antimony, and bismuth, and of their combinations with one another.

NOTICES OF PATENTS.

330. *Treatment of Soda Waste and Sulphurets.* J. L. JULLIOT, Tynemouth. Dated February 9, 1861.

By different modes of treatment the inventor proposes to economise the oxy-sulphide of calcium refuse of the alkali

manufacture, either by eliminating from it the sulphur in the free state, or by effecting its further oxidation, and so giving rise to the production of marketable compounds, having one of the alkaline earths acting the part of base.

Immense quantities of hyposulphite of soda are now being manufactured from this oxy-sulphide of calcium refuse, and in this instance the inventor appears to have in view the substitution of the cheaper bases, lime or magnesia, for the soda commonly employed. The expedient of burning out the sulphur from the waste is also contemplated in the specification.

331. *Capsules or Covers for the Necks of Bottles and Jars.* J. G. JENNINGS, Holland Street, Blackfriars. Dated February 9, 1861.

THIS invention consists in employing suitable covers, metallic or otherwise, to which a short length of vulcanised india-rubber tubing is securely attached, so as to admit of being readily turned back, and thus allow of the capsule or cover being put on to a jar or bottle with great facility, and by turning down the piece of india-rubber tubing it is made instantly to embrace that part of the jar, bottle, or aperture to which the capsule-cover has been applied. The binding or grip of the india-rubber tube will be tight or slack upon the neck of the bottle according as the interior diameter of the elastic tube is less than that part of the jar or bottle to which it is applied and may be regulated also by the thickness of the rubber employed.

The principle of this invention is highly ingenious and susceptible of very wide application. That the junction may easily in this manner be made perfectly air-tight is attested by the general practice of connecting the joints and parts of laboratory gas apparatus by short lengths of vulcanised rubber tubing. Capsule lids formed entirely of india-rubber have been found to answer well for closing the mouths of jars and wide-necked bottles. It must be borne in mind that metallic specimens and many chemical products are liable to become quickly tarnished by exposure to the sulphuretted volatile emanations which continually escape from ordinary vulcanised india-rubber; in such cases the employment of the new capsule covers would necessarily be inappropriate.

335. *The Manufacture of Sub- or Oxy-Sulphate of Lead.* A. LEIDEMANN and T. LANGR, Newcastle-upon-Tyne.

A Communication. Dated February 9, 1861. For the purposes of a white lead paint the patentees claim the production and use of white precipitates containing sulphate of lead in combination with different proportions of oxide of lead.

Provided that the pigments formed in this manner be found to possess the requisite degree of body or opacity, this treatment would very likely be found serviceable in the working up of the precipitated sulphate of lead which is obtained as a bye-product in the process of making acetate of alumina mordant from alum and sugar of lead.

337. *Artificial Leather.* E. GERVAISE and J. E. BERNIER. Dated February 11, 1861. (Not proceeded with.)

THE mode of manufacturing artificial leather is as follows:—The inventors propose to employ caoutchouc, gutta-percha, or other gums, fusible materials and substances. These are to be submitted to a sufficient degree of heat to liquify them, after which they are mixed either with cow-hair, wool, or vegetable fibres, and when desirable with waste pieces of leather or cuttings of skins. Pitch, tar, and certain oils may be substituted for the gutta-percha and india-rubber. When required to possess the greatest degree of strength the artificial leather should be subjected to the action of super-heated steam during the operation of incorporating the materials; and after perfect combination has been effected in this manner, the mass,

whilst in a warm state, is passed between pressure rollers to agglutinate the fibres and give it the required thickness, after which the material undergoes a final operation for imparting to it the appearance of natural leather. To effect this, the surface has to be coated with a varnish composed of solution of india-rubber or gutta-percha, gums, or drying oils, and is again passed between pressure rollers.

The manufacture thus described is very similar in its main features to that followed in the preparation of "American cloth," and also to that of "Clark's patent waterproof felt," which latter appears to be composed of cotton-wool matted together with gutta-percha, applied in a coal-tar naphtha medium as a paste or solution in order to facilitate the incorporation of these ingredients. The success of such a process as that defined in the specification will mainly depend upon the careful selection of the raw materials, both gums and fibres, employed in the manufacture.

358. *Manufacturing a Glutinous or Viscous Substance to be Used in Dressing Textile Fabrics, and for other such like Processes, or in Brewing and Distilling.* W. MALTBY, Camberwell, Surrey. Dated February 12, 1861.

In carrying out this invention the patentee takes ground rice and mixes it with about one-third of its weight of water, the temperature of which he prefers to be moderately warm. He now leaves it to stand for several days, with occasional stirring, until fermentation has commenced; then dries it with the assistance of artificial heat, which he regulates accordingly as the product is required to be more or less glutinous. The dry material resulting is ground to powder, and after mixing with hot water is ready for use.

The description above given, refers precisely to the ordinary method of preparing dextrin by fermentation. A process in common use consists in allowing to ferment starch, either in the form of damaged wheaten flour or ground rice, whichever may happen to be cheapest in the market at the time, simply mixed with water, and then left until fit for use, without resorting to any intermediate drying process.

365. *Refining Malt Liquors.* C. S. ROSKILLY, Falmouth. Dated February 13, 1861.

The patentee employs a mixture of malt, hops, the leaves of ground ivy, and nettles, boiled in water, and when cold he adds a small quantity of the decoction to the liquor to be refined.

The mucilaginous constituents of the two plants last named, would probably operate like gelatine in causing the more rapid precipitation or subsidence of the suspended impurities always present in malt liquors before refining.

Grants of Provisional Protection for Six Months.

1705. Marc Antoine Francois Mennons, Rue de l'Echiquier, Paris, "Improvements in the construction and arrangement of apparatus for the manufacture of ice, and for the refrigeration of solid and fluid masses in general."—A communication from Edouard Blée, Rue d'Amboise, Paris.—Petition recorded July 4, 1861.

1845. Nicholas Etienne Dumesnil, Rue aux Loups, Sotteville-les-Rouens, France, "Improvements in materials for lubricating parts of machinery and for other lubricating purposes."—Petition recorded July 23, 1861.

2029. Stephen Carey and William Morgan Pierce, Animal Charcoal Works, East Ham, Essex, "Improvements in apparatus for reburning animal charcoal."—Petition recorded August 15, 1861.

2067. Richard Archibald Brooman, Fleet Street, London, "An improvement in preserving meat and other animal substances."—A communication from Jeanne Pierre Liès-Bodard, Strasbourg, France.

2077. George Frederick Muntz, French Hall, Birmingham, "Improvements in apparatus for melting metals."—Petitions recorded August 20, 1861.

2083. William Clark, Chancery Lane, London, "Improvements in optical and illuminating apparatus."—A communication from William Ferdinand Count de Douchet, Boulevard St. Martin, Paris.

2103. Thomas Richardson, Newcastle-upon-Tyne, and Robert Irvine, Hurler, Renfrewshire, "Improvements in the manufacture of paper."—Petition recorded August 22, 1861.

2129. William Edward Newton, Chancery Lane, London, "Improved machinery for filtering liquids, decolourising saccharine and other juices, and rectifying alcoholic liquors."—A communication from Horatio Nelson Fryatt, Belleville, New Jersey, U.S.—Petitions recorded August 26, 1861.

Notices to Proceed.

1084. Robert Laing, Ince, near Wigan, and Israel Swindels, Wigan, Lancashire, "Improvements in the treatment of certain ores containing metals, and in obtaining products therefrom."

1109. Marc Antoine Francois Mennons, Rue de l'Echiquier, Paris, "The manufacture of paper and cardboard from a fibrous vegetable matter not hitherto used for that purpose."—A communication from Edmond Nuewens, Montreuil-sous-Bois, France.

1115. James Alexander Manning, Inner Temple, London, "A mode or method of collecting ammonia from the waste gases arising from the combustion of coal."

1131. Joseph Victor Vignon, Dean Street, Soho Square, London, "A mode of preparing or making enamel applicable to various purposes."

1217. William Clark, Chancery Lane, London, "The treatment of ammoniacal waters resulting from putrified urines, and from products extracted by the distillation of coal."—A communication from Messrs. Louis Joseph Frédéric Margueritte and Alfred Lalouët de Sourdeval, Paris.

1223. William Clark, Chancery Lane, London, "Improvements in the manufacture of steel."—A communication from Messrs. Louis Joseph Frédéric Margueritte and Alfred Lalouët de Sourdeval, Boulevard St. Martin, Paris.

1705. Marc Antoine Francois Mennons, Rue de l'Echiquier, Paris, "Improvements in the construction and arrangement of apparatus for the manufacture of ice, and for the refrigeration of solid and fluid masses in general."—A communication from Edouard Blée, Rue d'Amboise, Paris.—Petition recorded July 4, 1861.

1800. Sir William O'Shaughnessy Brooke, Westminster, "Improvements in apparatus for suspending and insulating electric telegraph wires."—Petition recorded July 17, 1861.

2029. Stephen Carey and William Morgan Pierce, Animal Charcoal Works, East Ham, Essex, "Improvements in apparatus for reburning animal charcoal."—Petition recorded August 15, 1861.

CORRESPONDENCE.

Phosphate of Sesquioxide of Iron.

To the Editor of the CHEMICAL NEWS.

SIR,—Since attention is directed in your columns of September 21, to the manufacture of phosphate of iron, and of the probable use to medicine which will follow its production, I thought it would be well to inform your readers that the phosphate of sesquioxide of iron was made for use in medicine by my friend Mr. John Lightfoot, chemist, of Broad Oak Print Works, Accrington, more than two years ago, and that its use was attended with success. Soon afterwards the above-named gentle

man made a phosphate corresponding to the magnetic oxide, which is very freely soluble in citric acid and the neutral citrate of ammonia. This phosphate of protosequeioxide of iron has been used ever since by a surgeon in that town with marked success, in cases of albuminous and saccharine urine, and some forms of heart disease, and I have been told that Dr. Simpson, of Edinburgh, and several other medical gentlemen are in the habit of prescribing it.—I am, &c. W. R. BOOTH.

Chemical Notices from Foreign Sources.

TECHNICAL CHEMISTRY.

Processes for the Consecutive Preparation of Saltpetre, Potassio-tartrate of Soda, a pure Bitartrate of Potash, Tartaric Acid, and the Sulphates of Soda and Potash.—Guido Schnitzer gives (*Gewerbeblatt aus Württemberg*, 1861, No. 15) the following as an easy and convenient way of obtaining the above salts. He starts with equivalent weights of Chili saltpetre (nitrate of soda) and rough pot ashes which he heats, with just enough water to dissolve them, in an iron vessel, stirring well all the time. When the mutual decomposition which ensues is complete, he adds to the boiling liquid sufficient milk of lime to convert the carbonate into caustic soda. The solution containing the nitrate of potash and soda is then evaporated for the nitrate of potash to crystallise out, which the author says it does completely. The crystals are then drained from the soda solution, and washed with water slightly acidulated with hydrochloric acid, to remove the last traces of soda.

The soda solution is then used for the preparation of the double tartrate of potash and soda. For this purpose it is boiled with sufficient rough tartar to make a neutral solution. This operation is best conducted in a copper vessel. A deposit of some tartrate of lime from the rough tartar remains, which may be carefully washed and used in the subsequent process for making tartaric acid. When filtered from this deposit, the solution is evaporated for crystals of the potassio-tartrate.

When the author wishes to prepare pure bitartrate of potash, he takes the filtered solution of the double tartrate as above, and adds sufficient hydrochloric acid to combine with the soda, whereupon the bitartrate of potash separates in small granular crystals, which are drained and washed with cold water to remove any chloride of sodium.

To make pure tartaric acid, the author takes the solution of the double tartrate as before, and boils it with gypsum free from iron, whereby tartrate of lime and the sulphates of potash and soda are formed. The tartrate of lime is decomposed by sulphuric acid in the usual manner, and then (any iron having been removed by the alkali) the tartaric acid is obtained quite pure in the first crystallization. The sulphates of soda and potash are afterwards separated by crystallization.

Preparation for Gilding Porcelain.—This preparation, the invention of the Brothers Duterte, is reported on by Salvétat (*Bulletin de la Société d'Encouragement*, &c. March, 1861, p. 129). The peculiar advantage of it is, that after burning, the gold is so bright as not to require polishing. Thirty-two grammes of gold are gently warmed with 128 grammes of nitric acid, and the same weight of hydrochloric acid. To the solution are added 1.2 grammes of tin and 1.2 grammes of butter of antimony, and when all are dissolved, the solution is diluted with 500 grammes of water.

A mixture is now prepared by heating together 80 grammes of oil of turpentine, 16 grammes of sulphur, and 16 grammes of Venice turpentine. When the sulphur is dissolved, 50 grammes of oil of lavender is added. The gold solution is now added, and the two are well stirred together until the aqueous solution becomes decolorised,

showing that all the gold has united with the balsam. The watery portion is then poured away, and the oily fluid is washed with warm water, and then heated. When the last trace of moisture has disappeared, 65 grammes more lavender oil, and 100 grammes of oil of turpentine, are added, and the whole warmed to ensure the perfect admixture. While quite fluid, 5 grammes of subnitrate of bismuth are added. Afterwards, the clear part is decanted from any reduced gold and other insoluble matters, and the balsam is concentrated to a fit consistence for painting with. The balsam so prepared is a thick fluid of a pale green colour, the gold being perfectly dissolved; when proper care is taken to remove all moisture this preparation never blisters in burning.

MISCELLANEOUS.

The Polytechnic Institution.—We have already announced that Mr. Pepper has returned to the Direction of the Polytechnic Institution under the most favourable auspices. In his inaugural address he was greeted with a hearty welcome; he stated that although three years had intervened since he last appeared on that platform, the warmth of his welcome made him feel that the separation which had taken place between him and the Polytechnic audience was but then of yesterday. It made him feel that he was among friends, and he would appeal to them to aid him in the arduous duties he had undertaken. He said arduous duties, because when amusement, in its most attractive form, but totally devoid of rational instruction, was offered in other places to the public, it required no small degree of energy to manage an educational establishment in such a manner as to counteract the evil influences of the establishments to which he alluded. He would especially appeal to them to aid him, as they had aided his predecessor, Mr. Phené, by sending works of art and art manufacture to the institution for exhibition, and so increase its attractions. It would no doubt be interesting to the public to know that the Rev. J. B. Owen was chairman of the board of direction, and that the Rev. Chas. Mackenzie had taken charge of the educational classes. We understand that it is the intention of the directors of the Institution to issue honorary admission tickets, transferable, to all the eminent scientific men of the day.

ANSWERS TO CORRESPONDENTS.

* * All Editorial Communications are to be addressed to the EDITOR; and Advertisements and Business Communications to the PUBLISHER at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

* * In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

The Water Supply of London.—Mr. John Frederic Bateman writes to correct the announcement made by Mr. Fairbairn in his address to the British Association respecting a scheme for supplying the metropolis with water from Wales, in which he is represented as having estimated the cost of bringing water to London from the Bagshot Sands at £12,000,000. This is altogether a mistake, arising mainly from a typographical error in a return to an order of the House of Lords in 1852. The scheme which Mr. Bateman laid before Lord Shaftesbury in that year, and which was included in the return referred to, was one for bringing the soft waters—not of the Bagshot Sands—but of the Green Sands in the neighbourhood of Hindhead and the Leith Hills, and this estimate was £1,200,000, not £12,000,000, as erroneously printed in the Parliamentary return. In a subsequent part of the same return, p. 36, the estimate is correctly given.

W. P. W.—We are not aware that any special experiments have been made with the object of producing alums, in which sulphates of any of the organic alkaloids are caused to replace sulphate of alumina.

Omega.—A full account of the spectroscopy will be found in No. 149 of the *Photographic News*, for July 12, 1861, or it will be forwarded on application to Spencer, Browning, and Co., 111, Minories.

Curmione of Indigo.—A correspondent wishes to know the best way of preparing the above.

THE CHEMICAL NEWS.

VOL. IV. No. 96.—October 5, 1861.

THE PRESERVATION OF BUILDING STONE.

THE following Report has been addressed to the Right Hon. William Cowper, M.P., Chief Commissioner of Works, &c. We propose next week to give an analysis of the different projects for the preservation of stone which have been brought before the notice of the Committee.

We, the undersigned, being the Committee appointed "to inquire into the decay of the stone of the New Palace of Westminster, and into the best means of preserving the stone from further injury," have the honour to submit to you the following Report, in which we have adopted as the objects of our inquiry the several points referred to us by your letter of appointment and instruction, viz. :—

"I. The extent and position of the decay.

"II. The causes to which it is attributable, taking into consideration the composition of the stone, and the influence exerted upon it by moisture, and by the acids diffused in the London atmosphere.

"III. The best means of preserving the stone from further injury.

"IV. The qualities of the stones to be recommended for future use in public buildings to be erected in London."

1. In proceeding with the important inquiry thus intrusted to this Committee, we beg to state, that we first made a careful inspection of the whole of the buildings; and that after this inspection we proceeded to obtain such evidence as appeared to us best calculated to facilitate the inquiry intrusted to us, by examining a considerable number of witnesses who had been connected with the building from the commencement, or who had been concerned in the various processes which had been actually tried for arresting the decay which had occurred; and also another class of witnesses, who had suggested various theoretical remedies for the same purpose.

2. We delegated to a Sub-Committee, specially appointed, an examination and inquiry into the condition of other buildings erected in the metropolis, in which magnesian limestone had been used, and we particularly called the attention of the scientific chemists who had been appointed on the Committee to several points peculiarly within the limits of their acquaintance with chemical subjects.

3. We also considered it expedient to invite by advertisement the attention of chemists and others to the subject submitted to the Committee, requesting that any plan or suggestion for the prevention of decay, or for arresting its progress, might be brought under our notice.

4. Having thus premised the course the Committee thought it expedient and desirable to take, we now proceed to report *seriatim* on the subjects brought under our notice by your instructions, and in the order adopted therein.

I. *The Extent and Position of the Decay.*

5. It is extremely difficult to give any very exact account either of the extent or actual position of the decay. It seems from the evidence that it first began to make its appearance in the portions of the Palace at Westminster executed at the commencement of the building, about seven years after their execution; and yet, in some of the most recently executed portions, viz., those towards Old Palace Yard, facing Henry the Seventh's Chapel, the decay appears to be as obvious as in any other part of the building.

6. In the earlier works, viz., those towards the Thames, the decay is most apparent in the lower portion of the building, and in this portion the decay is confined to what may be called "zones," or general levels, which would seem to suggest that it depends as much upon position in the building as upon the use of particular beds of stone from the quarries employed.

7. The same remark applies to the part of the Palace fronting the approaches to Westminster Bridge, where the decay of the lower portion is considerable; but in the newest work, facing Henry the Seventh's Chapel, the decay occurs in positions which are more varied, and under circumstances which it is exceedingly difficult to appreciate.

8. We have examined with much care the upper portions of the building, and we cannot perceive that the decay has made any important inroad upon those much more exposed portions, where decay might more reasonably have been expected. The decay, however, occurs again to a considerable extent in the inner courts, which are sheltered in a great measure from external influences; and perhaps the very worst specimen we have noticed is to be found in the small archway leading to the reporter's gallery, near the entrance to Westminster Hall—a part of the work as much sheltered as in the nature and circumstances of a public building it could well be.

9. The general result of our observations, confirmed by the evidence, would seem to suggest that the stone used in the Palace of Westminster is much more likely to decay in damp and sheltered situations than where it is exposed to the full action of atmospheric influences. In the east and north fronts, before adverted to, the worst symptoms occur in the ashlar between the upper and lower mouldings of the plinth, and under the first cornice, where the exposure is inconsiderable; but the dampness, arising from the drip of the mouldings and from the action of capillary attraction, in cases where projections hold the moisture, appears to exercise an important influence on the condition of the stone itself.

10. It does not appear to us that the decay is attributable, as is commonly supposed, to the stones in the building not being placed upon what is technically called their natural bed, or in the same relative position as they occupied in the quarry; thus, stones which are found horizontally in the quarry, appear to have been often

placed perpendicularly in the building, and used for purposes of the most delicate decoration without any injurious result. As an instance of this fact, we may point out the elaborately carved shields of arms under the range of the first-floor windows; the stones used for those shields, though universally placed perpendicularly to their natural position in the quarry, present, so far as we are aware, few, if any, symptoms of decay.

11. The extent to which the decay on the whole surface has proceeded, it is not very easy to estimate. At the present moment the actual decay is doubtless considerable for a building so recently erected; but the change of colour in the stone itself, and the "fretting out of the surface," which are suggested as the first symptoms, lead us to apprehend that there may exist much more mischief than at present is actually apparent.

12. One of the witnesses examined, however, and whose judgment as a practical man is of considerable value, is of opinion that the decay, after proceeding to some depth in the stone, stops of itself; that an induration of the surface takes place, and that no further decay ensues. The Committee would willingly accept this opinion if they considered it well founded, but they cannot conceive that it is true to any considerable extent, notwithstanding there may certainly be some few indications which lead to the belief that in some cases it may be correct.

13. At present the decay appears for the most part on the plain surfaces, whilst the finer and more elaborately wrought portions of the building, unless under projections, are not seriously affected. And however disappointing and disfiguring these defects may be, especially in a building so recently erected, the Committee are of opinion that at present the decay does not affect the stability of the structure.

II. The Causes to which the Decay is attributable.

14. This part of the inquiry naturally leads to a reference to the evidence which has been obtained by the Committee on the subject of the stone itself. The result of this evidence may be thus briefly stated. The stone recommended by the Commissioners for this building was that from the quarries of Bolsover Moor and its neighbourhood, and this stone was actually contracted for in the first instance. Before the work began, however, it was found that blocks of sufficient size could not be procured from those quarries, and, in consequence, one of the Commissioners was appointed to proceed to the spot, to ascertain whether other quarries might not be discovered furnishing stone in beds of greater thickness and of larger dimensions. These conditions were found in the quarries of Anston, and the stone of greater thickness produced from these quarries has been used not only in this building, but in all the other buildings constructed of magnesian limestone in the Metropolis, after the quarries of Bolsover Moor had been abandoned, for the reason above stated.

15. The recommendation of the Bolsover stone in the Report of the Commissioners was founded on its similarity to that used in the Norman portions of Southwell Minster, which were stated in the Report to be in a high state of preservation. Evidence has since been adduced, in a letter from Mr. Scott, to be found in the Appendix, which renders it probable that the stone of this Minster was really obtained from the ancient quarries at Mansfield Woodhouse. The latter quarries were re-opened, and a considerable quantity of stone from them (exceeding 20,000 cubic feet) was made use of in the Palace at

Westminster; but in their turn they were relinquished from dissatisfaction as to the size of the blocks, though we have it on evidence, confirmed by our own observation, that the stone used from these quarries has stood remarkably well.

16. The evidence brought before your Committee on the subject of the stone obtained from the Anston quarries is very conflicting; the contractor and his principal foreman stating that the stone was, with slight exceptions, extraordinarily good, while other witnesses maintain that, even in the quarries themselves, there are stones in a state of actual decomposition; and one very important witness, a foreman employed at the Palace at Westminster, asserts that he knew that certain beds in some of the quarries were liable to decay, and that he abandoned them in consequence. With reference to the selection of stone, the Committee venture to remark, that it is much to be regretted that the offer made by one of the Commissioners particularly well acquainted with the selection and working of stone, to examine that used in the Palace at Westminster for the moderate salary of 150*l.* per annum was not accepted, owing to some difficulty in regard to the party who was to be held responsible for this unimportant amount; and that the matter was left to persons who admit that they had little or no prior experience of this description of stone, though they evidently entertained suspicions of the durability of some of it which they were employing.

17. With reference to the very natural and important question of the actual causes of the decay of this stone when exposed to the London atmosphere, the Committee take the liberty of referring to the Report of the chemists who were members of the Committee to whom this question was specially referred. This will be found in the Appendix.

III. The best Means of Preserving the Stone from further Injury.

18. This part of the inquiry referred to the Committee naturally divides itself into two questions: namely, as to the steps that have hitherto been taken, whether experimentally or otherwise; and as to those that are to be recommended for adoption hereafter. With regard to the first question, we have ourselves examined with care the result of what has been done at the Palace itself, either experimentally on the river front, or, as in the inner courts, by actual coatings or washings over large surfaces. With regard to the second question, our inquiries have been earnest and elaborate, and we have examined many witnesses and given much time to the consideration of the various propositions obtained by advertisement or otherwise. As will be seen in a subsequent part of the Report, we finally referred this question to the further consideration of the professional chemists who were on the Committee.

19. On the first question, the Committee are decidedly of opinion that it is not necessary nor desirable to proceed with any general coating, painting, oiling, or washing of the whole building. It is quite obvious, in their judgment, that a very large proportion of the stone does not require any such application; but that what is wanted is some efficient process which should be applied to the surface of any stone that begins to show symptoms of decay, with a view to arrest its progress. The Committee believe that the persons to whom the care of the building is entrusted ought to watch it, and note in the very earliest stages wherever decay is perceptible, by efflorescence, change of colour, crumbling, or slight decomposition.

20. In cases where the decay is important, and evidently occasioned by the fall of rain on an upper projecting or exposed surface, protection should be afforded by a covering of sheet zinc or lead; and if hereafter any composition should fortunately be discovered, by which the decaying stone could be at once covered or coated, and the injurious influences of the atmosphere prevented from further acting upon it, the difficulty would be solved. In some extreme cases the decayed stone might be cut out, and replaced by a new one. With regard to the processes which have actually been applied, whether experimentally or extensively, your Committee are decidedly of opinion that the discovery of a proper mode of treating stones in a state of decay has not yet been made, and there is no evidence whatever on the building itself to induce them to believe that the decay, where decay has arisen, has been arrested, or that permanently the decay has been prevented by any of the processes yet applied.

21. With reference to the second question, we found ourselves unable, after much labour, to come to any definite conclusion; and we finally requested the chemists in the Committee to examine and report upon it; but those gentlemen state, as appears by their Report in the Appendix, that the nature of the inquiry is so extensive, and that time is so important an element in the solution, that they are unable to give any opinion upon the subject. They further state, that they spent five whole days in the examination of only one suggested remedy, but they are unable, notwithstanding, to give any opinion on even that one suggestion. They allude to secret processes, regarding which they say they can offer no opinion, but they express a doubt of the applicability of any suggestion which would demand the veil of secrecy for protection. Concurring in this view, it may be further noted that even if such applications were found successful in sample or experiment, no security would be afforded for a corresponding success in any subsequent large operations. They recommend that a series of experiments should be conducted under chemical supervision for a considerable period of time, and the Committee are most reluctantly compelled to coincide with them, and to urge upon the Government the adoption of such a course.

IV. The Qualities of the Stones to be recommended for future Use in Public Buildings to be erected in London.

22. On this head of the inquiry the Committee have been unable, in the time allotted to them, to go into any very extensive examination. It is obvious, however, that although some varieties of magnesian limestone are an excellent and durable material, when not exposed to the deleterious influences of the London atmosphere, yet that in London it is subject to causes of decay, which render it an undesirable and unsafe material for the construction of public buildings.

23. It is equally obvious that Portland-stone, well selected, has been used in buildings in London from the date of St. Paul's downwards, under circumstances of great exposure, and with most successful results. Portland stone is a material to be obtained in any quantity, and in blocks of any size, beautiful in colour and texture, reasonable in price, not by any means so hard as the Anston stone, and yet with a power of resisting the influences of the London atmosphere, that leaves but little to be desired. It must be remarked, however, that Portland stone should be carefully selected, an operation which would be the most satisfactorily effected by an agent at the quarries.

24. On this subject the Commissioners could of course bring much personal experience to bear; but after the valuable explanation of the principles upon which the decay of stone depends in populous places as given by the Chemists, in their Report before referred to, the Committee refrain from repeating those conclusions, in which, however, they entirely concur.

25. During the inquiries of the Committee, one of their members, Mr. Burnell, who is well acquainted with architectural and engineering works in France, undertook, at his own expense, a journey to Paris, to inquire into the practice of the French architects engaged in the Government works in that metropolis. There, the stone used, the "calcaire grossier," though a carbonate of lime of tertiary age, and, therefore, of very different mineral composition from our magnesian limestone of the much older Permian age, seems to suffer also from decay in a comparatively pure atmosphere, and where wood is chiefly used as fuel.

26. From the evidence of Mr. Burnell, it does not appear that French architects or chemists have been more successful than ourselves either in the use of materials not subject to atmospheric influences, or in the application of processes for arresting decay, when it has once begun. The opinions of the most scientific chemists and architects in France on this subject have, however, in this way been obtained, and it is extremely probable that the inquiries undertaken by them, simultaneously with those undertaken in this country, may hereafter lead to some successful result.

27. The Committee have to thank the Government for the facilities given to Mr. Burnell in this important part of the inquiry, by providing him with an introduction which obtained for him the active assistance of Her Majesty's Ambassador at the Court of the Tuilleries.

28. The Committee delegated, as before stated, to a Sub-Committee the duty of examining the various buildings in London in which magnesian limestone from the Anston quarries has been introduced in the external architecture. The Report of this Sub-Committee forms part of the Appendix; and we beg to refer to that Report as confirmatory of our opinion of the uncertain character of magnesian limestone, and the risk attending the use of it in London.

29. In conclusion, the Committee venture to recommend that the architect of the Palace at Westminster, assisted by scientific chemists, should examine and record the actual state of the stone work of the building at the present moment; that experiments should be made by their direction, under various conditions of height, exposure, and aspect, with such preservative materials and agents as the chemists may suggest from time to time, and that researches should be continued into the effects of the various alkaline silicates, the phosphates, and other substances which have been brought under the notice of the Committee, or suggested in Germany, France, or elsewhere; that where decay arises from damp, means should be taken to protect the stone, as has been before suggested; that any stone extensively decayed should be removed and replaced; but that in particular the earliest symptoms of decay should be carefully watched, and examined with the view to the application of some immediate remedy. The Committee believe that a very large portion of the stone in the Palace at Westminster is of a very durable nature, and they entertain a confident expectation that a remedy will soon be found to arrest or control the decay when it has unfortunately begun to appear.

WILLIAM TITE, ROD. I. MURCHISON,
 SYDNEY SMIRKE, GEORGE GILBERT SCOTT,
 GEORGE GODWIN, M. DIGBY WYATT,
 A. W. HOFMANN, E. FRANKLAND,
 F. A. ABEL, JAMES TENNANT,
 GEORGE R. BURNELL, THOMAS HAWKSLEY,
 CHARLES H. SMITH, EDWARD M. BARRY.
 ALFRED BONHAM-CARTER, Secretary.

The following letters have been addressed to the Chairman of the Committee on the Decay of Stone of the New Houses of Parliament:—

June 17, 1861.

Sir,—We have the honour to inform you that we have complied with the wishes of the Committee by examining into the several proposals which have been laid before them for the preservation of the stone of the New Houses of Parliament; and that we have arrived at the following conclusions:—

1. Amongst the processes proposed, varying in principle and value to a very considerable extent, there is not one which we at present feel justified in proposing that the Committee should definitely recommend as a preservative, either for general or local application.

2. A minute examination into one class of processes, submitted to the Committee at an early period, has convinced us that, surrounded with great difficulties as the subject appeared at the outset, the obstacles eventually met with in an effective experimental inquiry are of a far more formidable character than could have been anticipated. Having devoted five days exclusively to the practical study of one of those processes (Ransome's), and having been unable, in that period, to elaborate even this single process sufficiently to warrant us in expressing a definite opinion upon its merits, it is obvious that anything like an elaborate examination of the numerous proposals which have only just now been submitted to us, would require the expenditure of a far greater amount of time than the Committee could place at our disposal.

3. Whilst regretting that it is not in our power to lay before the Committee a positive recommendation of any particular process, we beg to submit the following observations:—

An examination into the nature of the several processes proposed leads to their classification, under two heads:—

(a) Processes which are likely to afford permanent protection to the stone.

(b) Processes which are only calculated to afford protection of a temporary character.

In both of these classes there are proposals which may at once be excluded from further consideration, on account either of their inapplicability to stones when placed in a building, or of the obvious misapprehension, on the part of the proposers, of the problem to be solved.

A proposal to protect stones by immersion in a boiling mixture of pitch or resin and oil, may be quoted in illustration of the processes which are only applicable to stones previous to their having become integral parts of any structure; again, the suggestion to cover the building with a coating of a mixture of silica with sulphur, applied in a semi-fluid condition, would involve almost insurmountable difficulties in its practical application, not to speak of the inflammability of the sulphur, which is only slightly diminished by the presence of the silica, or the uncertainty of the temporary character of the protection which, even under the most favourable circumstances, could be afforded by this material.

Several of the suggestions are based upon notions so obviously erroneous, such as coating the building with sulphate of lead, and procuring an alleged galvanic protection, by establishing connections of this coating with plates of zinc; or, of ridding the building of the principle of decay by fermentation, that no object whatever could be gained by entering more fully into the merits of these proposals.

Of the processes which are intended to afford permanent protection to the stone, and the use of which is not precluded by the conditions of the case, there are several which claim a careful investigation. These processes may be classed under the following heads:—

1. Application of silicates of the alkalies, in various states of concentration.

2. Application of silicates, in conjunction with various saline compounds, intended to produce double decomposition.

3. Application of hydrofluoric, or hydrofluo-silicic acid, or their saline compounds.

4. Application of phosphoric acid, and acid phosphate.

5. Application of solutions of the alkaline earths, or their bicarbonates, in water.

All these processes are more or less based upon chemical considerations, which are supported by analogy, and which, in the case of the two first-named classes, have received considerable experimental confirmation. The experiments which are now in progress with several of the processes included in the two first subdivisions will, we believe, in the course of a few years, furnish ample data for correct conclusions regarding their applicability. In the meantime it might be advisable to apply to portions of the New Houses of Parliament actually undergoing decay, certain processes selected as representatives of the remaining classes above enumerated, in order that their merits might be submitted to the only conclusive tests, those of actual application, and protracted exposure to the corrosive influence of a London atmosphere.

The second division of processes,—namely, those which are only calculated to afford protection of a temporary character, are, from their very nature, of minor importance for the purposes of the Committee's inquiry; nevertheless, as the claims to permanence of none of the processes of the first division have as yet been substantiated by the test of time, we would recommend that, in addition to the experiments already made in this direction, further trials be instituted of some of the more promising materials of this particular description. This recommendation is based upon the consideration that substances, included under the appellation of organic, differ essentially in their powers of resisting the destructive action of the atmosphere. Whoever is acquainted with the nature of organic substances, cannot fail to appreciate the different degrees of stability under atmospheric influence exhibited by gluten, gelatin, or starch (which we find enumerated among the proposed protective agents), and by bees-wax and paraffin, not to speak of many of the fossil gums, which exhibit a degree of permanence approaching that of mineral substances.

The materials which we would recommend for selection to be tried in comparison with linseed oil, are paraffin, bees-wax, and some of the more permanent gums and resins, applied in the form of solutions in volatile solvents.

We should not omit to remark, that some of the witnesses and others who have addressed the Committee, speak of secret processes. We cannot of course offer

any opinion regarding such proposals, but we should doubt the applicability of any suggestion which would demand the veil of secrecy for protection.

Finally, we beg to state, as the result of the experience which we have been enabled to acquire during the prosecution of our investigations on this subject, that a definite solution of the question at issue can only be arrived at after the lapse of a considerable period, since the relative merits of the processes which we recommend for trial can be established only by the test of time.

We have, &c.,

(Signed)

A. W. HOFMANN.

E. FRANKLAND.

F. A. ABEL.

(To be continued.)

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

ALL the analytical researches undertaken for nearly a century past tend to show that compound bodies possess a definite, constant, and unvariable composition. The proportions deduced from these researches have been successively termed "Proportional numbers, chemical proportions, chemical equivalents, or atomic weights." The illustrious Berzelius devoted a great part of his life to settle the relative weights of these chemical proportions. His works on this subject remain imperishable monuments of his penetration and genius. The minute and reiterated control to which I have had the presumption, not to say the temerity, to submit them has convinced me that his analytical skill has never been surpassed—in fact, has never yet been equalled. Berzelius, it is known, concluded from his researches that no relation whatever existed between the atomic weight of bodies. He remained convinced of this fact all his life.

In 1815 Dr. William Prout, in a memoir entitled "The Relation between the Specific Gravity of Bodies in their Gaseous State, and the Weight of their Atoms," propounded the idea that the atomic weights of those bodies best ascertained at that time might be represented by multiples of the atomic weight of hydrogen. Prout had sufficient diffidence as to the correctness of his hypothesis to publish his work anonymously. Whatever may be the general opinion of this hypothesis, or whatever fate is reserved for it, it is impossible not to pay homage to the rare penetration of its author. True or false, matters not; it enabled him to divine the specific gravity of hydrogen forty years before it was determined in an almost absolute manner by experiment.

In a philosophical point of view the compass of Prout's idea is immense. In England this hypothesis has been very generally accepted as an absolute fact. The work published by Professor Thomas Thompson, of Glasgow, to support it by analytical experiment, contributed much to this result. Yet it was not so in Germany and France. The immense *prestige* which surrounded the name of Berzelius, and the great confidence which his works upon atomic weights inspired, was undoubtedly the reason. Doubts even arose in England in 1833. Professor Turner, at the invitation of the British Association for the Advancement of Science, undertook a series of analyses, from which he came to the conclusion that the hypothesis of Dr. Prout was not correct. In 1839 Pro-

fessor Penny, of Glasgow, arrived at the same conclusion, although his results differed in various points from these obtained by Turner. The determination that M. Dumas and the author made in 1839 and 1840 of the atomic weight of carbon again drew the attention of chemists to this subject. The carbon atom derived from the synthesis of carbonic acid, agrees entirely with the fundamental hypothesis of Prout. The new synthesis of water that M. Dumas made known in 1843, and which was confirmed by the works of MM. Erdmann, and Marchand, lead to the same result. In 1842 and in 1843 M. de Marignac, of Geneva, undertook a series of experiments upon the atomic weights of chlorine, bromine, iodine, nitrogen, silver, and potassium, with the view of submitting Prout's law to a new and rigid examination. The Genoese chemist, whose accuracy is well known, and to whose ability and conscientiousness Berzelius paid the homage which they deserved, stated most positively that the unity taken by Prout was too great by one-half, as far as chlorine is concerned. But he stoutly declared, that, considering the extreme difficulty of arriving by experiment at absolutely accurate results, the atomic weights that he had found for silver, potassium, bromine, iodine, and nitrogen, should not be considered as contrary to Prout's law. Thus, according to M. de Marignac, the principle may be correct, only the unity ought to be lessened by one-half.

Towards the end of 1857 M. Dumas published a memoir "On the Equivalents of Simple Bodies." As might have been expected, this work produced a great sensation. The celebrated French chemist, relying upon the works of M. de Marignac on silver, found that all the well-known bodies which he had had occasion to examine obeyed perfectly Prout's law, provided this law were slightly modified. According to him, the simple bodies are multiples of hydrogen either by 1, or by 0.5, or by 0.25. My illustrious master accompanied his work by important considerations on natural philosophy, but which have no connexion with the question of which I intend to treat.

Such is the present state of our knowledge of the relations between the atomic weights of simple bodies.

For several years I have devoted all my leisure to elucidate this problem. I will say candidly, that when I undertook my researches I had an almost implicit confidence in Prout's law. The doubt that I expressed in my work on the atomic weight of carbon, published in 1845, being owing to the result of two determinations which would not agree with my other synthetical experiments. My investigations have been on chlorine, bromine, iodine, fluorine, sulphur, nitrogen, potassium, sodium, lithium, calcium, barium, lead, and silver. It will scarcely be credited that during the long and tedious work to which I had devoted myself, I obtained successively the most discordant results, according to the body and the means I employed. Sometimes these results agreed entirely with Prout's law, as, for example, in the case of lead, silver, sodium, and calcium defined in relation to the atomic weight of carbon; at other times they were thoroughly at variance with this law, as in the case of lead determined in relation to the atomic weight of nitrogen and sulphur; of silver determined in relation to nitrogen and chlorine; of potassium in relation to oxygen and chlorine.

I am able to state without fear of being contradicted, that every chemist who has devoted himself to researches on the atomic weights, if he has varied his methods of operating, if he has sought for the atomic weight of a body by referring it to the atomic weights of two or

three different bodies, has met the same obstacles and the same contradictions. The most tedious perplexities have in this way attended my investigations.

Although I have but lately made it known to the Academy, begging them to take note of my assertion, all doubt has since disappeared from my mind. I have acquired the most complete conviction, and the greatest certainty that man can feel on such a subject, that Prout's law, with all M. Dumas' modifications, is a mere illusion, an hypothesis absolutely contradicted by experiment. Chemists, after having examined the work which I have now the honour of presenting to the rigid scrutiny of the Academy, if they can divest themselves of their prejudices and their present ideas on the subject, and keep to experiment, will soon arrive at my conviction, which is—That no common divisor exists between the weight of the simple bodies which unite to form all definite combinations.

The present work only contains my researches on nitrogen, chlorine, sulphur, potassium, sodium, lead, and silver. I have chosen these bodies because they are well known; they give the most stable compounds, and are supposed generally to obey Prout's law.

I have sufficiently explained that the object of my work is not to determine the absolute atomic weight of these bodies, but only the relation between their atomic weights, so as to see if there exist among these relations a common divisor. I have employed methods which, to my mind, are likely to give the most certain results; and in varying the processes I have given the preference as much as possible to those which have been already used by chemists of well-known accuracy. In this way I have obtained the most valid results.

It is clear that if these results, repeated several times, agree with those previously obtained, the probability is in favour of their accuracy; if, on the contrary, they differed, I found, by studying attentively all the conditions of the experiment, the way to ascertain the truth. I have thus discovered errors which would otherwise undoubtedly have escaped me.

Although the end I had in view was to search for the relations between certain numbers, I have nevertheless made two determinations, both of which assist to deduce from all my experiments the atomic weight of the seven simple bodies in relation to oxygen. We shall soon see that the atomic weights are very similar to those generally accepted, with the exception of nitrogen.

The determinations of these relations have been made either by synthesis or by double decomposition. I have only had recourse to analysis to obtain the means of calculating the atomic weights in relation to oxygen.

Before giving a succinct account of my researches, I ought to explain for the extremely large quantity of material with which I have experimented. These quantities may well astonish chemists. I did not, however, break through the established rules without due consideration. I can quote two precedents in the history of science:—M. Dumas, in his memoir on the composition of water, and M. de Marignac, in his work on the atomic weights of chlorine, nitrogen, and silver, previously employed large quantities of material. For myself, in my memoir on the atomic weight of carbon, published in 1845, I have increased tenfold the weight of the quantities usually employed. Up to the present time, as far as I know, no one has objected to this method of operating.

If we examine attentively the atomic weights which are generally recognised, and compare them with what they should be according to Prout's law, we shall soon

see that they are separated by unimportant variations. Besides, if it were not so, the problem would have long been solved. But experiment shows that the only means of proving with certainty, and of measuring exactly, the small discrepancies, is by increasing the quantities which produce them, so as to make a great disparity between these errors and those of the experimenter, which must be deducted from them. It may be imagined that the difficulties of the researches increase with the increase of quantities. Those chemists who may take the trouble to verify my results in all the various stages, will soon perceive that this is not the case: the only real inconvenience resulting from the use of large quantities is the length of the experiment, which in some cases is increased in the same ratio as the quantity. But, in return, if they continue the experiment to the end, the result carries conviction with it, because it is susceptible of only one interpretation; whilst, by employing less quantities, one is always apt to regard slight variations in the experiments as due to the inevitable errors of manipulation. One remains in uncertainty; and besides that, truth sought is liable to be lost altogether.

In looking over works treating of the atomic weights, I have often regretted that all the details of the experiments are not stated. I have thus been deprived of the means of satisfying myself as to how far the results merited the confidence which has been given to them. Being desirous, therefore, in all that concerns the results of my own researches, to give to every one the means of verifying and correcting them if they desire to do so, I have, therefore, endeavoured to give in the present memoir a minute description of every condition through which my determinations have passed. For this reason I have described accurately the balance and weights which I used to weigh them, the methods employed to guarantee the exactitude of the weights, the materials and the vessels; and to preserve the identity of the weight of the vessels during and after the experiment, I have also mentioned the nature of the vessel in which each operation was made.

I commence by showing how I prepared the substances submitted to experiment, the substances I made to react upon them, and lastly, the means employed to find the purity of each.

It is impossible to reproduce in this work a complete detail of my experiments; I shall therefore confine myself to the facts indispensable in appreciating the value of my researches, and the confidence which they should inspire.

(To be continued.)

On Tinkalite (Borate of Lime and Soda) from Peru,
by T. L. PHIPSON, *Phil. Nat. Doct. Bruxelles University, Member of the Chemical Society of Paris,*
&c. &c.

THE useful mineral of which I am about to speak was discovered only a few years ago in the deposits of nitrate of soda in Peru. It is now imported into Europe in considerable quantities as a substitute for borax. It was first examined in 1850 by M. Ulex, who found that it contained boracic acid, lime, and soda; but the analysis published by this author shows too small a quantity of water and too much boracic acid. In 1859, M. Kletzinaky received some samples of this same mineral from the Western Coast of Africa, and the analysis he has made

of them coincides pretty well with my analysis of some specimens from Peru.

Tinkalzit is found in the layers of nitrate of soda of Southern Peru, in the shape of irregular globular masses, which the natives call "tiza," and which vary in size from that of a nut to that of a potato. The outer crust of these tubercles is rather hard, but they are easily broken, and are then seen to be formed, in the interior, of a mass of crystalline needles intersecting each other in all directions, and of a brilliant white, satiny appearance. Often these globular masses contain reddish crystals of gypsum and other minerals; and they always contain a certain quantity of common salt, which gives to the mineral a brackish taste.

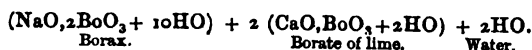
Water extracts from tinkalzit all its chloride of sodium and borax; acids easily dissolve the whole mineral, leaving only a small residue of very fine sand.

The density of tinkalzit I find to be 1.93 (according to M. Kletzinsky, = 1.9212; and according to M. Ulex the satin-like fibres, = 1.8). The analysis of the mineral in question has given me the following result, by the side of which I have placed the analysis of M. Kletzinsky:

	American Tinkalzit. Phipson.	African Tinkalzit. Kletzinsky.
Water	34.00	37.40
Soda	11.95	10.13
Lime	14.45	14.02
Boracic acid . .	34.71	36.91
Chlorine	1.34	1.33
Sulphuric acid .	1.10	0.50
Silica	0.60	..
Sand	2.00	..
Phosphoric acid .	Traces	..
Alumina	..	..
Magnesia	..	..
	100.00	100.00

The difference in the quantities of water, as shown in these two analyses, is owing to the gradual loss of water by soda-salts when exposed for some time to the air.

By neglecting the accidental elements and calculating the water, soda, lime, and boracic acid, we arrive at the formula:—



The presence of the other substances given by analysis seems to indicate that this mineral has been formerly deposited by mineral springs; and the fact that the biborate of soda it contains has crystallised with ten equivalents of water, shows that the temperature of these springs could not have been greater than + 55° Centigrade. As it is imported from America for industrial purposes, tinkalzit contains about 60 per cent. of borax, 25 per cent. of borate of lime, and 2½ per cent. of common salt. It forms an excellent flux for metallurgic purposes, and has been employed with success in the porcelain manufactory of Sèvres by M. Salvétat. In fact, it appears capable of effectually replacing borax in all the applications of this rather expensive salt to industrial or manufacturing processes.

To extract boracic acid from tinkalzit a given quantity of hydrochloric acid is diluted with water and saturated, while boiling, with the pulverised mineral; the clear solution is decanted while hot, and upon cooling the liquid throws down the boracic acid in large quantities.—*The Technologist*, Sept., 1861.

TECHNICAL CHEMISTRY.

The Deodorisation of Sewage at Exeter.

At the last Monthly Meeting of the Local Board, Mr. Pince said, in addition to the report which he had to make, as Chairman of the Committee appointed to investigate the plan for disinfecting the sewage, he had received a very satisfactory letter from Mr. Margery, Engineer to the South Devon Railway, who said he had used the carbolic acid which Mr. Pince had supplied him with, with effect in horse-boxes and fish-vans. He had also received another letter from Dr. Kingdon, of the St. Thomas Lunatic Asylum, to whom a bottle of the disinfectant was sent, requesting him to use it in the most offensive parts of the Institution. The Doctor bore testimony to the high qualities of carbolic acid as a deodorising agent, and that having used it for some time he had found its effects to be instantaneous. In addition to these letters, he had to state that on Friday evening he attended, with Mr. Farrant and their Surveyor, at the depôt, and they had tested the sewage in the same kind of manner as other members of the Board had previously done. They proceeded to a place where there was a low level, and which was covered over with a thick scum. They scraped off a portion of this, and Mr. Farrant tested it. All of them smelt it, and nothing could be perceived but a rather pleasant smell of carbolic acid. That was highly satisfactory as far as Mr. Ellis and himself were concerned, and was fully confirmatory of the evidence adduced by the rest of the Committee. The letter of Mr. Farrant was to the effect that he had tested the sewage, as mentioned, and could not detect the slightest trace of noxious effluvia. He considered the present system of deodorisation most perfect. Mr. Ellis, the Surveyor, then read his report. Having stated that the three months proposed for the experiment of deodorising the sewage had expired, Mr. Ellis proposed to offer a few remarks on the subject:—

"The treatment of the sewage must necessarily be taken under two heads,—first, its disinfection, so as to render it free from smell and prevent it causing any nuisance; secondly, to utilise it, in order to make it remunerative to the Board. The average quantity of carbolic acid used per day, since its commencement, has been about one gallon, at a cost of 11d., diluted with lime-water in the proportion of about 1 in 250, and applied to the sewage about ten hours per day, Sundays excepted. The result, I have no hesitation in saying, has been most satisfactory. . . . The action of the disinfectant on the sewage is not, as many suppose it to be, of a temporary character,—viz., to abate and destroy smells for a time only, but, on the contrary, the carbolic acid possesses considerable power to disinfect, through staying, or, rather, preventing decomposition, which prevents the sewage becoming putrescent and giving off offensive emanations, or again reverting to its former state; and when the sewage is once disinfected at the depôt, or in any part of the sewer, it matters little how long it takes passing off; decomposition cannot again set in, nor can any noxious gases be afterwards evolved in the atmosphere. Through the application of the disinfectant upon the sewage being previous to the commencement of decomposition, ammonia is not formed, but becomes locked up and preserved in the sewage, both in the liquid and solid state, which necessarily enhances its value very much in an agricultural point of view. This is of the utmost importance, as it also renders the sanitary

state of the atmosphere in the immediate neighbourhood where the sewage passes comparatively pure, the carbonate of lime having answered the purpose of disinfecting the sewage, during the past three months of summer, and by the use of, on an average, about one gallon of carbolic acid per day, at a cost of 11d., whilst the quantity required to produce the same effect in winter, with the atmosphere at a low temperature, would be about two-thirds that quantity. Thus, estimating six months of the year at one gallon per day, at 11d. per gallon, and say 1½d. per day for lime, would give a cost of 18. 0½d. per day, or, for the six months, of 7½. 10s.; whilst for the remaining six months the price will be 5½., giving the cost of the disinfectant for one year at 12½. 10s. The superintendence of the man, at 10s. per week, interest on outlay, and wear and tear, say 1½. 10s. per annum, will give a total of 40½. per annum. . . . The disinfecting being satisfactorily overcome, the next and very important part, that of utilising the sewage and rendering it remunerative to the Board, requires attention. First, in order to utilise the sewage, it is of course necessary to procure the assistance of the owners of the property through which the sewage passes; and were they sufficiently acquainted with its value, it would not have flowed to waste so long unheeded; but as prejudice is frequently a barrier to improvements of this kind, it necessarily becomes a work of time to undertake and carry it out, however much its merits are known and appreciated. And previous to the application of the disinfectant several objections offered themselves in preventing its being undertaken as a whole, which now no longer exist: First, from the land immediately below the dépôt, and through which the sewage passes, belonging to several owners, some difficulty was experienced in obtaining the sanction and assistance of the whole; but there would be no difficulty in that respect, as the whole may now be taken by Lord Devon and Sir Lawrence Palk, who are, I believe, both favourable to the scheme. Secondly, before the sewage became disinfected, it was thought necessary to apply the whole of it to the land at all times of the year, but which is now indispensable, as at certain times of the year the sewage, when disinfected, might be allowed to pass down the course, as at present, and be carried off without any additional expense than that of disinfecting, which would be self-acting. Thirdly, another important advantage offers itself, which is the increased value of the sewage, after being disinfected, by looking up and retaining its manurial qualities until absorbed by the soil. . . . I believe no difficulty whatever would be experienced in getting a sufficient number of shares taken up by practical persons who have long since perceived the desirability of facilitating and grappling with a question so considerably discussed and possessing such national interest,—viz., that of returning to the soil those beef-growing properties which are well known to exist in the sewage of our towns, instead of allowing it to empty into and pollute our rivers. As such a step would, in my opinion, be a stepping-stone to the utilising the whole of the sewage of Exeter, as I have no doubt but that our neighbours on the opposite side of the Exe will, at no very distant period, not only from the interest they will feel in improving the sanitary state of the neighbourhood where their sewers empty into the river, but also to recognise and remedy the great agricultural waste which at present exists,—a waste which could be easily obviated and turned to a very profitable account."

Mr. Pince said, with regard to the fertilising qualities of the sewage, he wished to state that he had procured a

barrel of it, after the application of the carbolic acid. He spread the contents of the barrel over a poor, hungry piece of land, about an inch and a quarter thick. This he did on Thursday night, and after he had spread it he went out and knelt down, and no smell existed. On Friday afternoon he sowed three drills of mustard and three of rape. On Monday morning the seed was up vigorously, showing how actively stimulated it had been by the manure. Now it was broadly in leaf, which was quite black, and he should be happy to show it to any person. If he had had recourse to stable dung he must have spread it ankle-deep in order to have produced such an effect. He believed that those who had previously tried to utilise the sewage had expected too much from it. They had expected that a little would have acted like guano, but this was not so. If, however, it was spread over the land in a fair way, he believed it would have the same beneficial effect as it had had on his land. He did not wish to take the sewage away from the dépôt, because it might be said that he was an interested party, but this he could say, that if it was desired he should be happy to have it. Mr. Ellis, in reply to a question, said he had no hesitation in saying that the value of the sewage was considerably improved. The ammonia in escaping caused the smell, and the disinfectant arrested the ammoniacal qualities of the sewage. Mr. Pince said that the man at the dépôt, who was scientifically ignorant of the disinfectant, spoke of the altered appearance of the sewage after the disinfectant was applied. He said it was "yeasty-like," and that was so. Mr. Bradbeer said he had noticed the same appearance in the sewage. Mr. Dyer said the ammonia in it was fixed; it was not like guano, the ammonia of which was always evaporating. Mr. Bradbeer said that, as far as the experiment had gone, it had proved successful, but next came the question of expense. Mr. Ellis stated that, including the attendance of the man, the expense per annum would be about 38½. 10s. Mr. Pince said that at present the experiment had been conducted in its rudest form; but if it was made self-acting, as it would be, then the expense would be considerably reduced. After some further discussion with respect to utilising the sewage, Mr. Pince said he had received a letter from the Earl of Devon, who said that as soon as he returned from London he should have great pleasure in visiting the dépôt, and see it, with a view to the ultimate disposal of the sewage. He (Mr. Pince) was sure that the noble Earl was perfectly earnest about the matter. He would now ask the Board to continue the disinfecting process for a specified time, until some plan was devised for utilising the sewage; and in the meantime he urged the members to unite with him for the purpose of seeing if they could not make a profit of the sewage, and if not to get rid of it without loss or nuisance. The scavenger wanted to cart it away at an expense to the Board, which ought not to be. Mr. Dyer was asked to take the sewage, but that gentleman said he had not time to attend to it. Mr. Pince then repeated his former offer in the same terms, and observed that he thought the reason why Mr. Dyer had failed in his attempt to utilise the sewage was because he had tried to make it into an artificial manure, whereas it should be used in its natural state. Mr. Dyer said he had sent some of his manure to a gentleman at Southmolton, and it having been used for turnips, the gentleman said he had never had a better crop in his life. Mr. Bradbeer said, as the former experiment had been effectual, and as Mr. Pince wished the Board to back him up by continuing the experiments for a certain time longer, so as to see in the meantime what terms they

could make with the landowners, he had great pleasure in proposing that they do continue their present system for a twelvemonth longer. They could also in the mean time appoint a day to meet a committee of the South Devon Directors. It was then resolved, after further discussion, on the motion of Mr. Pince, seconded by Mr. Snow, that the disinfectant operations be continued for another year.

NOTICES OF BOOKS.

The Retrospect of Medicine: being a Half-yearly Journal containing a Retrospective View of every Discovery and Practical Improvement in the Medical Sciences. Edited by W. BRAITHWAITE, M.D., &c. &c., and J. BRAITHWAITE, M.B. Vol. XLIII. January to June, 1861. London: Simpkin, Marshall, and Co.

ANY Chemist who has felt the want of an Annual Retrospective View of every discovery and practical improvement in Chemical Science—and what English Chemist has not?—will be able to form some idea of the value of a work like this to the medical practitioner. The book is, of course, a compilation from the various medical journals, from which, all relating to the science and practice of medicine and surgery that is worth perpetuating, is brought together, systematically arranged for easy reference, and reduced to a readable compass. It cannot pretend to supersede the weekly periodical, which is, indeed, a necessity for it, as well as for every man who wishes to keep perfectly *au courant* with the progress of science. The object of the Editors is rather to fix in a more stable form the valuable matters which are to be found, necessarily associated with much that is comparatively worthless, in the pages of the weekly publications. This process of precipitation and filtration it is acknowledged the Editors carry on with much skill; and how much the service is appreciated by the profession is shown by the fact that the *Retrospect* has now reached its forty-third half-yearly volume.

The present volume is enriched by a condensation of Dr. Pavy's valuable Lectures on Diabetes, from which we make an extract relating to the testing for sugar in diabetic urine. After reviewing the different tests for grape sugar, Dr. Pavy gives the formula for the preparation of a cupropotassic liquid which he prefers to those of Barreswil and Fehling. The composition of this liquid is as follows:—

" Sulphate of copper . . .	320 grains.
Tartrate of potash (neutral) . .	640 "
Potassa fusa . . .	1280 "
Distilled water . . .	20 fluid ounces.

Dissolve each in a separate portion of the water, then mix the sulphate of copper and tartrate of potash, and afterwards add the alkali."

The above liquid "is of such a strength that 100 minims of it are exactly decolorized by half a grain of the purest well-dried grape sugar." Dr. Pavy then describes the method he adopts in quantitatively estimating the sugar in diabetic urine,—"100 minims of the blue liquid are taken, and a mixture of one part of urine, with five parts of water (because the urine is too concentrated alone) allowed to fall into it, drop by drop, whilst kept boiling in a small porcelain capsule, over the flame of the spirit lamp or gas. As soon as the blue colour of the copper solution has been completely removed, the operation is at an end. The amount of diluted urine employed, can be read off, and will contain an equivalent to half a grain of glucose." The Doctor reviews at some length, the objections to both the copper and the fermentation tests, neither of which, he shows, can be relied on as giving an infallible indication of the presence of sugar.

The following "*New Salt of Iron and Quinine*," as it is called by Dr. Fergus, may be new to our readers, and is we dare say a very useful compound. It is composed of 80 parts sulphate of magnesia, 15 parts sulphate of iron, and 5 parts sulphate of quinine; or in a scruple 16, 3, and 1 grain of the respective salts. This mixture is quite unalterable in the solid state, and forms a perfectly clear solution, remaining so for an indefinite period. The proportion of quinine may be increased if necessary, an additional quantity being readily soluble in a solution of the salt.

There are many papers of great value and some of rather startling novelty, in this volume, witness that one, utterly destructive to a bilious man's faith in a blue pill, showing that mercury has no specific action on the liver.

We wish we could have to record the publication of a *Retrospect of Chemistry* as useful to Chemists as this *Retrospect of Medicine* must be to Medical men.

NOTICES OF PATENTS.

479. *Fire Places.* W. DRAY, Farningham, Kent. Dated February 25, 1861.

THIS invention has for its object the more complete combustion of fuel and the prevention of smoke. The grate or fire-basket is constructed of a cylindrical or spherical form, and is capable of being revolved or turned on pivots, trunnions, or otherwise, so that the gaseous and carbonaceous products evolved immediately after the addition of a fresh supply of fuel may be caused to pass through the hot fire, which by half rotation has been brought above it, and in this manner the consumption of the greater part of the smoke secured.

Such a fire would probably require a little attention to maintain uniformity of heating; its construction, however, is calculated to aid in preventing smoke, and is in principle somewhat similar to the smokeless grate which is fed from below.

505. *Artificial Stone and Cement.* F. RANSOME, Ipswich. Dated February 27, 1861. (Not proceeded with.)

THIS invention is a modification of similar processes for which patents have on previous occasions been granted to Mr. Ransome. For the manufacture of artificial stone the inventor proposes to employ crushed or powdered chalk, which, after being saturated with silicate of soda solution, is moulded into shapes or blocks; then freely exposed to air to allow the material to become dry and hard. The surfaces are then repeatedly washed over with a solution of chloride of calcium to induce the formation by double decomposition of silicate of lime, so that the pores of the mass may be filled with a mineral substance, and, when finally dried, constitute a kind of artificial stone of great durability, and to a certain extent impervious to water. It is proposed to apply the same materials for coating or plastering walls, ceilings, &c.; and by way of enumerating other substances which are equally applicable to these purposes, the inventor claims the use of either of the soluble alkaline silicates in conjunction with quick lime, any one of the soluble salts of the alkaline earths, or the chloride of aluminium or iron.

The processes referred to in the specification have been frequently the subject of experiment and discussion ever since the first announcement of M. Fuchs's original researches. The early objection has not perhaps up to the present time received a satisfactory answer. This relates to a tendency of the chloride of sodium, formed as the secondary product in the double decomposition, to give rise to a kind of efflorescence which impairs, and ultimately destroys, the outer surfaces of the stone by exfoliation.

634. *Improvements in Preventing Incrustation in Steam-Boilers, and in Tanks for Containing the Feed for the same.* J. FARRER, Clapham, Surrey. Dated March 13, 1861. (Not proceeded with.)

WITH the intention of preventing the formation of calcareous incrustation in steam-boilers, the inventor proposes to employ either rain or distilled water for feeding the same. In order then to collect and store a sufficient supply of rain-water for this purpose, it is recommended to construct large tanks for its reception, lined with non-absorbent bricks, united with hydraulic cement, or with water-resisting cement alone.

The applicability of distilled water to these purposes is out of the question, since it involves the consumption of at least a double proportion of fuel to generate but one equivalent of steam. Rain-water could be advantageously employed if it were possible to command a sufficient supply, and by so much as the ordinary well or river water is diluted with rain-water, so will the thickness of the incrustation in the boiler be diminished, but this mode of proceeding resolves itself merely into the common practice of draining the roof into the water-tank from which the engine draws its supply.

Grants of Provisional Protection for Six Months.

1722. William Pask, Lydney, Gloucestershire, "Procuring a colouring matter from the refuse of iron stone, called 'colour spert.'"—Petition recorded July 6, 1861.

1776. Thomas Cobley, Meerholz, Hesse, Germany, "An improved process for the production or manufacture of fluo-silicates of tin, zinc, and baryta, and their application as pigments for glazing, enamelling, and in the manufacture of glass."—Petition recorded July 15, 1861.

1790. Joseph Pierre Gillard, Paris, "Improvements in the manufacture of soda, carbonate of soda, and hydrochloric acid, and in apparatus connected therewith."—Petition recorded July 16, 1861.

1826. Constantine Nicolaus Kottula, Holborn, London, "Certain new compositions to be used in the manufacture of soap."

1840. William Edward Newton, Chancery Lane, London, "Improvements in engines for obtaining motive power by an explosive mixture of inflammable gases and air."—A communication from Francisque Million, Paris.—Petitions recorded July 22, 1861.

1885. James Robertson, Park Street, Mile End, London, "Improvements in apparatus for the treatment of bodily pain with hot water, steam, hot air, and the like."—Petition recorded July 27, 1861.

1910. Horatio Mearing, Great Randolph Street, Camden Town, London, "An improved lucifer match and prepared paper for igniting the same."—Petition recorded August 1, 1861.

2026. William Wilds, Hertford, "Improvements in apparatus for ventilating."—Petition recorded August 14, 1861.

2023. John Cowdery Martin, High Street, Barnes, Surrey, "An improvement in treating bones, and the manufacture of the products thereof."—Petition recorded August 15, 1861.

2053. William Bennett, London Street, Paddington, London, "A new and improved composition to be used as fuel, and in the lighting of fires."—Petition recorded August 19, 1861.

2110. Richard Arohibald Brooman, Fleet Street, London, "An improved method of treating the hop plant to obtain a material resembling wool."—A communication from Barthélemy Picard, Nancy, France.

2142. Benjamin Browne, King William Street, London, "An improved process for concentrating ores or tailings or separating pulverised mineral substances of different kinds or qualities from each other."—A communication

from Henry Eberhard Roeder, Broadway, New York, United States.

2159. Alexander Taille, Agen Lot et Garonne, France, "An improved manufacture of manure."

2161. Henry William Spencer, Stepney Causeway, Commercial Road, London, "Improvements in the manufacture of animal oils, the said improvements relating more particularly to the processes of refining them to be used for lubricating purposes."

2191. George Knight, Foster Lane, London, "Improvements in giving lustre to written and printed letters, figures, and devices."—Petitions recorded September 2, 1861.

2031. John Bethell, King William Street, London, "Improvements in the manufacture from steatite of journals, axle-boxes, and bearings for machinery, axles and spindles; in making smoking pipes, buttons, crucibles, and pots for chemical and smelting purposes, and also of a lubricating compound for railway and other carriages."—Petition recorded August 15, 1861.

2196. Patrick Robertson, Sun Court, Cornhill, London, "Improvements in treating yeast, and in the manufacture of ammoniacal salts and a substitute for animal charcoal."—Petition recorded September 3, 1861.

2213. Frederick Bennett, Upper Works, Bagillt, Flintshire, "An improved method of coating the interior surface of lead and lead composition pipes with tin or its alloys."

2229. Charles Felton Kirkman, Palace New Road, Lambeth, "Improvements in obtaining manure from sewage and in the apparatus employed therein."

2262. Marc Antoine François Mennons, Rue de l'Echiquier, Paris, "An improved combination of chemical and mechanical processes for the conversion of fibrous vegetable matters into paper-pulp."—A communication from Louis Achille Carteron, Dieuze, Meurthe, France.—Deposited and recorded September 12, 1861.

Notices to Proceed.

1163. Raoul Bernard, Bordeaux, France, "Improved means of catching flies."

1166. John Rolfe Hunt, Chichester Place, Wandsworth Road, Surrey, "Improvements in the manufacture of gutta-percha, and compounds thereof, with other matters and substances."

1200. Auguste Cesar Achille Gerard de Melcy, Paris, "An improved treatment of natural phosphate of lime for several purposes."—Petitions recorded May 11, 1861.

1207. Francis Puls, Francis Terrace, Hackney Wick, Middlesex, "A mode of preparing an oxidising agent."

1231. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in apparatuses for the manufacture of aerated waters, and in vessels or receptacles for containing the same."—A communication from Jean Louis André and Philippe Ferdinand Guillot, Paris.—Petitions recorded May 14, 1861.

1276. Frederick Oldfield Ward, Hertford Street, May Fair, London, "Improvements in manufacturing manure, and in obtaining accessory products."—Petition recorded May 8, 1861.

1955. Alcidaïs Auguste Romain Damoiseau, Paris, "Improvements in apparatus for drawing blood or other fluids from the human or animal body."—Petition recorded August 6, 1861.

1214. Thomas Bell, Usworth House, Gateshead, Durham, "Improvements in the decomposition of the compounds of aluminium and in coating metals with aluminium or its alloys."—A communication from Louis Le Chatelier, Paris.—Petition recorded May 13, 1861.

2221. John Reid, Leith, Mid-Lothian, North Britain, "Improvements in the treatment of gas and the apparatus employed therein with a view to its more accurate measurement in wet gasometers."—Petition recorded September 5, 1861.

CORRESPONDENCE.

On the Theory of Three Primary Colours.

To the Editor of the CHEMICAL NEWS.

SIR,—I am somewhat surprised that all your readers have passed over in silence the paper of Professor J. C. Maxwell, contained in the CHEMICAL NEWS of June 29, on the theory of three primary colours. As I believe he is in error on two or three points, and especially when he affirms that red, green, and blue are the primary colours, perhaps you will kindly allow me to make a few observations on the subject. Mr. Maxwell says that "experiments with pigments do not indicate what colours are to be considered as primary." This he says apparently because "the colours of a mixture of pigments are often very different from a true mixture of the colours of the pure pigments," and because the "green tint produced by mixing most blue and yellow pigments" is unsatisfactory. But the colour resulting from the combination of some of the colours of the solar spectrum is often very different from the intermediate colour of those employed, as well as those resulting from a mixture of coloured bodies; and the reason why some blue and yellow bodies do not produce a pure green is capable of satisfactory explanation. Mr. Maxwell remarks, that "by mixing chrome and ultramarine we get all hues of green." But these bodies are generally not adapted for producing green, because most of the ultramarine has a violet tint, and the chrome an orange one. When we wish to produce a pure colour from a mixture of two others, it is necessary to select either pure colours or, perhaps still better, those which are of the tint of the colour which we desire to obtain. By proceeding in this manner we shall never fail to get pure colours. There are bodies of a crimson colour, and others which are scarlet. The first is a red with a violet hue, and the second is a red with an orange hue. Now, when we wish to obtain a violet colour, we combine a crimson colour with a blue one; but when we wish to produce an orange colour, we mix a scarlet colour with a yellow one. If in the latter case we employ a body of a crimson colour, an orange colour is not produced, but a buff; and if Mr. Maxwell has not been very careful to procure coloured bodies of the proper tint, we need not be surprised that he has failed to obtain satisfactory results, and that he disapproves of the use of pigments and mixtures in determining which are the primary colours.

Evidently coloured bodies form the basis for a sound decision upon this subject; and I cordially agree with the generally received opinion, that red, yellow, and blue are the primary colours, because all colours of bodies can be produced from them, while those colours of bodies cannot be produced from any, and because, as every prismatic colour can be formed by combining the colours of the spectrum, we cannot so readily ascertain by experiments with them which are the primary colours.

Presuming, then, that red, yellow, and blue, are what are known as the primary colours, though it is not meant to include any theory of the manner of their production by the prism. I will attempt very briefly to show that this view is confirmed by experiments with the prismatic rays, in order that it may not be necessary to admit the incongruous statement of Professor Maxwell that red, yellow, and blue, are the primary colours for pigments, while red, green, and blue, are the true primary colours as determined by experiments on the prismatic spectrum. There are three different effects produced when coloured rays of light are superposed:—

1. When all the colours of the spectrum are combined, all colour is destroyed; as it may be also by the superposition of two or three of those colours which would produce the prismatic colours in about the same relative proportion as they are found on the spectrum.

2. When some of the coloured rays are superposed, no colour is destroyed, as in the formation of orange, green, indigo and violet from red, yellow, and blue.

3. When other colours of light are combined, a portion of colour is destroyed and a portion remains, as is always the case when red, yellow, and blue, are produced from the other colours.

This occurs because for colour to be destroyed, the colours combined must be of a certain tint or united in certain proportions, and if one of them be in excess, colour will be produced.

If this is true, it divides the prismatic colours into two classes; and that the division is correctly made, is exceedingly probable, because no one who is theoretically and practically acquainted with colour, can deny that when orange, green, indigo, and violet, are produced by the combination of two of the three colours, red, yellow, and blue, that the sensation of colour produced is a compound of the sensations which the two colours produce separately; and on the contrary, that when red, yellow, and blue, are produced from two of the other four colours, that the sensation produced is not a compound of the sensations which the two colours produce separately. I, therefore, conclude that this natural division of red, yellow, and blue, into one group, and of orange, green, indigo, and violet, into another, and its being the same division as we make of coloured bodies, is a sufficient reason why yellow should still be considered a primary colour.—I am, &c.

J. J.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Quadrachloride of Iodine.—Dr. H. Ksemmerer (*Journ. für Prakt. Chem.*, Bd. lxxxiii. s. 21) noticed some small, red, well formed octohedral crystals, which had separated from some fluid chloride of iodine that had been kept in a sealed tube for about six years. These he at first took for an allotropic modification of the chloride; but analysis showed that they contained four atoms of chlorine to one of iodine, and were consequently an hitherto unrecognised compound of those two bodies. Their formation is explained by the equation $4\text{ICl} = \text{ICl}_4 + 3\text{I}$, the iodine remaining dissolved in the mother liquor of the crystals.

Bromine Compounds of Molybdenum.—C. W. Blomstrand describes these (*Journ. für Prakt. Chem.* Bd. lxxxi. s. 433). *Bromide of Molybdenum* (Mo Br_2), formed by passing bromine vapour over metallic molybdenum strongly heated. Oxybromide is first formed, then a greyish-green sublimate of sesquibromide, and lastly some yellow bromure. A few dark-coloured crystals of the bromide are found mixed with the preceding. The author has varied his experiments, trying various temperatures, and diluting the bromine vapour by varying proportions of carbonic acid, but never could get much of the bromide. It forms brilliant, black, well-formed acicular crystals, which are fusible, and give a brown-red vapour. They deliquesce in the air, and yield a black fluid, give a brownish-yellow solution with water, from which alkalis precipitate a rusty yellow precipitate of oxyhydrate. By heating it is easily decomposed into the sesquibromure and free bromine, $2\text{Mo Br}_2 = \text{Mo}_2\text{Br}_3 + \text{Br}$.

Sesquibromure of Molybdenum.—(Mo_2Br_3) Formed as before, when bromine vapour is passed over metallic molybdenum, and also by the use of a mixture of the oxide and charcoal. It forms, at times, short blackish-green acicular crystals, but is mostly obtained in slender threads, like asbestos. By a strong heat it is decomposed into the bromure and free bromine. Under ordinary circumstances the sesquibromure has great stability. The threads made into a ball and thrown on water swim like a cork. It is not sensibly attacked by boiling hydrochloric

acid, nor by dilute nitric acid at the ordinary temperature. Dilute alkalis act on it slowly in the cold, and on boiling perfectly decompose it.

Bromure of Molybdenum.—(Mo Br , or Mo_2Br_2 , Br) Formed when the sesquibromure is heated to redness, and by the direct action of bromine in the metal. It is altogether insoluble in water and acids, but dissolves in boiling dilute alkalis, the yellow solution thus formed containing the bromomolybdous oxide united with an alkali, $\text{Mo}_2\text{Br}_2, \text{Br} + 2 \text{KOH} = \text{KBr} + \text{KO}, \text{Mo}_2\text{Br}_2, \text{O}$. The yellow solution, on standing, yields small, brilliant, orange-coloured crystals, of the compound $\text{Mo}_2\text{Br}_2, \text{O} + 9\text{HO}$. Saturated with carbonic or acetic acid, the hydrate, $\text{Mo}_2\text{Br}_2, \text{O} + 3\text{HO}$, is precipitated. The author notices another salt, the bioxybromide of molybdenum, formed in the same way as the corresponding chlorine compound lately described. It is obtained in orange-coloured scales of peculiar brilliancy, deliquesces in the air, and gives a colourless solution with water.

Composition of a Roman Mirror.—Souhay had the opportunity of analysing a fragment of a Roman mirror dug up near Mayence. It was of a greyish-white colour, brittle under the hammer, had a fine granular fracture, the fractured surfaces showing small holes filled with small metallic prisms. The sp. gr. = 9.21. The percentage composition was,—tin, 19.05; lead, 17.29; copper, 63.39. The alloy was probably made by melting together 1 part of tin, 1 of lead, and 3 of copper.—*Journ. für prakt. Chem.*, Bd. lxxii., s. 275.

Combustible Gas in a Salt Mine.—There is an opening in the deepest part of the Wieliczka mine, from which a stream of gas issues. When a light is brought in contact, the gas ignites, and a flame two or three feet long is seen. It quickly lessens, however, and soon goes out. Pebal (*Annal. der Chem. und Pharm.*, Bd. cxviii., s. 27) analysed the gas, and found it to be composed of

Nitrogen	65.47
Carbonic acid	1.18
Oxygen	12.90
Light carburetted hydrogen.	20.45

100.00

MISCELLANEOUS.

Telegraphic Night Signals.—On the evenings of the 24th and 25th ult. a series of experiments were made under the direction of Captain Bolton, for the purpose of ascertaining the capabilities of the lime-light as a means of communicating by signals between distant stations during the night. Southend and Sheerness were the stations selected, and, as the result of the trial, it was found practicable to hold correspondence by a system of time signals depending upon the alternate exhibition and screening off of the light. Notwithstanding the haziness over the water at the time, it was not difficult to interpret the indications on the opposite shores—the stations being distant from each other six miles. Further experiments are in contemplation to be carried out at Portsmouth over sea and land, when the *Excellent* gunnery ship will be provided with one of the signal lamps.

The Prevention of Railway Accidents.—With that keen attention to passing events which characterised his former management of the Polytechnic, Mr. Pepper has recently provided the visitors to that ever-attractive institution with a new lecture on the prevention of railway accidents, a subject which the recent catastrophes at Kentish Town and in the Clayton tunnel have invested with a fearful interest. The lecture was illustrated by numerous models and diagrams, among which may be mentioned those of Mr. Delafond's apparatus for the prevention of

accidents through attempts to enter or leave trains in motion, and Mr. Allen's electric bell and dial, for instantaneous communication between guard and driver. The great feature of the lecture, however, as bearing directly upon recent disasters, was a series of beautiful models of the safety kerb and life guards, patented by Mr. Wright, C.E. The former consists of a strong iron kerb, backed with earth, and of sufficient height to prevent locomotives or carriages from running off the rails, parallel to which the kerb is firmly fixed. In the case of the wheels mounting the rail, they rub against the kerb, and are turned on to the rail again directly, without the possibility of an accident. The life guards are strong pieces of iron, so affixed that, should a wheel or axle break, they rest on the kerb, supporting the carriage, and, by their grating, warning the guard, while they act also as a drag. Some beautiful photographs of the new International Exhibition building, produced by Messrs. Birnatingle, were shown, after the lecture, by Parr's new electric lamp.

Grocers v. Doctors.—A Company is, we understand, in progress of formation, to be called "The Equitable Grocery Consumers' Company;" and its object is—at least, so says the programme, 'to supply the public with all descriptions of grocery in a pure and unadulterated state.' Every pater- and materfamilias will feel deeply obliged to such an enterprising grocery establishment, provided only it will carry out punctually its promises. The benefits of it would reach very widely; for it would teach all other grocers to mend their wares. We have often adverted to the rascality of trade now pretty universally going on; and have also shown that the certificates of purity of their wares, as usually given by chemists and microscopists to the shopkeepers, are mere delusions, and worse than delusions. These certificates are, in fact, mere traps to catch customers; and we must say that those gentlemen who are such free dispensers of them are rather to be considered as abetting the tricks of the rascally shopkeeper than defending the public. Analyses of the kind we refer to are, in truth, scientific warrants, which may be, and often are, used as a cover or shield, under which vile goods are sold as superfine. Molasses of Oxford-street takes a specimen of his goods—we need not say Number One of its kind—to Dr. Singley, who, after due microscopical and chemical investigation of its nature, supplies Molasses with a certificate testifying that his specimen is perfect of its kind. Molasses bows and smiles, and thankfully pays his *honorarium*. He then repairs to his printer, and has a few thousand of the certificates struck off, and pasted on to the most villainous of wares. These wares then go forth to the world under the trumpet and patronage of the famous Professor Singley. We will say that on the very face of it, and in the way here described, our men of science in the analytic line are assisting rascally tradesmen in the distribution of their rascalities. May this Company teach and follow a better path.—*Brit. Med. Journal*.

ANSWERS TO CORRESPONDENTS.

F. W. G.—The complete researches are not yet published. We will give an account of the experiments as soon as possible.

J. J.—The paper was received with thanks.

A Reader.—The methods of calculating formulae of chemical compounds from the per-centage composition are given in a very complete manner in the last edition of *Frobenius' Quantitative Analysis*, published by Churchill, pp. 452 to 476.

C. R. C. Tichborne.—Received with thanks. A proof will be sent.

Alfred Peile.—We do not know the price. The book is published by Baillière, Regent Street.

Book Received.—"An Introduction to Practical Chemistry." By John E. Bowman. Edited by C. L. Flossam. Fourth Edition. London: Churchill.

THE CHEMICAL NEWS.

VOL. IV. No. 97.—October 12, 1861.

THE PRESERVATION OF BUILDING STONE.

(Continued from page 181.)

July 17, 1861.

Sir,—Having been requested to submit to the Committee our opinion on the nature and causes of the decay of building stones generally, and of the stone employed in the construction of the New Houses of Parliament in particular, we now have the honour to submit the following observations:—

Building stones in general may be divided into two classes:—

1. Those which consist of materials not easily acted upon by acids.
2. Those composed of materials which are, partially or entirely, acted upon by acids with facility.

As an illustration of the first class, granite porphyries and serpentines may be quoted, whilst to the second belong limestones, dolomites, and certain sandstones, containing carbonate of lime as cementing material.

The stone used in the New Houses of Parliament belongs to the second class of building materials, consisting, as it does, almost entirely of the carbonates of lime and magnesia. The following analyses of several varieties of dolomite by Professor Daniell and Messrs. T. Ransome and B. Cooper, are quoted in illustration of the general composition of the stone in question:—

	Bolsover Moor.		North Anston.		Woodhouse.	Steeple.
	Daniell.	Ransome and Cooper.	Ransome and Cooper.	Ransome and Cooper.	Ransome and Cooper.	Ransome and Cooper.
Carbonate of lime ..	51.1	52.07	54.89	55.37	52.80	53.95
Carbonate of magnesia ..	40.2	40.60	42.07	41.71	44.31	43.78
Silicate of lime ..	..	..	..	..	..	trace
Protioxide of iron ..	..	0.89	0.49	0.73	0.63	0.64
Oxide of iron and alumina ..	1.8	..	..	..	..	..
Peroxide of iron ..	..	0.83	0.24	..	..	..
Protioxide of manganese ..	..	trace	trace	1.68	1.84	..
Silica ..	3.6	3.64	0.36	0.92	0.47	0.44
Water ..	3.3	0.48	0.51	0.45	0.28	0.18

	Roch Abbey.	Huddlesstone.	Park Moor.	Lindley's Bolsover Quarry, Woodhouse, near Mansfield
	Daniell.	Daniell.	Daniell.	Ransome and Cooper.
Carbonate of lime ..	57.5	54.19	55.7	54.05
Carbonate of magnesia ..	39.4	41.37	44.6	38.58
Protioxide of iron ..	..	..	..	0.74
Peroxide of iron ..	..	..	..	0.62
Oxide of iron and alumina ..	0.7	0.30	0.4	..
Carbonate of manganese ..	..	..	..	2.43
Silica ..	0.8	2.53	0.0	1.30
Water ..	1.6	1.61	2.3	0.46

¹ The analyses by Daniell are quoted from the "Report of the Inquiry undertaken under the Authority of the Lords Commissioners

Regarded from a purely chemical point of view, the difference in the resisting power to corrosive agents of different stones would appear, at first sight, to depend entirely upon their chemical composition; but even a moderate acquaintance with the properties of the components of such building stones demonstrates that there are other conditions at least equally instrumental in determining the degree of permanence of different stones.

It is a well-established fact, that the same chemical substance exhibits, in different conditions, a great variation in its behaviour with chemical agents. Numerous examples might be quoted in illustration of this. Thus, marble and chalk are chemically identical, but, owing to the difference in their physical structure, the one being crystalline and the other amorphous, the former is much less readily acted upon by acids than the latter. Again, artificial peroxide of iron is readily soluble in acids; peroxide of iron, in the form of hæmatite, is attacked with difficulty by acids; and the same oxide, after exposure to a powerful heat, is almost entirely insoluble in acids. The influence of aggregation in these instances, and in numerous others which might be quoted, is obvious and generally admitted by chemists, however different and imperfect may be their views regarding the connection between physical condition and chemical effect.

The observations just made regarding the behaviour of substances, such as enter into the composition of building stones, cannot but apply with equal force to the aggregates of such components,—to the building stones themselves.

The atmospheric influences to which building stones are subject are many of them essentially chemical actions, involving processes analogous to, or identical with, those performed in the laboratory, although, from the extreme dilution of the chemical agents, as existing in the atmosphere, they must necessarily be of a very gradual character.

There are few instances in which the influence of the state of aggregation upon the permanence of a building stone is more apparent than in that of the dolomitic limestone, used in the construction of the New Houses of Parliament. Here, in one and the same block of stone, of comparatively small dimensions, we find certain portions of the surface powerfully disintegrated, while others appear in a perfectly sound condition. Chemical analysis has hitherto failed to establish any important difference in the composition of sound portions of such stones and those parts which are subject to decay; it is, therefore, legitimate to attribute the unequal permanence of the stone, under atmospheric influences, to such

of Her Majesty's Treasury, by C. Barry, Esq., H. T. De La Beche, Esq., W. Smith, Esq., and Mr. Charles Smith, with reference to the Selection of Stones for Building the New Houses of Parliament." Those by Ransome and Cooper are extracted from a paper "On the Composition of Limestones used for Building Purposes, especially on those employed in the Erection of the New Houses of Parliament," contained in vol. II., part II. (1848) of the "Memoirs of the Museum of Practical Geology."

structural differences as may be comprehended under the term, "state of aggregation."

Before proceeding to an examination of the particular character of the decay observed in the stones of the New Houses of Parliament, it may perhaps be desirable to glance at the nature of the changes to which building stones generally are subject under atmospheric influences. Under normal conditions these changes must be ascribed to the action of the oxygen, carbonic acid, nitric acid, and water in the atmosphere. In the air of towns, however, there are certain other constituents, such as several acids of sulphur, and occasionally hydrochloric acid, which cannot fail to exert an additional disintegrating influence upon building stones.

The action of oxygen must be of comparatively a subordinate character, its effects being confined to constituents which occur but rarely, and generally in limited proportions, in building stones, such as the sulphides of iron, and the protoxides of iron and manganese; these compounds, being very prone to oxidation, would tend to disintegrate the stones by the absorption of oxygen. Of far greater importance are the effects of carbonic acid and water. Carbonic acid, in the presence of water, is a powerful solvent; it not only corrodes the calcareous and magnesian carbonates (more or less powerfully according to their state of aggregation), whether they form the principal constituents of the stone or are only present as cementing materials, but is capable even of attacking and gradually decomposing the hardest and most indestructible rocks.

In the case of the calcareous and magnesian constituents of stones, carbonic acid acts by transforming the insoluble earthy carbonates into soluble bicarbonates, which are thus removed from the substance of the stone; whilst its influence on silicious rocks consists in the elimination of the alkaline bases, in the form of carbonates, and the separation of the silica in a more or less friable condition. The weathering of granites, and their gradual transformation into the several varieties of porcelain clay, affords an interesting illustration of the latter kind of action. In the changes just mentioned, the carbonic acid and water are equally concerned; the water serving not only as a vehicle for the introduction of the carbonic acid into the pores of the stone, but also as a solvent for the products of its action. There are changes, however, to which building stones are subject, in which water is the sole agent, and which are more of a mechanical than of a chemical character. The expansion which water undergoes on freezing, and the irresistible force which it then exerts, are well known; it is obvious that water freezing within the pores of a stone must exercise a disintegrating action not less powerful than those above referred to.

Recent researches have demonstrated that nitric acid is a frequent and perhaps even a normal constituent of the atmosphere, and, as such, must undoubtedly assist in the destruction of magnesian and calcareous stone; but the proportions in which this acid has been found are so minute that it need not be dwelt upon as an important destructive agent. This remark, however, does not apply to the acids referred to above, as existing in the atmosphere of towns. The quantity of sulphur-acids in the air of towns, where a considerable amount of coal is consumed, is quite appreciable. According to the determinations of Dr. Angus Smith, the air of Manchester contains an average proportion, corresponding to one part of sulphuric acid in every 100,000 parts of air, which, in the centre of the town, rises to 25 parts in 100,000. No numerical data exist with regard to the

proportion of sulphur-acids in the London atmosphere, but it can scarcely be doubted that in the neighbourhood of the New Houses of Parliament they are present to an extent equal to the average amount found in the Manchester air; they must, therefore, be regarded as amongst the more important agents destructive to stone which are present in the London atmosphere.

A few observations remain to be offered regarding the particular nature of the decay manifesting itself in some of the stones of the New Houses of Parliament. It has already been pointed out that, so far as our experience goes, we are inclined to attribute the local character of the decay to structural differences obtaining in different parts of the stone. The general structure and the composition of the stone in the New Houses of Parliament render it moreover amenable to all the sources of disintegration which we have above enumerated, with the exception perhaps of oxygen, which can scarcely produce any appreciable alteration in dolomite. Thus, the chemical action of carbonic and sulphuric acids, in combination with water, will gradually dissolve and remove the carbonates of lime and magnesia, whilst the porous nature of the stone renders it liable to the mechanical effects of water under the influence of frost. The presence of sulphuric acid in the air of towns appears, in the case of magnesian limestone, to bring into play another process of destruction. This acid not only corrodes and renders soluble, as we have pointed out, the earthy carbonates (in which respect it resembles carbonic acid in its effects), but, forming with magnesia a readily crystallisable salt, the well-known sulphate of magnesia, remarkable for the large proportion of water of crystallisation which it fixes, it gives rise, in addition, to a mechanical destruction of the stone precisely similar to that produced by freezing water. The powerful mechanical effects resulting from the solidification of water, induced by crystallisation, are well known, although it would appear that they have not hitherto been sufficiently appreciated as auxiliaries in the process of disintegration of stone. The analogy between the solidification of water, by freezing and by crystallisation, is perfectly obvious, and a French chemist has suggested, as a means of recognising stones liable to disintegration by frost, to immerse them in a solution of sulphate of soda, and to note the subsequent effects of its crystallisation within the stone.

We have ourselves recently had occasion to observe some phenomena which go far to elucidate these destructive effects of crystallisation. The exfoliations exhibited by many of the fictile vases deposited in the British Museum were found to be due to the formation and crystallisation within the substance of the vessels of nitrate of lime. Again, in experiments on the preservation of fabrics by impregnation with saline substances, it was found that the crystallisation of sulphate of magnesia within the material produced a disintegrating effect upon the fibres, sufficient greatly to weaken the material.

In conclusion, we would remark, that the effect attributed to the crystallisation of the sulphate of magnesia, in assisting the decay of dolomitic stones, and more particularly of those used in the construction of the New Houses of Parliament, is borne out by the existence of a marked efflorescence of sulphate of magnesia upon those portions of the stone where exfoliation has taken place.

We have, &c.

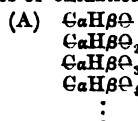
(Signed) A. W. HOFMANN.
E. FRANKLAND.
F. A. ABEL.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

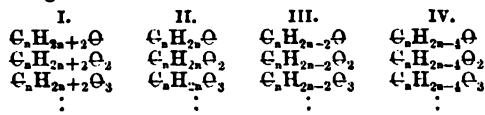
On the Classification of Organic Substances by Series,
by JAMES SCHIEL, of St. Louis.

THE progressive series which, in 1842,¹ I introduced into organic chemistry, have become the chief means of classification, and of connecting organic substances generally. Several years after the introduction of the series, Gerhardt used them in the sense just mentioned in his "Traité de Chimie," and thereby contributed a great deal toward their general knowledge.² But the idea this distinguished chemist had formed of series was very imperfect, as is proved by the single fact that he ranged benzoic acid and acetic acid in what he called an isologous series ("Traité," t. i., p. 127). As in the present state of chemical science it is highly important that the principles of seriation be clearly understood, I will try to expound them in a more general and methodical manner than has been done hitherto.

The general formula of organic compounds containing carbon, hydrogen, and oxygen, is $C_n H_{2n+2} O_\gamma$. By giving, to γ successively the values 1, 2, 3, 4, . . . we may form the following series of oxidation:—



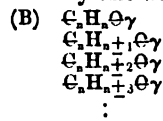
From this all the series necessary for the classification of organic substances may be derived by putting $\alpha = n$ and $\beta = 2n+2$, making β successively to decrease, the decrement being 2, 4, 6, 8, . . . By this we have the following series:—



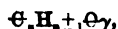
and so forth.

Every general formula in one of these generic series represents special homologous series, the members of which have the difference $n \pm H_2$. When comparing the above series with the hydrocarburets known to exist in a free state, they will be found to represent different states of oxidation of these hydrocarburets.

Another kind of series is formed by making in the general formula $\alpha = n$ and $\beta = n$ and making β successively to decrease or increase, the increment or decrement being, 1, 2, 3, 4, . . . By this we form the series:



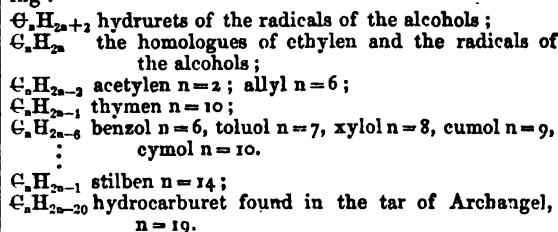
Every general formula here represents a series, the members of which are distinguished by $m \pm H$, and which I therefore call hemilogous series. It is to be remarked that a hemilogous series whose general formula is



can only exist when n is an odd number, as the number

of atoms of hydrogen entering into an organic compound is always an even number.³ The hemilogous series are very useful for comparing the physical properties of organic substances.

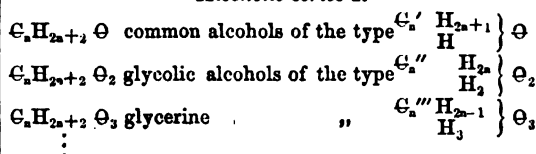
I will now apply these principles of classification to three large classes or groups of substances, the hydrocarburets, the alcohols, and the acids. The generic series formed by the first of these groups is the following:—



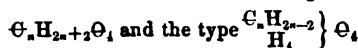
The maximum or minimum number of atoms of hydrogen which may be combined with n atoms of carbon cannot be determined at present, but there is no hydrocarburet known to contain more than $(2n+2)$ and less than $2n-(n+2)=n-2$ atoms of hydrogen to n atoms of carbon.

The alcohols form several generic series:—

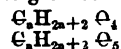
Alcoholic series I.



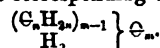
Whether tetracid alcohols of the general formula



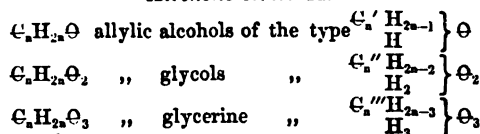
can exist, is not known, as we know nothing certain about the existence of tetrabasic radicals. The poly-ethylenic glycols of the general formulas



are bibasic alcohols corresponding to the type

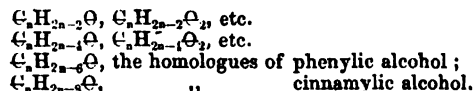


Alcoholic series II.



Of this series the only member known as yet is allylic alcohol $C_3 H_6 O$ corresponding to the general formula $C_n H_{2n} O$.

Alcoholic series III. IV. V.



¹ *Annal. Liebig and Wöhler*, July number, 1842.
² Gerhardt changed the name progressive into homologous; in the discovery itself of the series he has no share.
³ If one atom of nitrogen enters into the compound, the atoms of hydrogen are an odd number, as nitrogen is triatomic. For this reason alone the formula of quinine, for instance, could not be written $C_{10} H_{12} N O_8$, as chemists formerly used to do, but $C_{20} H_{24} N_2 O_8$, as it is written now for other reasons.

The homologous series answering the general formula $C_nH_{2n-4}O$ is

C_6H_6O	phenylic alcohol	boiling point, 184°
C_7H_8O	benzoyletic	" " 204°
$C_8H_{10}O$	xyletic	" " 220°
$C_9H_{12}O$	(?)	" " "
$C_{10}H_{14}O$	cuminic	" " 243°

as homologous with phenylic alcohol, the last member of this series, cuminic alcohol, ought to boil at 264°.

Forming a hemilogous series out of such of those alcohols as answer to the general formula $C_nH_{n+1}O$, we have:—

C_7H_8O	benzoyletic alcohol	boiling point 204°
$C_9H_{10}O$	cinnamyletic	" " 250°

The difference in composition is here $2\Theta H$, the difference in boiling point 46°; to the difference ΘH in composition there corresponds therefore a difference in the boiling points of 23°, and as to the difference ΘH_2 we mostly find a difference of 19°—20° in boiling points, the influence of the atom of H on the boiling point is in this case 3°. In a similar manner the influence of Θ on the boiling point may be shown to be 26°, as benzoyletic alcohol and cinnamyletic alcohol differ by $\Theta + \Theta H_2$, and ΘH_2 answers to 20°, we have 46°—20°=26°. The influence of the single elements on the boiling point may thus be found by seriation and general laws deduced by comparing the different results obtained by this method.

(To be continued.)

On the Estimation of Tin by Standard Solutions, by M. SCHEURER-KESTNER, of Thann.

(Continued from page 102.)

FOR estimating the oxide of tin by permanganate of potash (or, what comes to the same thing, a solution of protochloride with the addition of carbonate of soda), I made use of the ingenious method M. Péan de Saint-Gilles¹ employed while studying the oxidising action of permanganate in alkaline liquids. This method consists, as is well known, in the joint use of permanganate of potash and protochloride of iron.

In making the following experiments a slight excess of carbonate of soda was added to the protochloride of tin, then an excess of permanganate of potash; and, lastly, ferrous chloride and acid.

Thus, the following are the standard solutions:—

Sn Cl. employed. Cubic centimètres.	Quantities of water.	Observed Standard.	
		With the protochloride.	With the protochloride.
2	400	14	14
2	500	14	14
2	600	14	12
2	700	14	10
2	1000	14	8
2	1500	14	5.5
2	2000	14	5
2	3000	14	4.2
2	3000	14	4

The small degree of affinity between oxide of tin and oxygen is made evident by passing a current of oxygen in a solution of protochloride of tin with carbonate of soda in excess. A solution containing 2 cubic centimètres of protochloride to 1000 cubic centimètres of water is unaltered, even though a rapid current of oxygen is passed through it for two hours.

The oxygen of water then has no influence on

protochloride of tin, and the results obtained are very regular, which explains why M. Lessens, by using an alkaline liquid, arrived at corresponding results.

This property might be of service in estimating the oxygen dissolved in water if the oxidation of the protochloride were not so slow. It is sometimes necessary to wait very long before adding the permanganate, until all the oxygen of the water is absorbed, which is always done after some time, as may be seen by the following experiments:—

Water.	Permanganate required by the Chloride of Iron.	Permanganate required by the Protochloride of Iron.	Permanganate required by the Protochloride of Tin.
1500	24	12.5	{ After quarter of an hour 13 After half an hour . . 13

We have seen that M. Stromeyer, by a happy modification, has reduced the estimation of tin to that of iron. Applying the same principle, a salt of copper may be substituted for a salt of iron. An equivalent quantity of copper can thus be estimated in place of tin; and M. Mohr's as well as M. Terrell's² experiments show that copper can be very exactly estimated by permanganate of potash.

A double decomposition takes place on protochloride of tin being added to nitrate or chloride of copper in excess; a salt of suboxide of copper forms, and the tin passes to the maximum state of oxidation, according to the following equation:—



To estimate tin, it is, then, sufficient to transform it into protochloride, to add to it a solution of nitrate of copper, slightly in excess, before diluting it with water, and to estimate the liquid obtained by permanganate of potash.

The following experiments show that, whether the operation is performed with nitrate of copper or whether we avail ourselves of oxidation in an alkaline medium, the results are the same:—

10 c. c. SnCl in 1200 c. c. of water required	52 of permanganate
10 ,, SnCl in 1200 c. c. of water, to which is added CuO.NO ₃ , required	87 ,,
10 ,, SnCl + NaOCO ₂ + SO ₃ + FeCl required	231 ,,
Deducting that needed for the salt of iron	144 ,,

Leaves remainder . . . 87 the salt of tin.

1.117 gr. of tin, obtained by the electrical means, were dissolved in hydrochloric acid and diluted with water sufficient to make the entire volume 200 cubic centimètres. 30 cubic centimètres of this solution required 9.2 cubic centimètres of permanganate, whether the operation is performed with nitrate of copper or with an alkaline solution.

¹ *Comptes-Rendus*, vol. xli., p. 230.

² For the estimation of copper, variable quantities of water can be used without affecting the results:—

Water employed.	Permanganate required by a reduced salt of copper.
0	16.3
100	16.4
500	16.3
1000	16.3
1500	16.4
2000	16.3
2500	16.4
3000	16.4

Permanganate was standardised by means of crystallised double sulphate of iron and ammonia $\text{FeO}, \text{SO}_4 + \text{NH}_4\text{O}_2\text{SO}_4 + \text{H}_2\text{O}$.

2.918 gr. of this salt required 24 cubic centimètres of permanganate. The salt containing 14.386 per cent. of iron, the 2.918 grm. of it contain 0.416865, or, rather, 1 cubic centimètre of permanganate = 0.017268 of iron, corresponding to 0.01818 of tin ($59\text{Sn} = 2 \times 28 = 56 \text{Fe}$.)

0.2 cubic centimètres of permanganate were required by 30 cubic centimètres of the foregoing solution of tin, making 0.16725 of tin.

Thus, we have:—

Tin taken	:	:	:	0.16755
Tin found	:	:	:	0.16725

Difference = 0.00030

By calculating the equivalent of tin by these data, and by removing the iron from it, we obtain 59.105, which differs little from 59.00, the number generally adopted.

The result of these experiments is, that by passing an oxygen current through a concentrated solution of protochloride of tin, the oxygen is not fixed; in diluted solutions, on the contrary, the oxygen is rapidly absorbed by the stannous salt. Oxygen does not act on hydrated protoxide of tin. The variations noted in the strength found when protochloride of tin is oxidised by permanganate of potash are not observed in concentrated solutions; they take place only in presence of oxygen dissolved in water.

There are three different processes for estimating tin by permanganate of potash:—1. To operate with water freed from air by boiling, protecting it from access of air while cooling. 2. To oxidise protoxide of tin in an alkaline medium. 3. To decompose protochloride of tin either by a salt of iron, as proposed by M. Stromeyer, or by a salt of copper.

On Ozone, Nitrous Acid and Nitrogen, by T. STERRY HUNT, F.R.S.

THE formation of a nitrite when moist air is ozonized by means of the electric spark (the old experiment of Cavendish), or by phosphorus was shown by Rivier and de Fellenberg, who concluded that the reactions ascribed by Schönbein to ozone were due to traces of nitrous acid. The subsequent experiments of Marignac and Andrews have, however, established that ozone is really a modification of oxygen, which Houzeau has shown to be identical with the so-called nascent oxygen, which is evolved, together with ordinary oxygen, when peroxyd of barium is decomposed by sulphuric acid at ordinary temperatures. The spontaneous decomposition of a solution of permanganic acid also evolves a similar product having the characters of ozone.

Believing that the nitrous acid in the above experiments is not an accidental product of electric or catalytic action, but dependent upon the formation of active or nascent oxygen, I caused a current of air to pass through a solution of permanganate of potash mixed with sulphuric acid. The air, which had thus acquired the odour and other reactions of ozone, was then passed through a solution of potash; by which process it lost its peculiar properties, while the potash solution was found to contain a salt having the reactions of a nitrite.

As I suggested in this Journal in 1848, I conceive gaseous nitrogen to be the anhydrid amid or nitryl of nitrous acid; which in contact with water might under

certain circumstances generate nitrous acid and ammonia. From the instability of the compound of these two bodies, however, it becomes necessary to decompose one at the instant of its formation in order to isolate the other. Certain reducing agents which convert nitrous acid into ammonia may thus transform nitrogen (NN) into 2NH_3 . In this way I explain the action of nascent hydrogen in forming ammonia with atmospheric nitrogen in presence of oxydising metals and alkalies. (Zinc in presence of a heated solution of potash readily reduces nitrates and nitrites with the evolution of ammonia.)

Now, an agent which instead of attacking the nitrous acid would destroy the newly-formed ammonia, would permit us to isolate the nitrous acid. Houzeau has shown that nascent oxygen is such an agent, at once oxydising ammonia with formation of nitrate (nitrite?) of ammonia, and thus when ozone (nascent oxygen) is brought in contact with moist air both of the atoms of nitrogen in the nitryl (NN) appear in the oxydised state.

From this view it follows that the odour and most of the reactions ascribed to ozone are due to nitrous acid which is liberated by the decomposition of atmospheric nitrogen in presence of water and nascent oxygen. We have thus a key to a new theory of nitrification and an explanation of the experiments of Cloez on the slow formation of nitrite by the action of air exempt from ammonia upon porous bodies moistened with alkaline solutions. I hope soon to send the results of my farther inquiries in this matter.—*American Journal of Science*, Vol. xxxii., No. 94.

On the Formation of Picramic Acid, by M. CAREY LEA.

IT is to M. Aimé Girard that we owe the first isolation of picramic acid, and correct determination of its constitution. His views, however, of the circumstances under which it is formed do not altogether agree with the results of my observations, and I advert to them now, because his second paper contains a criticism on the results obtained by another chemist, which criticism I think depends upon an inexact view of the reactions.

In a paper published by Dr. Evan Pugh, in Liebig's "Annalen," he endeavoured to establish the identity of picramic acid with Wöhler's nitrobenzamic acid. M. Girard, while he agrees with Mr. Pugh in his conclusions, rejects his experiments and reasonings as insufficient.

"This chemist, in fact," he says, "proceeded exactly as Mr. Wöhler had done before I demonstrated the formation of picramic acid by means of sulphydric acid. His process consists in mixing picric acid with proto-sulphate of iron, boiling with excess of baryta, precipitating the soluble baryta salt with ammoniacal acetate of lead, and finally in decomposing the lead salt by sulphydric acid. Now it is evident that under these circumstances, even supposing that the protoxide of iron had not converted the picric acid to picramic, the sulphydric acid alone would have produced this reduction."

At first sight this conclusion seems perfectly legitimate, so much so, that it is probable that no test by experiment was thought necessary. Had such been made it would have been ascertained that sulphydric acid is wholly without power to reduce picric acid whether free or in combination with lead, to picramic acid.

The fact appears to have been overlooked that sulphydric acid is quite incapable of producing this reaction either upon picric acid, or, as far as my experiments go, upon any picrate. It is the alkaline sulphides that possess this power, and not free sulphydric acid, a circumstance which has not been noticed, in consequence of chemists operating on solutions of picric acid in alcohol to which excess of ammonia had been added. When sulphydric acid is passed through such a solution, sulphhydrate of ammonia is formed and acts on the picric acid.

Sulphydric acid may be passed through solutions of picric acid, picrate of potash or picrate of ammonia, either at ordinary temperatures, or at a boiling heat, for any length of time, without producing the slightest effect. On the addition of excess of alkali the reducing effect of the sulphide formed is immediate.—*American Journal of Science and Art.*

On the Presence of Nitrates in Mineral Waters, and on the Production of Saltpetre, by R. HAINES, M.B., Professor of Materia Medica, Grant Medical College, Bombay.

(Continued from page 166.)

In confirmation of the above view, I will mention the result of an analysis of a nitre-yielding earth, which was sent me from Patree, in Guzerat,—the only substance of the kind I have had the opportunity of examining. It is said to occur in a space of limited breadth, extending from East to West, and not elevated. It was not a general sample of the soil, but the rich surface sweepings which contained the saline effluence. This earth contained the large proportion of 26.207 per cent. of soluble salts, the per-centage composition of which was as follows:—

Chloride of sodium	56.507
Nitrate of soda	24.126
Nitrate of potassa	8.631
Sulphate of magnesia	5.284
Sulphate of lime	5.452

100.000

Here it will be observed that although the total nitrates amounted to nearly 33 per cent. of the soluble matters, yet the nitrate of potassa was little more than a fourth of this, the greater part of the nitric acid being in combination with soda; and the disproportion is even greater than appears at first sight, as from the lower equivalent of soda, the nitrate of soda contains, proportionally, more nitric acid than that of potassa; the 24.126 parts of the former containing 15.327, the 8.63 of the latter but 4.615 of nitric acid. By the ordinary mode of crystallisation, the greater part of the nitrate of soda would remain in the mother-liquor, and be thrown away. The remedy is simple enough in theory,—viz., to add potash salts, or wood ashes, which always contain them, whence, by double decomposition, nitrate of potassa and a soluble salt of soda will be formed. I recommended a trial of this plan, but I have not heard anything of the result. It is the general practice in preparing saltpetre by the artificial method in Europe to add carbonate of potassa to the crude solution as long as a precipitate of carbonates of lime and magnesia is formed, or, what comes to the same thing, until the liquor acquires an alkaline reaction. The liquid contains the nitrates chiefly of these bases, and the cessation of the precipitation

marks the term of the decomposition; but when nitrate of soda is to be decomposed by these means, the manufacturer has no such guide to assist him. No precipitation occurs, and the first addition of potash renders the liquid alkaline, so that he must be content with noting the specific gravity of his crude liquor, and adding as much potassa as experience shall show him to be the minimum quantity requisite to increase the yield of nitre to the utmost. The most economical plan would appear to me to be, to add a portion of wood ashes to the earth under lixiviation; and then, when all the saltpetre has crystallised out in the ordinary way, to stir into the mother-liquors sufficient wood ashes to make the mass nearly solid, and to dry it with a pretty strong heat. The nitrates would thus be fully preserved in a portable, and not deliquescent form, and the extraction of the salts could be performed elsewhere; while, even if an excess of ashes were used, their potash salts could be made available by the manufacturer, and would be worth the carriage. I regret not having paid attention at the time to the insoluble matters. I have searched for recorded analyses of a similar kind, but, to my surprise, I have been only able to find two old ones, by Dr. John Davy. One of the surface earth of the saltpetre-producing cavern of Memoona, in Ceylon, the other of the saltpetre earth of Tirhoot, in Upper Bengal,¹ and one by Mr. Stevenson, also, of saltpetre earth from the Tirhoot district,² the latter being an average sample of the ordinary soil, while that analysed by Dr. Davy was from the rich surface collections.

	Analysis of earth.				100 parts of soluble salts consisted of		
	Memoona (Davy)	Tirhoot.		Memoona.	Tirhoot.		
		Davy.	Stevenson.		Davy.	Stevenson.	
Nitrate of Potassa	2.4	8.3	0.7	72.7	63.8	12.3	
" Magnesia	0.7	..	..	21.2	..	..	
" Lime	..	3.7	0.9	..	28.5	15.8	
Sulphate of Soda	..	..	2.7	..	..	47.4	
" Magnesia	0.2	..	..	6.1	..	..	
" Lime	..	0.8	..	..	6.1	..	
Chloride of Sodium	..	0.2	1.4	..	1.5	24.5	
Carbonate of Lime	26.5	35.0	44.3	..	..	..	
Water and organic matter	9.4	12.0	..	..	..	..	
Matter insoluble in nitric acid	60.8	40.0	50.0	..	..	..	
Total	100.0	100.0	100.0	100.0	100.0	100.0	

Lavoisier states that different samples of natural saltpetre in France yielded him, after treatment with potashes, from $1\frac{1}{4}$ to $5\frac{3}{8}$ per cent. of nitrate of potassa. Ragsky lately examined numerous samples of saltpetre earth (surface-sweepings) from Hungary, and found from 0.50 to 2.33 per cent. of nitrate of potassa. (In Hungary, an earth which yields 0.26 per cent. only, is said to be rich.) The most remarkable thing in Davy's analyses, given above, is the almost total absence of chloride of sodium, and, indeed, of all soda salts whatever. Another notable circumstance is, that the richest earths of France and Hungary cannot be compared for productiveness with the random samples of Indian earths.

On referring to Mr. Stevenson's paper, I find that it partly answers one of the questions that occurred to me, or at least gives some assistance towards answering it;

¹ Knapp's "Chemical Technology," i. 347.

² "Journal of Asiatic Society of Bengal," ii. (1833) 23.—"On the Manufacture of Saltpetre, as practised by the Natives of Tirhoot. By Mr. J. Stevenson, Superintendent Hon. Co.'s Saltpetre Factories in Behar."

and the statements are so much to the purpose, that perhaps I may be excused for quoting them:—

"The soil of Tirhoot almost everywhere contains a large proportion of saline matter, such as nitrate of potassa (saltpetre), nitrate of lime, sulphate and muriate of soda, &c.,³ but in general, the muriate of soda is most abundant. The saltpetre (as well as the other salts) lies in patches, as it were, some parts being more productive than others, according as carbonate of lime or sand alternately predominate. By analysing the different soils, I have found those places most productive of nitre to contain a redundancy of the former; and, on the contrary, where the soil was unproductive, I found a redundancy of the latter substance. I am, therefore, naturally led to the conclusion that carbonate of lime is one of the principal agents in the production of this article. This will also account for the district of Tirhoot being more productive of nitre than any other place in India, for almost half of its soil is calcareous."

Mr. Stevenson's observations, therefore, remarkably corroborate the views of Kuhlmann and others, as to the agency of carbonate of lime in nitrification; but, may we ask, is limestone essential? Cannot the nitric acid be formed and combine directly with potassa or soda, as well as by the indirect process suggested by Kuhlmann? Is there in potassa any peculiar property or affinity which makes the combination of nitric acid with it more frequently observed than that with soda? or is it merely that the collateral agencies which determine nitrification happen, by some combination of circumstances, to be more frequently present when there is potassa in the soil than when there is soda? or is the fact simply that nitrate of soda, when it does occur, is often overlooked altogether? Is limestone absent from the soils which yield *sujee muttee* or native carbonate of soda?⁴ Is nitrate of soda never found in this soil, either when it is calcareous or when not calcareous? What is the source of the carbonate of soda itself? Could a solution of potassa or soda be substituted for milk of lime in Collard de Martigny's experiments with equal success?

With these queries I conclude, hoping to call the attention of those interested in developing the resources of India to these points, which are not only of scientific interest, but also (I think) of no little practical utility.—*Transactions of the Medical and Physical Society of Bombay.*

On the Chemical Composition of Steel.

DR. CRACE CALVERT read a short paper on this subject at the meeting of the British Association. He said it was only of late that chemists had attempted to investigate the composition of steel, though it was one of the highest importance to manufacturers. Science, however, had not rendered the service that was anticipated. The nature and properties of steel were so different to iron, that chemists had not progressed in regard to its composition as might have been expected. There was a very warm discussion going on now between chemists on the

Continent, one party contending that nitrogen was an essential agent in converting iron into steel, and the other party that nitrogen was not at all necessary, and that carbon was alone essential. He (Professor Calvert) had made one observation which he thought would interest the chemists assembled in Manchester. For the last two or three years he had devoted considerable attention to the carbon existing in cast iron. In treating cast iron with a weak acid he had succeeded in removing 75 per cent. of the metal, leaving a light graphitic mass, composed of silicium, carbon, and nitrogen. What was exceedingly interesting was that this cast iron contained 1 per cent. of nitrogen, a body which chemists did not actually know existed in it, though its presence was suspected. It occurred to him that if a weak acid enabled him to get at this carbon in cast iron, it would be interesting to see what carbon there was in steel, and possibly he might come to the point in discussion amongst the Continental chemists. In making his experiments he came to this interesting fact, that the different properties which steel acquired by tempering was not a mere molecular change, but a complete chemical change took place during the moment. He took two pieces of the same steel, one soft and the other hardened. Both were placed at the same time in weak acetic acid. The hard steel now was nearly dissolved, whilst the soft steel remained nearly in the same state as when put in. The hard steel was covered with carbon, having the appearance of lamp-black; whilst on the soft steel the carburet of iron was perfectly grey, and was composed of carbon, iron, and perhaps nitrogen. He hoped, with future experiments, to be able to throw more light on this subject.

On the Estimation of Nitrogen, and on an Acidimetric Process, by M. CAREY LEA.

IN a late number of the CHEMICAL NEWS a modification of Will and Varrentrapp's process for the estimation of nitrogen is proposed by Mr. J. Walker. He decomposes the substance in a combustion tube in the ordinary way, but instead of conducting the products of the combustion into chlorhydric acid, conducts them into solution of chloride of zinc, and in place of determining the ammonia in the usual manner, determines the amount of oxide of zinc precipitated by it. "This process," the author observes, "has been practised by me for the last two years, and gives most accurate results, and I can with perfect confidence recommend it."

Few chemists would be apt to adopt a process so obviously worthless, but as its author recommends it especially for the analysis of manures, and as in that way it might be employed for technical purposes, it seems desirable that its gross inaccuracy should be pointed out. Oxide of zinc is well known to be easily soluble in ammonia salts; if a drop of caustic ammonia is let to fall into a solution of pure neutral chloride of zinc, it occasions a precipitate of oxide; if now solution of sal-ammoniac be added, the precipitate redissolves. Obviously, if more ammonia instead of sal-ammoniac is added, sal-ammoniac is formed by the decomposition of the zinc salt, and a portion of the precipitate is redissolved by it, or, to speak more correctly, a portion of the oxide of zinc which is liberated by the ammonia is held in solution by the sal-ammoniac. The necessary result must be an under-estimation of the ammonia, leading to

³ "I have not been able to ascertain whether the *sujee muttee* (native carbonate of soda) is found in this district; as far as my own observations have extended, it does not form a part of the composition of the soil. I also could not detect any alumina, though it is likely some parts may contain it."

⁴ In the Madras Presidency limestone does accompany the native carbonate of soda.—"A thick bed of lime frequently occurs at the depth of six or eight feet from the surface, covered by the dhoby's earth. The natives appear to be aware of this, and frequently dig in such localities in search of lime."—Hunter's "Indian Journal of Arts," &c., i. 432.

errors of such magnitude as would render the process wholly unfit even for the roughest technical process.

In consequence of overlooking the frequent solubility in alkaline solutions of certain metallic oxides and basic salts insoluble in water, even chemists of experience have been led into singular errors. Dr. Friedrich Mohr, in his interesting book on "Volumetric Analysis," highly recommends a process proposed by Kieffer for acidimetric estimation, and which is briefly as follows:—Sulphate of copper in solution is treated with liquid ammonia until the precipitate formed is nearly re-dissolved; it is then filtered, diluted to a certain strength, and constitutes the test liquid. If this be dropped into an acid, or an acid solution, after the addition of a certain quantity a precipitate falls, the appearance of which Kieffer and Dr. Mohr consider to mark the moment of the exact saturation of the acid liquid by the joint action of the ammonia and copper. Dr. Mohr observes:—

"Il n'est le moindre excès de la liqueur de dosage formera un précipité bien net, tout à fait insoluble dans la dissolution des deux sels complètement neutres."²

Here is an important error, this precipitate is by no means insoluble in such solutions, it is soluble with considerable facility in solution of sulphate of ammonia (in the above passage Dr. Mohr refers more particularly to the dosing of sulphuric acid), and also, though to, perhaps, not so great an extent, in solution of sal-ammoniac and of nitrate of ammonia. Consequently, when the precipitate appears, it is because the solution is saturated with it. If it were not that the effect of the cupro-ammoniacal solution is directed to be determined empirically and not by calculation, this solubility would have led to discrepancies no doubt sufficiently great to have awakened suspicion. But the error is just sufficiently great to make the process dangerous. More or less of the precipitate is held in solution according as the saline solution is more or less dilute. Consequently, if the liquid to be analysed contains no saline matter, the error may be but trifling, the error in the analysis correcting more or less accurately the original error in the determination of the "titre." This mode of estimation is particularly recommended for determining the quantity of free acid in acid saline solutions; but here is just where it would be most fallible, the nature and quantity of the saline substance in solution exerting a powerful influence on the solubility of the precipitate. It is evident that a solution containing five grammes of free sulphuric acid with five grammes of sulphate of ammonia, tested in this way, would give quite a different result from one in which five grammes of free acid was contained with twenty or fifty of sulphate of ammonia. Likewise its results when used in the "method of residues" must be false. Accordingly, in an experimental trial by Dr. Mohr³ for the determination of magnesia by supersaturation, with a known quantity of sulphuric acid, and determination of the residue of uncombined acid, the result which he obtained is erroneous by four per cent. Four per cent. too little of magnesia were obtained, because the free sulphuric acid was over-estimated, the ammonio-cupric solution having been added till a precipitate was obtained, which, as we have just seen, does not fall till after the saturation point has been reached.

The precipitate which appears in all these cases is

spoken of by Dr. Mohr as hydrated oxide of copper. It appears to be basic (probably quadribasic) sulphate.

In making these observations on a single process of Dr. Mohr, I wish, at the same time, to bear testimony as to the candour with which he has given the results of his experimental trials. The proposal of a mode of analysis such as that of Mr. Walker is a new proof how much it is to be regretted that any chemist should offer a new mode of analysis without having first controlled it by the analysis of one or more specimens of substance of a known composition. In this way MM. Glénard and Guillemond have recently proposed⁴ a method of estimating the quinine in barks. It has been, however, shown,⁵ that not only the presence of cinchonine destroyed the accuracy of these results, but that, owing to a mistake of the authors as to the reaction of the sulphates of quinine upon tincture of logwood, the process when performed with pure quinine gave only one-half the true result.

In connection with the foregoing observations on zinc I may remark, that an error has crept into the description of zinc reactions in Gmelin's "Handbook." It is there stated that zinc salts give precipitates with ferrocyanide and ferridcyanide of potassium, both of which are soluble in chlorhydric acid.⁶ As respects the precipitate with ferrocyanide, this is certainly erroneous, the precipitate does not redissolve in that acid.—*American Journal of Science.*

On some New Aniline Colours,

by W. CROSSLEY, Analytical Chemist, Middlesborough.

If aniline be mixed with bisulphide of carbon, and allowed to stand for some days, sulphuretted hydrogen is evolved and a cream-coloured, fatty-looking body left. If a little fuming nitric acid be carefully added to this, a beautiful crimson-coloured substance, along with a dirty-brown body, will be produced. I have found no good method for separating the two. But if a portion is first dissolved by boiling in alcohol, and then fuming nitric acid carefully added, drop by drop, a crimson-coloured fluid is produced, which dyes either woollen, silk, or cotton, although they take it more readily if it be first slightly acidified with sulphuric acid. Again, by dissolving the above-mentioned fatty substance in benzole and treating with nitric acid, a fluid is produced which may be used for dyeing lemon-colour. I ought to mention that if either the solution in alcohol or benzole be allowed to cool before the nitric acid is added, a large quantity of clear crystals will separate.

None of the above colours rival, or even equal, those already known, but the reactions are interesting, and I think them worthy that attention which I, from press of other business, am unable to give.

Chemical Prize.—A Gold Medal, worth 600 francs (23l. 16s.), is offered by the Utrecht Society of Sciences, for a memoir on *The Rigorous Determination of the Atomic Weight of at least 100 Elements among those on which M. Stas has not yet published Researches.* The memoir must be sent before November 30, 1862, to Dr. J. W. Gunning, the Secretary of the Society, Utrecht.

² Not having a German copy at hand, I quote from the French translation made by Forthomme under the superintendence of Dr. Mohr.

³ Forthomme's translation, p. 409.

⁴ *Op. cit.*, p. 417.

⁵ "Rep. de Chimie Appliquée," 1859, p. 131.

⁶ *Idem*, 1860, p. 61.

⁷ *Handbook*, Vol. v., p. 12, Careyless Edition.

TECHNICAL CHEMISTRY.

Notes on the Naphthaline Colours, by CHARLES B. C. TICHBORNE.

As the investigations of M. Z. Roussin are now attracting some attention, I may be excused for mentioning some peculiarities observed by myself during a repetition of his experiments, particularly as there has been very little published as yet regarding the manipulatory minutiae. Presuming, therefore, that the readers are already acquainted with the general *modus operandi* of his "artificial alizarine" experiments, I wish to point out some few difficulties which strike me on looking back at a *resumé* of my working, the overcoming of which are imperatively necessary to ensure success.

M. Roussin directs the binitronaphthaline and sulphuric acid to be mixed together, and then to be heated to 392° F. Now, as this is a great temperature, and generally requires some time to raise it to the requisite point when operating on a large scale, I think it preferable to heat the sulphuric acid nearly to 400° F. before adding the naphthalic compound, by which means a considerable loss of the latter substance is avoided. If this plan be not adopted, the temperature should be raised as quickly as possible to 392° F., but, as will be shown, on no account should the reducing agent be added until that point be reached. In the original memoir we are told to add the reducing bodies in successive portions, at a time when the menstruum has arrived at 392° F.; upon proper attention to this part of the process the entire success of the operation depends. The colouring matter is, in a measure, slowly formed at a lower temperature than that specified, so that on adding a quantity of the reducing agent it will under such circumstances produce a considerable amount; but from the chemical action sufficient heat is invariably generated after some time to bring it much above the stipulated point; when such is the case, a violent action sets in which is completely beyond control.

When the mixture is at 400° F., and not before, it should be taken off the heating medium, and the reducing agent in a state of fine division added in small proportions at a time, the whole being constantly stirred. A little heat may be again employed if the thermometer seems inclined to fall. By attention to these injunctions the action sets in at once on the first addition, and proceeds in a quiet and uniform manner; the heat generated (although being expended in a great measure) is generally sufficient to keep up the temperature.

The question was mooted at one of the late meetings of the British Association at Manchester, whether colouring matters could be procured from naphthaline at all, and whether such matters were not aniline products produced from the use of an impure hydro-carbon; and I believe, although I had not the pleasure of attending those meetings, that the statement was made that the colours could not be made from pure naphthaline. That this is a mistake is evident from the composition of M. Roussin's so-called alizarine, and because I have succeeded equally well with the chemically pure naphthaline. To procure this compound I took the ordinary article and re-crystallised it from an alcoholic solution acidulated with acetic acid. These crystals, on being re-dissolved in alcohol, were precipitated with distilled water and thoroughly washed. When boiled with acidulated water, which was afterwards concentrated and tested, it was found perfectly free from aniline.

When dry I projected it, a small portion at a time, into 4 times its weight of pure nitric acid (specific gravity 1.500); this gave about 1½ part of pure binitronaphthaline. The mother liquors which first drained from the crystals gave a precipitate on dilution with water, which, however, seemed to contain other compounds. On the further treatment with sulphuric acid and tin, the so-called artificial alizarine was procured in considerable quantities. The direct extraction of this colouring substance may be brought about in the following simple manner:—After boiling some minutes with water, it should be as nearly neutralised as possible with a solution of potash, and the resulting precipitate thrown upon a calico filter; as soon as it is thoroughly drained, the dye may be extracted from the black mass by hot methylated spirit, to the amount of about 60 to 70 per cent.

M. Dumas, in bringing M. Roussin's communication before the French Academy, said, that as the author had not at present given an analysis of the new product, its identity with alizarine was not yet proved, and I am not aware that one has been since published. I cannot myself supply the void, at present I have not succeeded in isolating it in a sufficient state of purity for analysis, but I have no hesitation in saying that it is destitute of nitrogen, and therefore can have no connection with any of the aniline colours. *Vide* the different analyses of those substances already published.¹ This can be proved by taking a solution of the colouring matter in potash, and evaporating to dryness on a water-bath to expel the least trace of ammonia, and then testing by Lassaigne's method—*id est*, by fusing with potassium, &c. The result is—no indication of the presence of nitrogen. But when this experiment is tried, and a small quantity of mauve or fuchsine added, the presence of Prussian blue is immediately exhibited.

I found that when binitronaphthaline was intimately mixed with *Hepar sulphuris*, and the mass slightly moistened with water, the whole of the naphthalic substance was converted into a most brilliant colour on the application of heat; but on going the following morning to examine this apparently cheap and easy method of procuring a splendid dye, I found my bird had flown, and the colour entirely faded.

On further investigation I found that this was due to the presence of the alkaline hyposulphite,—the hyposulphites instantly bleaching the naphthaline colours; but by subjecting the *Hepar sulphuris* to great heat, and so converting it entirely into penta-sulphides and sulphate, the difficulty is nearly overcome; and I have great hopes that this process may be practically turned to account. But the purest red, I got in an experiment in which I tried the direct reducing action of protoxide of iron upon binitronaphthaline,—*id est*, by treating the latter substance with proto-sulphate of iron and an excess of potash.

As M. Roussin states, a blue colour is easily procured by the action of cyanide of potassium upon binitronaphthaline, but it is not a very pure blue, and presents a slight greenish shade. This substance dyes a brilliant green on the addition of a small quantity of dinitraniline. Dinitraniline acid and the other yellow tar products are not applicable.

There have been few technical discoveries within the last few years which have created so much excitement amongst the *savants* and the public at large as the aniline dyes, not only from the scientific interest attached

¹ Béchamp's fuchsine $C_{12}H_{11}N_2O$. Phenamine (the name given to aniline violet by Scheurer-Kestner) $C_{10}H_{14}N_2O_2$.

to such disclosures, but also, as the President of the British Association intimated in his inaugural speech this year, because no doubt, in time to come, instead of being dependent upon other countries for our supply of dyeing materials, we should become an exporting nation to supply the wants of other kingdoms,² the advantages being, not only independence of foreign importation, but the opening of entirely new branches of trade, in which branches our old acquaintance King Coal is still our friend, and gives us preeminent standing in working this new mine of wealth.

If aniline opened such a field before us, how much more bright must be the prospect that the naphthalic colours present. The price of aniline being still very high, in spite of all the attention devoted to its cheap production. Nor is there the least probability of a cheaper result. But here is naphthaline, at one time the curse of the tar distillers, become an article of vast importance, although formerly it was frequently the case that they were compelled to bury this unprofitable acquaintance.

Laboratory, Apothecaries' Hall of Ireland.

PHARMACY, TOXICOLOGY, &c.

On the Active Principle of Cornus Florida,
by JOHN M. MAISCHE.

No complete analysis has as yet been made of the barks of the various species of *cornus*. In Vol. vii. p. 109, of the *American Journal of Pharmacy*, Mr. James Cockburn details the results of a number of experiments made with the bark of *cornus Florida*, which are not without interest, but leave us in the dark about the nature of the true active principle of this indigenous tree. This is ascribed by him and by other investigators to "bitter extractive," a name which has to cover a multitude of compounds, difficult to isolate.

In compliance with the subject accepted by me for investigation for the American Pharmaceutical Association, I have directed my efforts principally to the isolation of the active part of this bark, without taking much notice of the other principles it contains; but it was necessary first to obtain some information about these other constituents.

I have therefore repeated and variously modified some of the experiments made by Mr. Cockburn, and find them mainly correct; I was unable, however, to obtain the crystalline substance of which he speaks, and for which I did not search any farther, as these crystals are stated to be entirely tasteless, and my main object lay in a wholly different direction.

Alcohol and water both extract completely the bitter taste of the bark of *cornus florida*, leaving a tasteless residue which imparts no taste to a diluted acid. It is therefore unnecessary, for the extraction of the bitter principle, to contaminate the exhausting liquid with a foreign acid, which would serve no other purpose except to require to be removed again. By extracting the bark with alcohol, the tannin, resin, some waxy matter, free acid, &c., are taken up and interfere with the purification of the extract. Water dissolves no resin, wax, or fatty matter, but on the other hand takes up some gum which may be easily got rid of. Alcohol and water dissolve the whole of the colouring matter.

The neutralisation of the free acid, contained in the

infusion and tincture, does not visibly affect the appearance of the liquid; an excess of alkali deepens the colour somewhat, without otherwise disturbing it; alkalies cause no precipitate of the bitter principle. It was to be inferred, inasmuch as most alkalies are but sparingly soluble in water, that the bitter principle was not of an alkaline nature. If it is of neutral behaviour, the aim, it appeared, must be to remove the other substances so as to retain the bitter active body in solution; or else, take it up by a solvent, which leaves the other bodies behind.

Without attempting to enter into detail on the various experiments made for this purpose, I may as well state the result of my investigations, giving some explanations afterwards.

I have succeeded in obtaining the bitter principle of the bark of *cornus florida* in a perfectly colourless and neutral solution; but all attempts to evaporate the liquid without decomposition have signally failed. No matter whether the alcohol or the aqueous colourless solution is evaporated by rapid boiling, at a moderate heat, at ordinary temperature, in a current of dry air or over sulphuric acid under a bell glass, in all cases a brownish colour is produced, which is deepened as the liquor is concentrated, and a dark brown extract is left behind without the least sign of crystallization.

This colourless solution of the active principle may be obtained in various ways; I have found the following to be the least troublesome:—

Coarsely powdered dogwood bark is twice boiled with water, and the expressed clear liquid is precipitated by solution of subacetate of lead; a flesh-coloured curdy precipitate takes place, which, on stirring becomes very voluminous, and now contains the tannic and gallic acids, gum, colouring matter, &c. The liquid, which is filtered off from the precipitate, is destitute of colour, and contains the bitter principle, together with free acetic acid, and an excess of the precipitant. The excess of lead may be removed in various ways, either by a little sulphuric acid, or by sulphuretted hydrogen; if sulphuret of ammonia, or an alkaline carbonate is used, the alkali serves to neutralise the free acid; the former precipitants require the subsequent use of an alkali to saturate the acid.

Now, in whichever way the lead has been removed and the acid neutralised, the solution remains colourless and neutral for some time at ordinary temperature; but if it continues in contact with the air, and is allowed to evaporate, a decomposition takes place and an extract remains behind, which is still of a neutral reaction, if for the neutralisation of the free acetic acid a fixed alkali has been employed; if, however, ammonia has been used, the extract will have a slight acid reaction from the formation of the acid acetate of ammonia, and the expulsion of some of this volatile base. When evaporated to a syrupy consistence, and now mixed with strong alcohol, the whole liquid is redissolved, with the exception of a white crystalline precipitate of alkaline acetates, and sulphates. All the colouring matter goes into solution, and on evaporation a bitter extractive mass remains, which is wholly soluble in water and alcohol. Its solution is again precipitated and decolorised by subacetate of lead, and the filtrate has now the same behaviour as in the first instance.

By digesting the decoction with carbonate of lead, litharge, or hydrated oxide of lead, the tannic acid of the dogwood bark is precipitated, but at the same time the liquid is rendered turbid by keeping some lead in suspension, which is not removed by filtration; as it

² "It is more than probable that England, instead of drawing her dye stuffs from foreign countries, may herself become the centre from which all the world will be supplied."—*Mr. Fairbairn's Speech*.

passes the filter the liquid is still turbid, and to render it clear, requires to be treated with sulphuric acid or sulphuretted hydrogen, in order to precipitate the lead, which at the same time throws down a considerable amount of colouring matter, but leaves the filtrate still with a decidedly brown colour, which cannot be removed in this way.

A similar result is obtained if the removal of the tannin is attempted by precipitating it with gelatine; the whole amount is not very readily thrown down, and the filtrate has retained a good part of its colouring matter.

More completely than by the latter two ways will the decoction be decolorised by first dissolving in it some alum, and precipitating it again by one of the alkalies, or their carbonates. The alumina which precipitates, will also carry down all colouring matter and tannin, so as to yield a clear colourless filtrate, which, besides the excess of the saline additions, contains the whole of the bitter principle. If this solution is now evaporated, the same change takes place with the organic matter; it turns dark; from the syrupy residue, the saline admixtures are thrown down by strong alcohol, but the colouring matter is dissolved, and, on evaporation, a bitter extract is left behind, without any traces of crystals. The extract, dissolved in water, and treated with alum and an alkali as before mentioned, again yields an almost colourless solution, which shows the same behaviour as before.

In the above experiments, after the bitter principle had been repeatedly deodorised and evaporated, the precipitates of lead or alumina would not carry down the whole of the colouring matter, but leave the filtrate of a yellowish colour, which grew deeper with a repetition of the experiment; the bitter taste of the resulting tincture and extract was now perceptibly less marked, and had nearly entirely disappeared in two specimens of extract, which had been thus treated, and afterwards kept for six months exposed to the air.

I have mentioned above in which various ways I attempted to evaporate the clear solution, so as to prevent change, and it is unnecessary to dwell more upon this matter, as the result has been nearly the same, the chief difference being a somewhat higher or less degree of colour.

A number of experiments were undertaken with the view of obtaining a precipitate from the colourless solution, which should contain the bitter principle; but all attempts have so far been fruitless. It was thought that an ethereal solution might be less likely to become coloured, as its evaporation might be carried on at a very low temperature, and the ether itself would act as a deoxidizer. But no such solution could be effected; ether would not take up the bitter principle from water or alcohol, nor would it cause its precipitation from the spirituous solution.

The presence of free alkalies or acids, during evaporation, appears to hasten the change of the bitter principle; the solution resumes a colour of a marked deeper tint, and the peculiar odour exhaled from a decoction of dogwood bark, seems to be stronger.

If I may be justified to deduce any inferences from the results of the experiments as stated above, they would be as follows:—

The bitter principle of the bark of *cornus Florida* is a colourless, neuter, persistently bitter substance, which is very soluble in water and alcohol, insoluble in ether, not precipitated by chemical re-agents, but very easily affected and destroyed by exposure to the air.

This at the same time furnishes an answer to the other question propounded to me; my inability to obtain the active principle in a pure state rendered a comparison with quinia impossible.

My experiments imply some useful hints regarding the mode of making pharmaceutical preparations of dogwood bark. Contact with the air and exposure to heat, it would appear, must be avoided; a solid extract is best evaporated in vacuo; in preparing a fluid extract, the liquid ought to be brought under the protective influence of sugar as soon as possible. For such preparations, water is a better exhausting menstruum than alcohol, which would take up inert resinous matter; this, in its pure state, is almost white, crummy, soluble in alcohol and ether, also, in alkalies; destroyed upon platina foil without fusion, leaving much charcoal; it is tasteless, and apparently without influence on the human body.

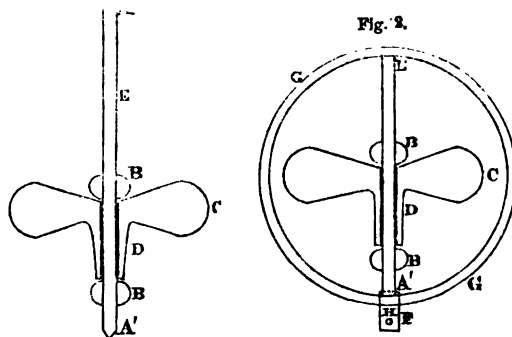
Although having failed so far in my attempts to isolate the bitter principle of *cornus florida* in a crystalline state, I intend to institute further researches upon this subject, the results of which I may probably lay before the Association, if they should prove to be new and more satisfactory. The isolated principle will, no doubt, be interesting, but, I suppose, of little practical value, if we are to judge by the above properties.

I may remark yet, that I have also somewhat experimented with the bark of *cornus sericea*, but with no better result.

PHYSICAL SCIENCE.

How to make a Gyroscope.

A simple Gyroscope may readily be made from a "patent metal top." Fig. 1 represents a section of one of these



latter: it consists of a heavy wheel, C D, revolving round a rod, A A', and kept in its place by projecting pieces of metal, B B'. Motion is given to C D by rapidly unwinding string from D, and if the top be now placed on the ground, the revolution of C D carries round the rod A A', and a continuous spinning ensues.

In order to make a gyroscope from such a top, we divide A A' at E (Fig. 1); a piece of copper wire, about one-sixth of an inch in diameter, is next formed into a circle, G G' (Fig. 2), the diameter of which is equal to the distance between the opposite ends A' and E, which are soldered into it. At the point where A' touches G G', a small piece of sheet copper, F, is fixed; to this a double swivel (such as is used for pike fishing) is attached by means of a piece of string passing through the hole H.

When C D is in rapid motion, the swivel is suspended from a piece of stout wire, and becomes a centre round which the whole may revolve.

This gyroscope costs about 2s.; it works well, although, as far as appearance is concerned, it will not bear comparison with its more elegant brethren, which can boast a finely-turned copper wheel, with a steel axis smoothly revolving in a brass ring.

NOTICES OF BOOKS.

Report of the Committee on the Decay of the Stone of the New Palace at Westminster.

SECOND NOTICE.

IN continuation of the reports which have already been presented *in extenso* to the notice of our readers, we purpose now to enter upon a critical examination of the several projects which have been submitted for the consideration of the "Decay of Stone Committee." Many of the propositions appear to have been at once rejected, on the ground of their being altogether inapplicable to the wants of the case; in other instances the proposals could not be entertained on account of the over-anxiety displayed on the part of the inventors, lest they should betray the secret of their composition, or by objecting to the announcement of the data upon which alone a sound judgment could be based. Thus it happens that among the great number of processes suggested, only a dozen or so appear, up to the present time, to have been selected for further examination; and even these few enumerate more than one example in which the efficacy of the mode of treatment will have to be determined solely by the inspection of treated surfaces, and without the assistance likely to be derived from a full knowledge of the chemical properties of the materials actually employed.

The witness first examined by the Committee was Mr. Nicholas Charles Szerelmey, who has gained for himself some celebrity by the successful manner in which he has hitherto preserved his secret, and who still declines to communicate any more than the barest information respecting the nature of the process employed by him in coating certain external portions of the Palace at Westminster, which, from their early showing signs of decay, were put under the treatment of Mr. Szerelmey as far back as the spring of 1857. The evidence elicited during this gentleman's examination is of the most conflicting character; at one time it is affirmed that no oily nor bituminous matters had been used in the preparation of the stone, but shortly afterwards the employment of Egyptian asphaltum, from the Dead Sea, is distinctly admitted, and, as a solvent for the same, some kind of mineral oil. With regard also to oxide of zinc, Mr. Szerelmey denies that his composition contains this material, but the report of the chemists on the Committee, addressed to their Chairman, refers particularly to the presence of oxide of zinc as a constituent of the preparation applied to the Spenker's doorway. This appears, however, to have been a special ingredient used at a time when Sir Charles Barry made the suggestion to try the effect of colouring matters in conjunction with the witness's ordinary preparation.

The use of a soluble silicate as a first application forms apparently the groundwork of Mr. Szerelmey's process, and the varnish made from Egyptian asphaltum, subsequently applied, is intended to protect the silicate in the pores of the stone from the action of the atmosphere and external influences generally. Three coatings are referred to as being necessary for the successful manipulation of the process; but it does not appear whether any two of these coatings are identical in character or otherwise.

In looking now to the results of the experiments, it would appear that the four years' interval which has

elapsed since their first application has demonstrated that a considerable degree of protection has been afforded to the stone; and when proper account is taken of the severity of last winter's frost, and the depth to which the stone itself must thereby have been subjected to extreme cold, there is reason to conclude that the effort of Mr. Szerelmey is at least a partial success; but in the event of its being selected among others for further experimental trial, it must surely be necessary to insist upon a candid explanation being afforded in regard to the exact nature of the ingredients actually applied to the surface of the stone.

NOTICES OF PATENTS.

361. *The Suppression of Arsenical and Sulphurous Fumes emitted during the First Operation or Calcination of Copper Ores.* E. T. JONES, Morden College, Blackheath. Dated February 13, 1861. (Not proceeded with.)

THIS invention has for its object the condensation and recovery of the arsenic and sulphur products which are given off in great abundance during the operation of roasting copper pyrites, and similar ores of that metal. The improved arrangement of apparatus consists, firstly, in the employment of large iron or clay retorts, such as are used in the manufacture of coal gas, in which retorts the roasting operation is carried on, and the volatile products escaping from the heated ore are led forward into a second or condensing arrangement, which consists of a long iron tube, about twenty feet in length, and from six to eight inches in diameter, the further end of which is made to dip just beneath the surface of water contained in a box or receptacle measuring four feet in every direction. For the effectual cooling of this part of the apparatus, a constant flow of water must be maintained in an outer cistern, within which the condenser is totally immersed; or, instead of this form of refrigerator, a one-inch leaden pipe, perforated on the lower side with small holes, and made to deliver freely a supply of cold water, may be fastened along the upper surface of the iron condensing tube. The appliances for clearing the tubes of the arsenical deposit, for receiving the heated charge from the retorts when sufficiently calcined, and other particulars relating to the mechanical arrangement, are described in the specification.

Anyone who has personally visited the copper-smelting district of Swansea, and noticed the desolation conferred upon the immediate neighbourhood by the volumes of copper smoke pouring forth night and day into the atmosphere, cannot fail to appreciate the value of any practical suggestion which will have a tendency to diminish the evil complained of, and if to this consideration be superadded the equally important question of economy, the subject becomes certainly worthy of renewed investigation in the hope of discovering really efficient means of satisfying the wants of the case. The process long since patented and worked by Mr. William Longmaid, for converting the sulphur of iron and copper ores into sulphate of soda, as a stage in the alkali manufacture, was a great effort in the right direction, but has not, so far as we are aware, met with general adoption in the case of the richer ores of copper. The method of proceeding referred to in the specification above, does not appear to take sufficiently into account the necessity for establishing a powerful current of air through the roasting furnace, in order to facilitate the oxidation and expulsion of the sulphur from the ore.

376. *Manufacture of White Lead, Zinc White, and Glazier's or Potter's Lead.* T. COBLEY, Dated February 14, 1861. (Not proceeded with.)

For the purpose of communicating increased density,

greater permanence of colour, and drying properties to white lead and zinc-white pigments, the inventor proposes to subject them to the action of a solution of borax or other compound of boracic acid. Potter's glaze is by the same means rendered more fusible, and metallic lead itself considerably softened by melting with borax.

It would be difficult, we imagine, to increase the density of white lead by the introduction of borax. The use of this substance in connection with pottery is well understood in this country, and always employed whenever the character of the work will warrant its expense.

378. *Impregnating the Atmosphere with Perfuming or Purifying Vapours.* R. RIMMEL, Strand, London. Dated February 14, 1861.

The patentee claims the vaporisation or volatilisation of perfumes by mixing them with an aqueous base and obtaining the disengagement of the fragrant molecules by means of heat or steam. He also claims the disinfection of the atmosphere, and of all bodies, by means of aromatic vapours.

In reference to the means of disinfection here referred to there is a danger of assuming that the introduction of pungent aromatic vapours must necessarily counteract as they disguise the element of contagion present in a vitiated atmosphere. There is no doubt that camphor, vinegar, and other substances are really efficacious when properly applied, but such remedies do not, like chlorine, strike at the root of the evil by exerting upon the dangerous effluvia nothing short of a power of destruction.

383. *Cure and Prevention of Toothache.* M. A. PRENSLAN, Liverpool. Dated February 15, 1861.

For the relief and cure of toothache the patentee uses "oil of paper" which he administers to the cavity of the tooth either by itself or upon cotton wool as a vehicle. The preparation is procured by submitting to destructive distillation in a close vessel any kind of paper made from a vegetable fibre.

This curiosity, "oil of paper," may perhaps owe any efficacy it possesses to the creasote among other tarry products resulting from its mode of formation. If the tuft of cotton wool were itself half consumed by fire before the remaining portion be applied to the tooth, perhaps equally beneficial results might follow; or would it not be better to burn it in the tooth itself?

Grants of Provisional Protection for Six Months.

2181. Charles William Siemens, Great George Street, Westminster, "Improvements in apparatus employed in connection with electric telegraphs, part of which improvements are also applicable to ascertain the heat in accessible places."—Partly a communication from Werner Siemens, Berlin.—Petitions recorded August 31, 1861.

2184. Thomas Shutt Stock, Joseph Sharp Stock, and Henry Taylor, Birmingham, "A new or improved tap or stop cock."—A communication from Allen Hill, New Orleans, U.S.

2186. William Muller, High Holborn, London, "Improvements in apparatus for roasting coffee, which improvements are also applicable to the roasting of cocoa and other vegetable matters."

2190. Alphonse Nicolas Saleres, Rue de Charonne, Paris, "Improvements in printing and colouring paper, chintz, and other fabrics, and machinery or apparatus for that purpose."

2202. Louis Rudolph Bodmer, Thavies Inn, Holborn, London, "Improvements in distilling apparatus."—A communication from Edme Alfred Egrot, Paris.—Petitions recorded September 4, 1861.

2225. William Spence, Chancery Lane, London, "Im-

provements in apparatus for dividing and softening vegetable fibres."—A communication from Felix Sorin, Rue des Fossés, Montmartre, Paris.

Notices to Proceed.

1233. John Chedgley, The Grove, Great Guildford Street, Southwark, "Improvement in the manufacture of glass rollers, plungers, and pipes, applicable to pumps and other uses."—Petitions recorded May 14, 1861.

1278. William Clark, Chancery Lane, London, "Improvements in electric telegraph apparatus."—A communication from Ludovic Guyot d'Arlinecourt, Boulevard St. Martin, Paris.—Petitions recorded May 18, 1861.

CORRESPONDENCE.

The Phosphorescence of Decaying Organic Matter.

To the Editor of the CHEMICAL NEWS.

SIR,—It may prove a subject of interest to some of your subscribers, particularly to those residing in the rural districts, to know that at this period of the year some very remarkable phenomena of the phosphorescence of decaying organic matter are to be observed. A great deal has been written, and many explanations given, on the *Medusa* and other phosphoric insects. It is also a well-known fact that many kinds of fish are phosphorescent even a few hours after they have been taken, and also that this does not apparently render them at all unfit for food, although perhaps it is the commencement of that stage when those chemical agencies which exerted all their powers to keep the body in a state of vital existence are devoted to the contrary work of decomposition.

On returning home a few evenings ago, my path lay through a wood of some ten acres in area. The sky was clouded, no moon visible, and the darkness intense. The surface of the ground presented a peculiar aspect. As far as the eye could pierce through the brushwood, a mass of vivid light covered the soil. There were no exhalations of phosphuretted hydrogen, as observed in marshy districts; but as I became accustomed to the change, I appeared to be walking over a carpet of glowworms. I accordingly stopped to examine the cause of the phenomenon; and, on stooping down, gathered several of the phosphorescent bodies which were moist and clammy; having the appearance of frosted silver, and apparently translucent throughout. On arrival at home, I transferred the materials I had collected, and which consisted of half-decayed oak and other leaves, pieces of dead grass, rotting wood and other organic matter in the first stages of decomposition, into a tray, and placed them in a dark room, when all the former phenomena became soon apparent as before. The light evolved enabled me to read printed matter when held close to the luminous bodies. After the leaves, &c., had been dried they ceased to be phosphorescent, nor did they become so on being moistened afresh with water; yet on being kept moist and exposed to the rays of the sun, they appeared to have regained their luminous properties by the next evening. During the rainless days of harvest, when no surface moisture was perceptible, the phenomena were not to be observed.—I am, &c.

Lux.

MISCELLANEOUS.

Disinfection of Sewage.—The Medical Inspector to the Westminster Burial Board, Mr. P. H. Holland, has on a recent occasion described the important results which have followed the adoption of Mr. Macdougall's process

for the deodorization of sewage. The plan has been put into operation in connexion with the town-drainage systems of Carlisle and Exeter, and is stated to have been completely successful both as regards the mitigation of a nuisance and the economical recovery of valuable manuring agents for agricultural employment. The process consists merely in the addition to the sewage of one gallon per diem of crude carbolic acid, costing, with carriage included, only 10s. a day—i.e. £16l. a year,—a sum extremely small as compared with that about to have been spent by the Boards of Health in these towns respectively for the purpose of conveying to a distance that which has hitherto proved a nuisance to the inhabitants and been subject to indictment. The apparatus for conducting the mixing process is as simple as could be desired, and the small proportion in which the disinfectant need be employed renders its application extremely economical.

The Canadian Oil Region.—A Correspondent of the *Toronto Leader* communicates some interesting particulars relative to the important discoveries of mineral oil, which have, in the course of a few months, proved a substantial source of wealth to those engaged, and opened out vast commercial relations in favour of a district which, until very recently, appears to have escaped special notice. The territory of Black Creek derives its name from a creek so-called running through the oil region. It is about half way between the eastern and western limits of Enniskillen township, distant twelve miles from Wyoming, and twenty-four from Sarnia. As we approach the Wyoming station of the Great Western Railway, the peculiar odour of the oil, which is here stored in large quantities for transmission to the eastern markets, is perceived at the distance of a mile or two if the wind happens to be favourable. At a distance of about five miles from Wyoming are the wells known as "Kelly's Wells." They are about a dozen in number, are yielding tolerably, and at one time attracted considerable attention. A refinery has been built at a cost of about 10,000 dollars, and is just now commencing operations. Arrived at the end of my journey, I found the oil region extending over a limited space. From the tests already made, the oilmen have concluded that it does not extend all over the township, as some at first supposed. Black Creek seems to be the richest territory yet discovered in Canada, and, according to some of the best authorities, it is the richest in America. Wandering along the banks and in the flats of the stream, which certainly deserves the name it bears, I at every few rods came across a well. They were in every stage of procedure. Some were just commenced, some were just finished; from many men were pumping oil into huge vats. Here let me say that the wells are of different kinds. First, there are the surface wells, so called because they are only sunk to the rock; these are for the most part on the flats of the creek. Some of them, when within a few feet of the rock, quickly fill with oil, which, with much noise and uproar of gas, bursts in from the loose gravelly substance overlying the rock. I have known wells fifty feet deep and five feet square full to the very brim with oil. Nay, I have seen it flow over the top and run away in a stream, wasting hundreds of barrels. These wells are dug large, the sides are cribbed and then puddled, so as to keep out the surface water. The oil from these wells is thicker and less pure than that from the rock wells, but a refiner told me that, though it was harder to deodorise and refine than the rock oil, yet, when manufactured, it made the best oil; having more body, it gave the best light, and burnt longer. It is also best for lubricating purposes. For rock wells they frequently sink a well like a surface well, and then drill; but more frequently the soil is bored out with a large augur until the rock is reached, a distance varying from forty to seventy feet. Then piping like pump logs is put in and driven down snug on the rock. After this the slow process of drilling is carried on, by foot, horse

power, or with steam engines, sinking from a few inches to perhaps ten feet per day, according to the nature of the rock. The character of the rock seems to vary with every well. It is generally, however, soapstone, with layers of shale, which is here in very small quantities; though at other parts of the township I have known drillers to go through fifty or sixty feet without change. There are also layers of sandstone, flint rock and sulphuret of iron; these vary in quantities at every well, though distant but a few rods. In the soapstone the oil is generally found, and at various distances from the surface. A continuation of little veins may be struck; then all at once a larger one, which may cause the oil to be forced in immense quantities out of the pipe. The quantity of oil this country is capable of producing is amazing. I have seen more oil, and a greater number of wells in operation and in course of being sunk, than I ever imagined as being in this county. I was told that, before January, 500 wells will be down. Large vats and tanks contain the oil, some of them holding as much as 1000 barrels; the largest are immense pits sunk in the stiff clay, cribbed and puddled, and these, they say, are better than wooden vats, which allow the oil to leak out, so penetrating is its nature. While I was there quietly looking on, I saw wells yield day after day from ten to fifty barrels. Estimating the wells in actual operation at 300, this would give 3000 barrels per diem at the lowest rate of production. And this, I assure you, is a small calculation; ten barrels per diem is moderate, and there will be double 300 wells before a year at the present rate of sinking. As far as I could judge, failure is the exception. Not one man in twenty who pretended to be in the oil region, failed to get oil; nor has any rock well shown signs of drying up. There is no doubt that we have at hand a source of untold wealth—an oil territory richer than any yet discovered. And, thanks to the enterprise of the Canadians, one firm in Toronto has undertaken the oil-refining business, while another is exporting it largely to Europe. Once the European market is opened, it must create an immense trade, for a substance from which are made refined toilet soaps, candles superior to wax, oil that gives a cheaper and not less beautiful light than gas, and much more convenient, and furnishing the best machine and lubricating oil yet known, must ultimately be brought into universal use.

ANSWERS TO CORRESPONDENTS.

*. * All Editorial Communications are to be addressed to the Editor; and Advertisements and Business Communications to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

*. * In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

J. A. D.—Declined with thanks.

A. H. B.—1. See the solution for marking on zinc plate, given in our Eighty-fourth Number. 2. It is a difficult process: try distilling. 3. We do not know the work.

Constant Reader.—To precipitate chloride of silver from a composite saline solution when present in the smallest quantities:—Concentrate as much as possible by evaporation, add sulphuretted hydrogen and white of an egg, boil. The albumen in coagulating will carry down the sulphide of silver, which may afterwards be treated by cupellation. Silver can in this manner be detected in sea water.

Red Gauntlet.—1. Allow strong ammonia to digest with metallic copper in fine turnings, with free access of air. 2. For a description of the mode of preparing artificial malachite, see our Second Volume, page 279. 3. We do not think an ether is known which has the odour of rose or patchouli.

Book Received.—A Manual of Chemistry, Descriptive and Theoretical, by William Odling, M.B., F.R.S., &c. &c. Part 1. London: Longman, Green, Longman, and Roberts. 1861.

THE CHEMICAL NEWS.

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SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Classification of Organic Substances by Series,
by JAMES SCHIEL, of St. Louis.

(Concluded from page 192.)

The acids form quite a number of generic series.

Series of Acids I.

$C_nH_{2n}O_2$ fatty acids $n=1$ to $n=30$.
 $C_nH_{2n}O_3$ glycolic acid $n=2$.
 $C_nH_{2n}O_4$ glyceric acid $n=3$ (isomeric with lactic acid).
...

The acids of this generic series correspond to the first alcoholic series.

Series of Acids II.

$C_nH_{2n-2}O_2$
 $C_nH_{2n-2}O_3$
 $C_nH_{2n-2}O_4$
 $C_nH_{2n-2}O_5$

The special homologous series deriving from this generic series are,

$C_3H_4O_2$ acrylic acid.	$C_2H_2O_3$ glyoxylic acid
$C_4H_6O_2$ crotonic "	$C_3H_4O_4$ pyroracemic acid
$C_5H_8O_2$ angelic "	...
&c. &c.	---
$C_2H_2O_4$ oxalic "	$C_3H_4O_5$ tartronic acid
$C_3H_6O_4$ malonic "	$C_4H_6O_5$ malic "
$C_4H_8O_4$ succinic "	...
$C_5H_{10}O_4$ lipic "	---
&c. &c.	---
$C_4H_6O_6$ tartaric "	$C_6H_{10}O_8$ mucic "
...	---

Series of Acids III.

$C_nH_{2n-4}O_2$
 $C_nH_{2n-4}O_3$
 $C_nH_{2n-4}O_4$
 $C_nH_{2n-4}O_5$
...

From this generic series are derived:—

$C_6H_8O_2$ sorbic acid.	$C_3H_2O_3$ mesoxalic acid.
$C_4H_4O_4$ maleinic acid.	$C_6H_8O_5$ citric acid.
$C_5H_6O_4$ itaconic "	---
$C_6H_8O_4$?	---
$C_7H_{10}O_4$ terebinic "	---

Series of Acids IV.

$C_nH_{2n-6}O_2$
 $C_nH_{2n-6}O_3$
 $C_nH_{2n-6}O_4$
 $C_nH_{2n-6}O_5$

From this generic series we derive:—

$C_6H_8O_2$ oxyphenic acid.	$C_8H_4O_3$ pyromucic acid.
$C_8H_{10}O_2$ terebentic "	$C_6H_8O_3$ pyrogallie "
---	$C_7H_8O_3$ ipecacuanic "
---	$C_8H_{10}O_3$ ricinoleic "

$C_4H_2O_4$ mellitic acid.	$C_6H_8O_5$ aconitic acid. ¹
$C_2H_{10}O_4$ cholesteric "	---

Series of Acids V.

$C_nH_{2n-8}O_2$
 $C_nH_{2n-8}O_3$
 $C_nH_{2n-8}O_4$
 $C_nH_{2n-8}O_5$
...

From this generic series we derive:—

$C_7H_6O_2$ benzoic acid.	$C_8H_8O_5$ krokonie acid.
$C_8H_8O_2$ toluylie "	$C_6H_4O_5$ comenic "
$C_9H_{10}O_2$?	$C_7H_6O_5$ gallic "
$C_{10}H_{12}O_2$ cuminic "	$C_9H_{10}O_5$ cholic "
---	$C_7H_6O_3$ salicilic "
$C_8H_8O_6$ phenoxacetic "	$C_8H_8O_3$ anisic "
---	$C_9H_{10}O_4$ phloretinic "
$C_8H_8O_4$ lecanoric "	$C_9H_{10}O_4$ veratric "

Series of Acids VI.

$C_nH_{2n-10}O_2$
 $C_nH_{2n-10}O_3$
 $C_nH_{2n-10}O_4$
...

Of the acids deriving from this generic series only a few are known as yet.

$C_9H_8O_3$ cumaric acid.	$C_7H_4O_7$ mionic acid.
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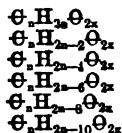
$C_8H_6O_4$ phtalic acid.
 $C_{24}H_{38}O_4$ choloidic "
 $C_{25}H_{40}O_4$ hyocholic "

From other known acids it is hardly possible to form series, as they stand mostly isolated; there is, for instance, only one acid—anemonic acid $C_{15}H_{14}O_7$ —

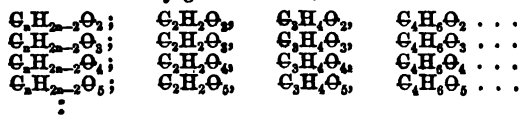
¹ The radical of the tribasic aconitic acid $C_6H_3O_3 \left\{ \begin{smallmatrix} H_3 \\ H_3 \end{smallmatrix} \right\} O_3$ may be derived from a hydrocarburet by substitution of H_4 by O_3 , as acetic acid is formed by substitution of H_2 by O in the radical C_2H_5 . The triacid alcohol, or glycerin corresponding to aconitic acid, would therefore be $C_6H_9 \left\{ \begin{smallmatrix} H_3 \\ H_3 \end{smallmatrix} \right\} O_3$; it belongs to the alcoholic series II, answering to the general formula $C_nH_{2n}O_2$, and the type $C_nH_{2n-3} \left\{ \begin{smallmatrix} H_3 \\ H_3 \end{smallmatrix} \right\} O$. This remark applies, *mutatis mutandis*, to other acids.

known as yet, which corresponds with the general formula $\Theta_x H_{2x-1} O_x$.

The generic series of acids considered above may be ranged into one primitive series—



where Θ_x means, that in order to form a generic series from one of these primitive formulas, Θ has successively to receive suffixed the numbers 2, 3, 4, . . . x . It is easy to see that as every generic series, as for instance:



consists of two kinds of series, of which those lying on the horizontal lines are homologous series, and those lying on the vertical lines are series of oxidation, the primitive series therefore expresses a seriation in a cube.

It is worth observing that there may be series of isomeric substances running parallel to each other; such series may be called homarithmical series.

Chemical Researches on the Assay of Silver by the Moist Way, by G. J. MULDER, *Math. Mag. Phil. Nat. Medicinæ, Artis Obstetriciæ et Pharmaceuticæ Doctor, Professor of Chemistry at the University of Utrecht, Grand Cross of the Order of the Netherlands Lion; of the Luxembourg Order of the Couronne de Chêne; of the Swedish Orders of Wasa and Polar Star, &c., &c.*

(Continued from page 139.)

I will even suppose, that when the last drop of nitrate of silver was added, a trace of precipitate made its appearance, but that experience has already taught us that one single drop more added would not have given any further trace of precipitate. Let us first get a nearer insight into this interesting matter; let us then try to explain it, and to answer the question which arises from this fact, and bears upon its technical application, viz.—how to determine exactly the precise quantity of chloride of sodium required to precipitate the quantity of silver originally present in a solution.

It follows from what has been just mentioned, that in the very liquid from which silver had been so precipitated, that the ultimate limit of a perceptible precipitate had been just reached, but nothing more; a decimal solution of silver will in its turn produce a precipitate in such a manner, that to 1002 milligrammes of silver at 15° to 17° C., it would appear as if one had used 0.54141 milligrammes of chloride of sodium too much; and this notwithstanding originally not a single drop of decimal salt solution had been added over and above that, necessary to complete the precipitation of chloride of silver.

The same liquid to which the decimal solution of silver has been just added, so that the utmost limit of precipitation had been reached, can now in its turn again yield a precipitate with the decimal solution of salt; for if this solution of salt be added we shall perceive that again about twenty drops of the solution, the temperature being from 15° to 17° C. will be required in order to reach the limit of another perceptible precipitate. If, on

the other hand, we now again return to the decimal solution of silver, we shall see the repetition of the same phenomena. As long as the chloride of silver keeps white, it is possible to repeat the just-mentioned experiment, and, *vice versa*, we shall find that we can use either the silver or salt solution of identically the same strength in order to reach the limits of a perceptibly visible precipitate, provided also the temperature of the test liquid remains constantly as mentioned.

I have to observe, however, that the precipitate produced by silver is more slowly formed than that produced by salt. It will not be deemed superfluous to enter into more details. It must be observed, that in all the following experiments, unless the contrary be expressly stated, pure chloride of sodium was used for the standard and decimal solution, the standard solution being purposely rendered a little weaker in order to be more fit for use in observing and studying the phenomena to be described now and hereafter. When it is stated that one gramme of silver was weighed off, this is only mentioned in order to make known the quantity operated upon; for in the majority of cases such a solution of salt was used as would require 1003 parts, at a temperature of 15° C., to precipitate 1000 parts of pure silver. It would have been impossible to make the following researches unless I had been provided with such accurate and exact drop apparatus as would enable me to measure the decimal solutions of silver and salt, so that twenty drops of each exactly corresponded to one cubic centimetre. The decimal solutions of salt and silver were chemically an exact equivalent the one towards the other, one drop of the one having the same value as one drop of the other. This also is a chief requisite in order to distinguish what follows.

Pure silver was weighed off in the following quantities:—

- I. 1000 milligrammes.
- II. 1002 "
- III. 1004 "

and each quantity was dissolved in five cubic centimetres of nitric acid of a density of 1.20; it was afterwards precipitated at a temperature of from 15° to 17° C., first with the standard solution of salt, then with the decimal salt solution, until the limit of a perceptibly-visible precipitate had been reached; after this decimal solution of silver was again added, guttatim, two drops at a time, until the limit was reached; then again salt solution and then silver (always the decimal solution of both) solution again, in every instance until the limit was reached.

Decimal Solution of Silver.		Decimal Solution of Salt.	
A	I. 20	B	I. 20
	II. 20		II. 20
	III. 20		III. 20
C	I. 23	D	I. 20
	II. 23		II. 20
	III. 21		III. 20
E	I. 22	F	I. 22
	II. 22		II. 22
	III. 20		III. 22 ¹

As the average of this experiment, we find that equally large quantities of the equally strong decimal solutions of silver and salt are required to produce a precipitate unto the limits of precipitability. We also find that this limit having been reached, there is a moment when, on applying

¹ The somewhat increased number of drops required when the experiment is continued for some time is due mainly to the heat the bottles receive while being shaken with the hands.

the last but one used precipitant, a new precipitate makes its appearance, which in both cases happens with 21 drops, equal to 1.05 cubic centimètres, or, if expressed in silver, equal to 1.05 milligramme weight thereof.

The experiment just mentioned has been often repeated with pure silver, and has always given similar results. We are, therefore, justified in stating, that, under the very same circumstances, that very liquid which apparently contains exactly enough salt, but no more, yields room for one milligramme of silver to precipitate salt (as chloride of silver of course); while, on the other hand, that liquid which apparently again contains just enough silver has room for 0.54 milligramme of chloride of sodium to precipitate silver.

The question, therefore, presents itself, what may be cause of this? As far as we are aware, the liquid only contains nitrate of soda and free nitric acid and chloride of silver in insoluble condition; nothing else. Chloride of silver may here be deemed as insoluble in water.* A solution of chloride of silver in water cannot be the cause in this instance.

Chloride of silver is soluble in solution of chloride of sodium; but not very much so at the ordinary temperature of the air. 1000 parts of chloride of sodium in concentrated watery solution only dissolve two parts of chloride of silver at 15° C. In case, even, therefore, the liquid in question might, after addition of chloride of sodium, contain a little free chloride of sodium, it would require 500 parts of chloride of sodium to dissolve one part of chloride of silver; but if, moreover, the chloride of sodium were the means of dissolving the chloride of silver, then, instead of a precipitate being generated about the end of the experiment,—a fact which visibly happens,—the formed precipitate ought to be seen disappearing, which is contrary to the effects observed in repeated experiments.

The solvent power of chloride of sodium here spoken of is, in fact, observed; but in order to be perceptible, a large excess of salt must be present, which is not the case in the experiment just alluded to.

Ten cubic centimètres of pure distilled water were mixed with one single drop of the decimal solution of silver, and also one single drop of the decimal solution of salt, a precipitate was formed, which was evidently stronger than a precipitate formed in 10 cubic centimètres of pure distilled water, to which had been added, first, one single drop of the decimal solution of silver, and then 20 drops of the standard solution of salt; in 20 drops of which are contained 5 milligrammes of salt, while one drop of the decimal solution of silver contains 0.05 milligrammes of silver. In this experiment the solvent effect of salt is already visible, but the excess of salt is also very large, for upon 5 parts of silver or 6.7 AgCl, there are 500 parts of salt,—i. e., 1 part of chloride of silver to 75 of salt.

The solubility of chloride of silver in common salt has, therefore, nothing to do with the phenomenon alluded to in the testing for silver. Chloride of silver, moreover, appears to be as much insoluble in dilute nitric acid as in water. To explain this phenomenon, there only remains to be seen whether chloride of silver is soluble in nitrate of soda: in fact, this solubility is the real cause of the mutual precipitation. I shall afterwards have often occasion to show the reality of this fact, but I now only mention the following simple experiment:—

In a bulk of 10 cubic centimètres of a solution of nitrate of soda of unknown degree of concentration, to which a small quantity of nitric acid had been added, 5 drops of a decimal solution of salt, and 5 drops of a decimal solution of silver, yielded a precipitate which on being heated was quite dissolved, whilst on cooling, the precipitate made its appearance again. Chloride of silver, therefore, is soluble in solution of nitrate of soda acidified with nitric acid and heat being applied. But it is not only so on application of heat: it is quite perceptibly so at the ordinary temperature of the room where the experiments take place.³

In order to ascertain whether the phenomenon of the mutual precipitation above alluded to is due to the formation of some double salt, or whether it belongs to the more simple phenomenon of solution, the following experiment was made at a temperature of from 15° to 17° C. Three grammes of pure silver were dissolved in 15 cubic centimètres of nitric acid, and partly precipitated with 300 cubic centimètres of standard solution, after which decimal solution of salt was added. The solution of the 3 grammes of silver was brought to the utmost limit of a perceptible precipitate, but no further. Decimal solution of silver was now added by drops, so as to give the utmost limit of precipitate; thereupon decimal solution of salt was added, and again silver, in every case just unto the utmost limit. The quantity required, therefore, expressed in drops, was the following:—

Silver, 60.
Salt, 60.
Silver, 60.

This experiment with 3 grammes of silver has thus required three times as much decimal solution of salt and silver as the experiment above alluded to. In the former experiment one gramme of silver was the starting point; in this experiment three grammes had been operated upon. Both experiments agree herein, that in either of them a large excess of chloride of silver insoluble in the liquid, is present. The phenomenon in question is, therefore, intimately connected with the quantity of the liquid. This liquid is water, containing nitrate of soda, with a small quantity of nitric acid. In the one instance there are rather more than 100 cubic centimètres; in the other case somewhat above 300 cubic centimètres. There is, therefore, every reason to assume that in order to explain the phenomenon of the doubly mutual precipitation the main cause of this phenomenon is solution, as defined by Berthollet.

It is now time to call attention to another peculiarity. We know already that one gramme of silver precipitated by common salt (standard solution) at a temperature of 15° to 17° C. easily bears 20 drops of decimal solution of silver, and again 20 drops of decimal solution of salt. What will be the condition if only 10 drops of the decimal salt solution are added after the silver solution? And if we refer to the experiment just described, in which three grammes of silver were used, what will happen if after the precipitation with salt to the very limit, instead of 60 drops of decimal silver solution only 30 are added?

Guy Lussac has already alluded to this, although only briefly, for in his "Instruction" he says (page 19):—"Il est à remarquer que, lorsque la dissolution normale

* It is not absolutely so, as has been discussed in this treatise already.

³ I have to observe that when to the last-mentioned liquid had been added 15 drops of the decimal solution of salt, and heat was applied, the precipitate thereby quite disappeared, but reappeared again after cooling. Nitrate of soda and chloride of sodium have, therefore, the property of dissolving chloride of silver with aid of heat.—M.

est parfaitement au titre, elle louchit très légèrement, soit avec le nitrate d'argent, soit avec le sel marin."

Suppose that 10 drops of the decimal silver solution are added to the liquid obtained by dissolving one gramme of silver (temperature 15° to 17° C.) which has been first precipitated with standard solution, and which has been afterwards brought by decimal solution of salt to the very limit; that after the addition of the 10 drops of decimal silver solution, the liquid has been well shaken, and after the precipitate has settled, the clear supernatant liquid be poured off and divided in two test glasses; on addition to the one part of these liquids of nitrate of silver, and to the other chloride of sodium, both in decimal solution, precipitates will be seen making their appearance, which, if the experiment was well executed, will amount to the same quantity in each of the liquids operated upon. I added 30 drops of decimal solution of silver (temperature 15° to 17° C.) to the liquid in which the 3 grammes had been dissolved. After having well shaken the liquid, and after the precipitate had settled, the clear liquid was poured off in two glasses. Five drops of decimal solution of salt having been added to the one, and 5 drops of decimal solution of silver to the other, gave rise in both cases to perfectly similar precipitates.

(To be continued.)

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 182.)

The Means of Weighing.—Four balances and two sets of weights have served for all my weighings. One of the balances, constructed by the celebrated French mechanician Gambey, carries one kilogramme in each pan, and turns when loaded with this weight to five-tenths of a milligramme. The other three were made by M. Sacré, mechanician to the Museum of Industry at Brussels. The largest of the three, which was constructed expressly for my researches, will bear a weight of five or six kilogrammes in each scale, and marks with certainty under this enormous load a difference of weight of one milligramme. Laden with two or three kilogrammes in each scale, it turns quite perceptibly with a variation of three or four-tenths of a milligramme. The second balance carries five hundred grammes in each scale, and denotes a difference of two-tenths of a milligramme. Lastly, the third, which is an assaying balance, turns definitely when loaded with twenty-five grammes in each side to a thirty-third of a milligramme. I do not believe that there exists another set of balances which for sensibility and accuracy are to be compared with these I have mentioned. Every chemist who has had occasion to examine them has come to the same conclusion; therefore I do not hesitate to say that they do honour to M. Sacré, and I may add that I feel much pleasure in being able to pay this tribute to his ability. The two sets of weights which I used are one of platinum, the other brass. The platinum set contains the kilogramme and all its subdivisions to half-a-milligramme. It was made by M. Gambey, under the superintendence of M. Arago. It constitutes the standard of the second order of the State of Belgium. I had occasion to verify its perfect conformity with the platinum standard preserved in the Chamber of Representatives, likewise made by M. Gambey, and compared by M. Arago with the standard kilogramme deposited with

the archives of the State in Paris. The other set of weights was adjusted by myself with every possible care by means of the platinum standard. The subdivisions of the milligramme down to tenths of these weights are made out of flat silver thread.

Finally, so as to be able to reduce all my weighings to a vacuum, I have determined the specific gravity of platinum and brass. I thus found that the platinum kilogramme displaces 47 cubic centimètres of air, and the brass 125 cubic centimètres. All my calculations have been made from these two data, whether I have performed the weighings in the air and have applied the corrections to both the weights and the material, or whether I have weighed in a vacuum and have submitted to correction the weights alone.

The Weighings.—Whenever the nature of the substance did not offer an insuperable obstacle, I have performed the weighings in air, and have placed as a counter-weight to the vessel containing the substance another vessel of the same nature and size, and, as near as possible, of the same weight as the one which it is intended to counterbalance, the difference between the weights being made up by a further addition of the same material. I have only resorted to weighing *in vacuo* as a means of control or when employing matters which were hygroscopic or liable to absorb air. I have effected the weighings *in vacuo* differently according to the shape of the vessels containing the substances, and according to the nature of the products which were to be formed. When the operation was performed in glass tubes, those containing the materials were introduced in a glass cylinder furnished with a metallic connection and a stop-cock, by which a vacuum could be obtained. To counterbalance this cylinder I employed another, absolutely identical as to its exterior volume, its shape, and matter of which it was composed; besides which, I took all the precautions at first suggested by M. Dumas for effecting in safety a weighing *in vacuo* and always obtaining constant results. When the shape of the vessels would not admit of this mode of operating, I fastened either a metallic connection, by means of a resinous cement, to the mouth of the vessel, or a cap of unvulcanised india-rubber furnished with a stop-cock. In this latter case I took care to use as a means of fastening thick pieces of platinum, silver, or copper wire slightly flattened; and to obviate the action of the air upon the caoutchouc—for in the air caoutchouc absorbs oxygen and increases in weight—I wound the metallic wire round in such close spirals that the surface was entirely covered. An arrangement of this kind perfectly preserves a vacuum, especially if care has been taken to roughen that portion of the glass to which the caoutchouc is applied.

This was equiposed by an apparatus absolutely identical in external volume and extent of surface. This arrangement, suspended in a large frame beneath the balance, hung freely in the air which was kept as dry as possible, and guarded by means of thick wooden partitions from all movements either from currents or heat. MM. Dumas and Regnault have already shown that on weighing with these precautions the equilibrium, once obtained, is preserved indefinitely. Whenever my apparatus has preserved a vacuum perfectly (which is much more difficult to effect than would be supposed) I have been enabled to verify the accuracy of this statement.

In all my weighings *in vacuo* the internal tension was, both before and after the experiment, between $0.0025''$ to $0.003''$; the air-pump would not permit me to go beyond that.

I have already stated above that, unless it was absolutely necessary, I performed the weighings in air; for I have shown in the most certain manner that vessels full of air always have a uniform weight as soon as the surfaces of all those portions of the apparatus which are in equilibrium are under the same conditions. When these conditions are changed, either by a slight elevation or depression of temperature, it takes a long time to bring them to their original state. Thus a tube of hard glass 60 to 80 centimètres long and 2 to 2.5 in diameter, after having been strongly heated, requires to be kept for two or three hours in the balance case before its weight becomes constant. Glass flasks of 1 to 4 litres capacity, after having been heated or rubbed, do not attain a constant weight until they have remained in the balance case for five or six hours.

Polished platinum heated out of contact with flame rapidly regains its primitive weight. But when this metal is more or less roughened it attains its proper weight much more slowly than either glass or porcelain.

The Vessels Employed in the Determination.—I have had recourse to platinum vessels whenever the nature of the operations permitted it. Otherwise I have only used for my determinations vessels and tubes of Bohemian glass. Berzelius has frequently cast doubts upon the inalterability of the vessels employed in analysis. According to my experience this doubt is well founded. I have, in fact, found that the weight of tubes, retorts and flasks, even of hard Bohemian glass, is changed very appreciably either by the action of flame or of chemical agents which are ordinarily considered to be without action on them. Without any exception whatever, all glass, when long heated to redness in a spirit or gas flame, slowly but constantly diminishes in weight.

This loss may reach 35 or even 40 milligrammes in the space of two hours when a tube of Bohemian glass 50 or 60 centimètres long is exposed to a decided red heat in a gas flame. The same glass preserves, on the contrary, a constant weight when it is heated to the point of softening and bending under its own weight, away from the flame, either by means of charcoal—taking care to keep the ashes from touching the tube—or in a bath of pure magnesia which completely surrounds it. It will be seen further on, when analysing chlorate of potash, that I have heated, for an entire day, tubes and flasks of Bohemian glass even to the point of rendering them milky and white without in the least altering their weight. The magnesia bath has rendered me great services. In it I have heated to redness not only glass but also the platinum vessels in which my determinations have been made. A wash with water acidulated with hydrochloric acid completely removes the traces of magnesia always adhering to glass or platinum which has been heated in contact with it. Were it not for this bath, I should have had to give up, in a great many cases, the employment of gas as a source of heat. As far as I know, M.M. Erdmann and Marchand were the first to employ magnesia for keeping surfaces away from the contact of flame.

I have sufficiently proved that at temperatures below 300° or 350° C. neither nitric nor hydrochloric acids exert any action upon Bohemian glass, nor in general upon any hard glass perfectly free from alumina and excess of alkali.

Flasks of Bohemian glass, after having been used for a considerable number of syntheses of nitrate of silver, nitrate of lead, and chloride of lead, have preserved their original weight almost absolutely. The very slight altera-

tion which they may have experienced proceeds rather from the friction of the stopper, which is ground into the neck with emery, rather than to the action of acids.

Retorts, globes, and flasks, of common glass, yield even at the ordinary temperature, traces of their basic constituents to hydrochloric and nitric acids.

Experience has shown that sulphuric and hydrochloric acids, as well as the vapour of sal-ammoniac, attack all kinds of glass at a red heat.

The Agents Employed in the Determinations.—

It was not only necessary for me to have vessels capable of withstanding the action of the agents used therein, it was also needful that these agents should not bring with them ingredients capable of altering, either as to weight or composition, the bodies of which it was desired to determine either the formation or decomposition.

The intermediate bodies employed in my experiments have been water, hydrochloric, nitric and sulphuric acids, and sal-ammoniac. I confess that previous to undertaking this long work I had been completely mistaken as to several of the difficulties which had to be overcome before I could procure these bodies in a state of purity. Berzelius in his work on the atomic weights¹ has already mentioned the difficulty of obtaining liquid hydrochloric acid which would not leave a residue upon evaporation, and even confessed that he could not himself arrive at this result.

I, therefore, give here the experiments which I have made, both the methods which I have devised to procure these bodies in a pure state, and the means employed to ascertain this purity.

Water.—Either rain or spring water, twice distilled, taking care the second time to condense the steam in a platinum refrigerator, yields a liquid which, when evaporated immediately afterwards in a platinum vessel, either covered or freely exposed to the air, volatilises without leaving the slightest residue. This same distilled water, after having been kept for some days in a platinum or porcelain vessel, and then evaporated, leaves a very appreciable brownish-yellow residue, which burns away completely at a red heat in the air. Distilled water, which would volatilise completely in a covered or open vessel, if acidulated with pure hydrochloric or nitric acid, then leaves, when evaporated in a covered or open vessel, a yellow residue.

Distilled water contains, therefore, volatile organic matter, which, after a certain time, becomes spontaneously fixed, and becomes instantly so under the influence of nitric or hydrochloric acids.

When the presence of organic matter, fixed, but capable of destruction by heat, offered no objection, I made use of water twice distilled, and condensed the second time in a long platinum tube properly cooled. In the other case, at the first distillation, I slowly passed the vapour through a long copper tube, luted with clay and sand, completely filled with oxidised copper turnings, bent in a zigzag manner, and heated to a decided red heat in a furnace. The water resulting from the condensation of this vapour was re-distilled a second time and condensed in a platinum refrigerator. This refrigerator is a long platinum tube, soldered with gold, bent into the form of a syphon, and employed in a manufactory to conduct and cool the vapour of boiling concentrated sulphuric acid. This water, either immediately after its preparation or after being kept a long time in a stoppered flask, if submitted to evaporation by itself, or

acidulated with nitric acid, always volatilises in a covered vessel without leaving any trace of residue.

Hydrochloric Acid.—The liquid hydrochloric acid which I have made use of was obtained by conducting hydrochloric acid gas, by means of a Bohemian glass tube, into water deprived of all matter, fixed or capable of becoming so, and contained in a platinum tube. The hydrochloric acid gas was disengaged by the action of heat upon the pure liquid acid, such as is met with in the laboratory.

The acid thus obtained can be evaporated to dryness in a platinum retort without leaving the least trace of residue. If the evaporation is effected in an open vessel, the matters floating about in the air are always fixed by the acid, and then the purest product will leave a yellow residue, in which iron is often found. The testing of hydrochloric acid should, therefore, be done out of contact of air, contrary to the plan usually adopted.

Nitric Acid.—To procure absolutely pure nitric acid, I heated the commercial acid, sp. gr. 1.500, until the condensed product contained no chlorine.² The nitric acid thus obtained was twice re-distilled, and preserved in flasks. When I desired an acid leaving absolutely no residue, I re-distilled the purified acid a third time in a large platinum retort, employing a globe of Bohemian glass as a receiver. The acid was always used immediately after its preparation. I test its purity by operating on a whole litre. It evaporates in a platinum retort without leaving the slightest residue, when its vapour is carefully guarded from contact with the air.³ If the air has free access to it, the acid always leaves a more or less appreciable yellow residue, exactly as we have seen in the case of pure hydrochloric acid and pure water acidulated with acids, when evaporated without guarding them from atmospheric impurities. According to my experience, I am led to believe that the impurities found by most chemists in acids do not always pre-exist there, but have been communicated to them by the air having free access to them during the assay. *For this reason I have been obliged to perform my assays in platinum retorts fitted tightly to a Bohemian glass receiver; and all the evaporations of acid liquids have been performed in close vessels so as to prevent the air from penetrating into the interior of the vessels containing the liquid undergoing evaporation.*

Sulphuric Acid.—To obtain this quite free from all fixed principles, I distilled the commercial acid in a luted glass retort. The acid so prepared was re-distilled by small portions at a time in a platinum retort, taking the precaution to volatilise the liquid without allowing it to boil. As a condenser I used tubes of platinum soldered together with gold, the ends being ground so as to fit tightly one within the other. The acid was collected

² The assay of fuming nitric acid for chlorine requires that it should be previously mixed with four or five times its volume of water. Indeed, an acid at its maximum of concentration can contain traces of chlorine without being rendered turbid by nitrate of silver solution. This is easily seen by adding nitrate of silver to pure water and then mixing a few drops of this solution with nitric acid, which contains traces of chlorine. The acid will remain clear, but by adding to it the remainder of the water, the liquid will be seen to be decidedly turbid.

³ Platinum vessels are not suitable for the preparation of pure acids until they have been freed from the iron which ordinary manufactured platinum always contains. The best way which I have found for arriving at this result is to cause the vapour of dry chloride of ammonium to react upon them at a red heat. As long as the chloride of ammonium is slightly coloured yellow, it is certain that the metal contains iron. The platinum in this manner becomes dull-looking and much whiter. Before weighing the vessels so purified, they must be polished. It suffices for that to put them in a lathe and burnish them by means of an opal. All my platinum vessels have been treated in this manner.

and preserved in a platinum crucible until required for use. It never left a trace of residue when evaporated in a platinum retort.

Sal-ammoniac.—I will detail subsequently all the precautions which I have taken to obtain sal-ammoniac absolutely pure.

Carbonate of Soda.—Carbonate of soda, deprived of alkaline chlorides and sulphates by successive crystallisation, always contains iron. To remove this metal the salt was dried and heated to dull redness in a silver dish. After cooling, the anhydrous carbonate was added to a quantity of cold water insufficient to dissolve it. The solution was evaporated in a silver dish, and the residue dried and calcined a second time. This treatment was repeated until hydro-sulphuric acid no longer coloured the solution of the carbonate green. I have been obliged to repeat three times the successive action of heat and water to obtain this result. Carbonate of soda so purified still contains silica.

(To be continued.)

Experiments on the Best Mode of Correctly Estimating the Per-centage of Alcohol Existing in Wines, by JOHN HORSLEY, F.C.S. Lond., Analyst to the County of Gloucester.

FIRST note the specific gravity of the wine to be tried, say that compared with distilled water at 1000 it is .997, or 3 short of that number; then take 5 fluid ounces of the wine and evaporate it by rapid boiling in a sufficiently capacious flask to 2 ounces; then place the flask in a pan of water for a short time to cool. By this operation you will have expelled not only all the spirit, but a good portion of water with it.

Next make this residuary liquor containing the sugar and other extractive matter of the wine, minus its alcohol, up to the original bulk with distilled water, and take its specific gravity, which in this case will be 1.020. If this excess of 20 over the weight of the standard of distilled water be deducted from the specific gravity of the wine thus,—

.997 including its spirit, water, and other extractive matter of the grape;
20 amount of extractive matter in the water only;

.977 will be the true specific gravity of the spirit and water contained in the wine, minus the extractive matter, which is equal to 17 per cent. of alcohol, according to the published tables of alcoholic strengths at different densities and temperatures. Or, which is nearly the same thing, we may proceed by subtracting the specific gravity of the wine, .997, from the residuary liquor, thus 1.020

997

leaving .23, which, deducted from the standard of distilled water 1000

23

leaves .977 as the true specific gravity of the spirit and water in the wine.

These examples may be further verified by a distillatory experiment thus:—Take 5 ounces of the wine; distil off exactly half by a gentle heat from a graduated retort to which a receiver is connected till oiliness ceases to form in the tube end of the retort, indicating that all the alcohol has passed over. The specific gravity of this distillate (consisting of spirit and water) will be

·955, equal to 34 per cent., which, divided by 2,—as only half the bulk has been drawn over, gives 17 per cent. Or the distillate may be diluted with its own bulk of distilled water up to the original 5 ounces, when its specific gravity will be ·977, equal to 17 per cent. of alcohol.

This per-centage may be converted into Revenue proof spirit by rule of three sum, thus:—

Alc.	Wat.	Alc.
As 49	: 51	: 17 = 17·69 water
		17 alcohol
Proof . . .		34·69

TECHNICAL CHEMISTRY.

On the Artificial Colouring Matters derived from Aniline, by M. E. KOPP.¹

In the abstracts of the papers by M. Kopp, which we published in vol. ii. of the CHEMICAL NEWS, a general history was given of the production of aniline, and the various processes adopted for obtaining the coloured derivatives of that body. M. Kopp now passes in review several Memoirs that have been published in France on the Chemistry of the Colouring Matters. This, he remarks, is still extremely obscure; and none the less so that in these Memoirs there are not only wide disagreements in the theoretical views entertained, but differences as to simple facts, and difficulties and contradictions to be met with in the writings of the same author. We shall give a short summary of these papers, at the risk of repeating once or twice what has already been published in the CHEMICAL NEWS.

The first paper noticed is M. Guignet's "Researches on Fuchsine." This substance is obtained in the form of a viscous mass, which in time becomes almost completely solid. It gives a bright red solution with alcohol, both absolute and somewhat diluted; it is also slightly soluble in ether and sulphide of carbon. It is soluble in boiling water, but on cooling it is deposited in reddish-brown scales, which do not appear to have a definite crystalline form. Nitric acid dissolves it, forming a yellow solution, which, on being diluted with water, becomes red. It gives a precipitate of chloride of silver with the nitrate, showing that the solution contains a hydrochlorate. (M. Guignet regards fuchsine as a mixture of a peculiar base with its hydrochlorate.) The nitric solution evaporated at a low heat yields well-formed prismatic crystals, of a deep brown colour. Water and alcohol partially decompose these crystals, giving a red solution; but a part of the salt crystallises again without having undergone any change.

Strong sulphuric acid decomposes fuchsine, disengaging an abundance of hydrochloric acid vapour. It forms a yellow solution, which, on cooling, becomes a mass of yellow plates, like mica. These plates appear to be an acid salt. When the solution is carefully saturated with carbonate of baryta, another sulphate is obtained, which crystallises in colourless needles. Lastly, hydrochloric acid dissolves fuchsine, forming a yellow solution, which yields brown prismatic crystals. All the above solutions are precipitated when they are exactly saturated with ammonia. Carmine-red flocculi are deposited, which are slightly soluble in water, and freely soluble in alcohol. In contact with an excess of ammonia

or caustic potash these flocculi are completely decolorised and changed into a white insoluble matter, which, however, re-assumes its red colour when treated with water or acids. The same is observed with stuffs dyed with fuchsine. The colour is discharged by caustic potash, but re-appears when the tissue is washed with water or treated with an acid.

M. Guignet expresses an opinion that fuchsine may have the composition $C_{17}H_7ClN_2$, and be analogous to nitrazophenylamine, $C_6H_5(NO_2)N_2$. This opinion, however, is not shared by M. Kopp, who observes, that the fuchsine of MM. Rénard and Franc is a very complex substance; and it is possible that many of the reactions described by M. Guignet, and some of the crystallised bodies he has observed, are derived, not from the colouring matter itself, but from the bodies which accompany it. The experiments should be repeated with fuchsine purified by the process of M. Schneider—which we shall presently describe—or with the colouring matter obtained from wool or silk previously dyed.

The ideas of M. Bechamp on the formation of fuchsine next quoted by M. Kopp, we have already given at p. 311, vol. i., and p. 215, vol. ii. We need not, therefore, repeat them here. The properties of fuchsine he describes as follows:—It is an organic base, slightly soluble in water, having the formula $C_{24}H_{10}N_2O_2$. In the hydrated state it is of a deep red colour. When pure, as it is obtained by precipitating the alcoholic solution with ether, it appears in form of non-crystalline scales of a brilliant metallic green colour. The aqueous and alcoholic solutions, and the solutions in wood spirit and acetone are red. It forms uncrystallizable salts, the solutions of which are red when they are neutral, and yellow with an excess of acid. Sulphurous acid slowly decolorises the solution, but by concentration at a gentle heat the colour is restored. With hydrochloric acid it forms two kinds of salts—neutral and acid—the former ($HCl + C_{24}H_{10}N_2O_2$) is red, and gives a red solution; the second ($2HCl + C_{24}H_{10}N_2O_2$), obtained by passing hydrochloric acid gas over the hydrated fuchsine dried in a vacuum, is of a cinnamon brown colour, and gives a reddish-brown solution. Experiment gave 17·3 and 15·25 per cent. of acid in the neutral salt; calculation requires 15·6 per cent. The same salt gives a purple precipitate with chloride of platinum, which dissolves in alcohol, giving a violet red solution. The formula of the precipitated salt is $PtCl_2 + C_{24}H_{10}N_2O_2.HCl$. This formula requires 24·26 per cent. of platinum. Experiments gave 24·43 and 24·56 per cent. of platinum.

Bechamp's views of the formation and constitution of fuchsine have been combated by Barreswil, who has shown that fuchsine is a non-oxygenated body. In fact, by operating with perfectly anhydrous bichloride of tin and aniline under circumstances in which it was impossible for air or moisture to reach the materials, he made fuchsine. The formula of M. Bechamp, therefore, cannot be exact. It is, moreover, contradicted by the fact that fuchsine is generated when aniline is treated with iodine, bromine, sesquichloride of carbon, and iodoform, neither of which contains oxygen nor reducible bases.

MM. Persoz, de Luyne et Salvétat, in their report on aniline red, have pointed out a number of facts. They first verified the production of the red colouring matter by heating an hydrous bichloride of tin with aniline, and also with the hydrated bichloride, protosulphate of tin, iodoform, iodine, mercurous, uranic, and ferric nitrates, and the fluoride of tin. When operating with 8 kilogrammes of aniline and 4 kilogrammes of anhydrous bichloride of tin, they obtained in twenty minutes

¹ Abridged from the *Moniteur Scientifique*.

a pasty mass of a bright red colour. By boiling together for one hour 4.2 kilogrammes of aniline and 2.3 kilogrammes of mercurous nitrate, they obtained a syrupy substance of a violet-red colour. They also made the colouring matter by means of arsenic acid, and proved that in this operation the arsenic was not reduced to arsenious acid, and yet that nearly the whole of the aniline was transformed.

Their ideas on the formation of fuchsine are the following:—It is not a product of the oxidation of aniline, but the result of a molecular change in that body, occasioned by the presence of various chemical agents. To effect this molecular change, it is necessary to employ a temperature of from 180° to 200° C. Indisine, or the violet colouring matter, on the contrary, is an oxidised product, the formation of which is always accompanied by the production of a good deal of resinous matter. From this it results that when non-oxidisers are employed, only fuchsine is formed, while oxidising bodies induce the formation of indisine, but in relatively small proportions, under even the most favourable circumstances.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 1, 1861.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

A letter from Dr. Fairbairn, F.R.S., &c., tendering his resignation of office as one of the Vice-Presidents of the Society having been read, it was moved by Mr. SPENCER, seconded by Mr. J. A. RANSOME, and unanimously resolved, "That the Members of the Literary and Philosophical Society express their regret that Dr. Fairbairn feels it necessary to resign his Vice-Presidentship of the Society, and hope he will still continue to favour them with his presence and his valuable contributions as formerly."

Dr. SCHUNCK, F.R.S., &c., was elected a Vice-President, in place of Dr. Fairbairn, F.R.S.

Mr. Alfred Fryer was elected a Member of the Council in place of Dr. Schunck, F.R.S.

Dr. SCHUNCK announced to the meeting that Messrs. Spence; Roberts, Dale, and Co.; Tennants and Co., and others had, at his request, presented to the Society a portion of the beautiful products which had formed part of the interesting exhibition in the laboratory of Owen's College during the late meeting of the British Association.

It was moved by Dr. SCHUNCK, seconded by Dr. CLAY, and unanimously resolved, "That the thanks of the Society be given to the donors of chemical products."

Mr. BAXENDELL, F.R.A.S., read a Paper entitled "Observations of Comet 1, 1861."

Although this comet was not at any time a very conspicuous object to the naked eye, yet some of the features which it presented when viewed with a good telescope at the time of its greatest brightness were sufficiently remarkable to render it an object of peculiar interest to the astronomer; and I have therefore thought that a brief account of the observations made with the excellent instruments of Mr. Worthington's observatory might be acceptable to the members of this Society.

My first observation was made on the night of May 3, 1861. The comet was then already visible to the naked eye as a dull hazy-looking star of the 4½ magnitude. At 10h. 17m. 48.7s. G.M.T. a comparison with the star *Arg.*

178,8=190,112 made with the equatorially-mounted achromatic of 5 inches aperture, and a dark field photographed micrometer constructed by Mr. Dancer, gave the comet's apparent place R.A. 10h. 5m. 27.76 Dec. + 48° 52' 7.7". Turning the 13-inch reflector upon the comet with powers of 81 and 196, it was found that the nebulousity was more than 20' in diameter, considerably condensed in the middle, but without any distinct planetary or stellar nucleus. There was a faint tapering elongation extending about a quarter of a degree from the north following side, and stars of the 11th and 12th magnitude were easily seen through the comet at the distance of half a radius from its centre.

May 4.—Three comparisons with *Arg.* 173,122 gave the place of the comet at 9h. 26m. 19.3s. G.M.T. R.A. 9h. 52m. 19.83s. Dec. + 45° 18' 28.1".

With the 13-inch reflector the diameter of the nebulousity constituting the head of the comet, carefully estimated by comparison with the known diameter of the field of view, was 22'. It was much condensed in the middle, but there was certainly no distinct stellar nucleus. The centre of greatest condensation was not in the centre of the nebulousity, but towards the north following side. The tapering elongation of last night was now a narrow and slightly fan-shaped tail of 2½ degrees in length, but apparently separated from the nebulousity of the head by a remarkable and comparatively dark interval. The point of origin of this singular tail was estimated to be from 12 to 15 minutes distant from the centre of the head, and its breadth at this part was about 4½ minutes, and at its extremity about 15 minutes. Its axis was perfectly straight, and its brightness was greatest at the narrow end, where it was equal to that of the nebulousity of the head at two-thirds of the radius from the centre.

May 5.—At 10h. 16m. 59.7s. G.M.T., six comparisons with *Lalande's* 19168 gave the comet's apparent place R.A. 9h. 39m. 37.82s. Dec. + 41° 21' 34.5".

The sky to-night was not very favourable for the observation of faint objects, but the general features of the comet did not appear to have undergone any material change. Last night it occurred to me after leaving the observatory that the axis of the tail was not exactly in the direction of the comet's radius vector, and to-night I found its angle of position at 13h. 35m. sidereal time to be 96.7°. At this time the position of the sun and comet were:—

The sun R.A. 42° 54' — N.P.D. 73° 33'.

The comet R.A. 144° 50' — N.P.D. 48° 43'.

From these data we find that the angle of position of a prolongation of the comet's radius vector, was 69.9°. The apparent deviation of the axis of the tail was therefore 26.8° in the direction of the comet's motion.

May 7.—At 12h. G.M.T. the comet appeared to the naked eye to be nearly equal to μ Leonis, and equal to, if not brighter than, 38 *Lyncis*.

May 9.—Three comparisons with *Lal.* 17,987 gave the comet's apparent position at 9h. 57m. 30.5s. R.A. 9h. 3m. 23.25s. Dec. + 26° 11' 26.1".

At 13h. 15m. sid. time the angle of position of the axis of the tail was 103.2°. At this time the angle of position of the comet's radius vector was 74.5°; the deviation therefore amounted to 28.7°.

With the 5-inch achromatic the tail appeared to be half a degree in length; but with the 13-inch reflector it was fully one degree, though fainter than when last observed, and still much less in breadth than the diameter of the head. The average diameter of the head was about 20', but the nebulousity extended farther on the south preceding side of the point of greatest condensation than on the north preceding or north following sides. There was still an entire absence of any stellar nucleus. To the naked eye the comet appeared as a star about equal in brightness to μ Leonis.

May 14.—Notwithstanding the moonlight, the comet was

still visible to the naked eye, and in the 5-inch achromatic with a power of 68 it was about 10' in diameter. The tail, however, could not now be seen.

May 17, 10h. 25m. The comet, though at a very low altitude and with strong moonlight, was still very easily seen with the 5-inch achromatic, and did not appear to have diminished since the 14th inst. This was the last opportunity I had of observing it.

Lalande's stars Nos. 19168 and 17987 occur in Bessel's Zones Nos. 454 and 347, and Bessel's places have been used in making the reductions.

Some remarks having been made by Mr. ATKINSON and Mr. VERNON respecting the vibrations of the barometric column which frequently take place during gales of wind,

Mr. FRYER stated that, with a view of ascertaining whether the barometer was subject to minute oscillations inappreciable by the ordinary method of observing, he had placed an aneroid barometer under the microscope, using the quarter-inch object glass, and by bringing one of the strise on the surface of the steel index into focus, he was able, by means of the micrometer, to observe a movement of the 20,000th of an inch. The index appeared to be in constant motion, but at present he was not prepared to lay any set of observations before the Society. Mr. Fryer also suggested, for the same purpose, the use of a barometer filled with the oleine of olive oil, such an instrument being free from the disturbances caused by the watery vapour present in the water barometer and exhibiting a greater range.

PHYSICAL AND MATHEMATICAL SECTION.

April 25, 1861.

Mr. MOSLEY read an extract from a letter received by him from Dr. Martin, of Lisbon, in which were enclosed a series of meteorological observations taken at that place, from the 6th to the 12th February last, both inclusive.

Mr. VERNON produced a printed series of meteorological observations made at Sydney and Paramatta, in 1857, 1858, and 1859 which he presented to the Section.

Mr. BAXENDALL gave details of observations which he had made during the last few days on a large and remarkable solar spot, in which the secondary penumbra had been plainly visible, and had been observed by him to consist of a fine net work of bright lines. The penumbra was strikingly striated, and the middle of the nucleus had been occupied by a bright spot detached from the penumbra at first, but afterwards joined to it, as the spot began to close up, by one of the projecting spurs usually observed to occur in decaying spots. For the purpose of the observations a trough filled with sulphate of indigo was placed in the cone of rays coming from the large mirror of a Newtonian reflector of seven inches aperture, before they reached the eyepiece. The sun's image was a deep red colour, but the definition was very good.

Professor CLIFTON suggested that the unusual visibility of the secondary penumbra in this case might arise from its being so coloured as to be rendered more visible than usual by the use of the sulphate of indigo.

July 27, 1861.

Mr. BAXENDALL read a Paper entitled "*Observations of Comet 1, 1861.*" [This Paper was afterwards read at the Ordinary Meeting of the Society, held on October 1, 1861.]

Mr. BAXENDALL also read a note "*On a Solar Spot of Long Duration.*"

At the last Meeting I exhibited a sketch made on April 24, 1861, of a solar spot in which the secondary penumbra had been seen with unusual distinctness. This spot has since twice crossed the visible disc of the sun, and has had a term of existence considerably above the ordinary average. It was first seen on April 17, near the eastern limb of the sun, and was then a fine spot of not less than the third magnitude, and after undergoing many consider-

able changes of form and structure, was finally observed very near the western limb on June 21, as a spot of only the 9th or 9½ magnitude, and from its previous rate of decrease it is probable that it would become entirely extinct in four or five days afterwards. Its duration, therefore, would not be less than 70 days, and it thus affords a striking instance of the fact that frequent and extensive changes of form and structure do not necessarily tend to produce an early extinction of a spot. Between April 20, at 5h. 13m. G.M.T., and June 15, at 5h. 30m. it moved through 783.3 degrees of longitude, or at the rate of one sidereal revolution in 25.74 days. On April 20 its heliographical latitude was 14° 11' N., and on June 15 14° 2' N. These results indicate a direction and rate of drift which agree very nearly with the results which Mr. Carrington has obtained for other spots in and near the same parallel of latitude.

NOTICES OF BOOKS.

A Manual of Qualitative Analysis. By R. GALLOWAY, F.C.S. Professor of Practical Chemistry in the Museum of Irish Industry. Third Edition. London: John Churchill, 1861.

In the preface to the second edition of Mr. Galloway's Manual, the author describes in some detail the method of teaching Chemical Analysis which he has found to answer best. The comparatively rapid sale of his work, and the appearance of a new and improved issue, at the present time, when so many other volumes on the same subject have been published, seem to demand a careful notice of the points in which Mr. Galloway's plan differs from other plans. We propose therefore to criticise the account which the author gives of his own scheme of analysis, and the way in which he wishes it to be used (pp. vi., vii., viii.)

He says:—"In all other works on Chemical Analysis with which I am acquainted, the properties of the bases and acids, and the application of these properties to the analysis of substances, are treated separately. As a consequence, the student, when he goes through the experiments on each base and acid, performs these illustrations of the properties, without perceiving their use or application." This objection is easily obviated, if those properties of each base and acid which are actually made use of in analysis be distinguished from the others by a different type, at the same time that all the compounds mentioned under one acid or base are mentioned under all others, even if they are soluble. This plan has not yet been adopted in more than one Analytical Treatise, so far as we know.

Mr. Galloway proceeds:—"In the part of the work devoted to the Analytical Course, he is told what to do, either by tables (as in 'Giessen Outlines,' and some other works,) or, as in Fresenius, by a more tedious, and, in my opinion, as defective a method. The principles upon which these instructions are based, are not given with the instructions, but in a separate chapter of the work; and, unless the student is able himself to unite the two,—the principles which the practical instruction,—he becomes a mere analytical machine." Here we think that our author depreciates too much the methods of other teachers of the analytical art.

The student is generally first taught the chief features of the bases and acids, the most characteristic compounds of each base and acid having been specially pointed out to him. This done, and a broad view of the whole subject having been acquired, he proceeds to utilise his knowledge by the application of particular facts in the actual course of analysis. He has already learnt the distinction between group and special tests, and will find no difficulty in seeing why such and such re-agents are used in different cases.

We concur in Mr. Galloway's opinion of the plan adopted by Fresenius. There can be no doubt about its being tedious and difficult. The objection to tabular statements of analytical plans entertained by the great German analyst, seems almost morbid. Whether they appear as tables or not, analytical operations can only be represented in tabular statements. Those given by Fresenius are merely spread over some two or three pages, somewhat vaguely, instead of being concentrated in a systematic and intelligible form on one page only. Fresenius' plan is less costly to print, but, at the same time, more difficult to understand. One can scarcely work through a few pages of his "System of Instruction in Qualitative Analysis" without meeting with such delightfully complicated directions as this,—"Test for cobalt and nickel, as directed, § 192, 1, b, β , $\beta\beta$, (149)."

Mr. Galloway characterises the ordinary system as difficult as well as defective; he says, "It is so difficult, that those who employ it generally teach one thing, useless in itself, in order that they may teach another; they teach the student first to look for one acid and one base—a limitation which is not adopted in actual practice—solely for the purpose of rendering the course in which they have [?] he has] to look for all the bases and acids less difficult." We do not think it quite useless for the student to be able to detect one acid and one base; the principle of progressive exercises is one universally adopted, even by Mr. Galloway himself, who recommends the student to examine each group by itself—why not, then, each individual member?

Our author proceeds thus:—"In the plan I have adopted, the properties of the different members of each group are contrasted by placing them in parallel columns; the advantage of this is, that the student, after he has performed the experiments, is able himself to devise methods for the detection and separation of the different members; it places him, in fact, in the position of a judge who has to decide some cause after having heard all the pleadings." The plan here referred to consists merely of a further development of the plan of tabular statements. Mr. Galloway speaks of the reason and judgment exercised by the student using these tables. We think that the behaviour of the various bases and acids ought to be ascertained in a less narrow way, as pointed out above, and that the tables of properties are not unlikely to assist in making the student an "analytical machine." A careful selection of reactions ought to be presented to the beginner under each base and acid, while the analytical schemes subsequently given, ought not to do much more than suggest how the general and special tests may be successfully applied in the detection of the substances met with in analysis. The tables showing the reactions of the various bases and acids are very full, and present, without doubt, much information in a small compass, but they are not peculiar to the present Manual, and have been largely employed in other works.

Those features of his Manual to which Mr. Galloway specially directs attention, do certainly enable a student to acquire the power of performing analyses more rapidly and more easily than the ordinary system does: whether this power rests upon as secure a foundation of knowledge, is a matter of doubt.

The present edition is brought down in every important particular to the state of analytical knowledge at the present time. The photo-chemical method of Cartmell and the spectrum experiments of Bunsen will be found described very clearly and fully. Many of the special tests recently introduced into the laboratory, such as the excellent method of separating cobalt and nickel by means of nitrite of potash, will be here found in their proper places. We do not find, however, any reference to the use of binoxide of lead as an oxidizing reagent, although it is found extremely useful, in conjunction with nitric acid, for detecting traces of manganese carried down in the

ammonia precipitate (Group iii.), and for ascertaining the presence of chromium in the same precipitate. In the latter case a small portion of the precipitate is boiled with binoxide of lead in the presence of a solution of potash, chromate of potash being thus formed. We have not been able in actual practice to carry out quite successfully Mr. Galloway's table for the analysis of Group iii.; we do not think that much reliance should be placed upon the solubility, when iron is present, of sesquioxide of chromium in cold soda-solution taken in connection with its subsequent precipitation on boiling.

With a few trifling exceptions, such as those we have noticed, the book is a thoroughly good Manual of Analysis, and can be mastered in a short time. It is highly appreciated in several large schools, and is found of very great use by Medical students.

Report of the Committee on the Decay of the Stone of the New Palace at Westminster.

THIRD NOTICE.

IMMEDIATELY following the examination of Mr. Szerelmey, the Committee were engaged in taking the evidence of Mr. Frederick Ransome, of Ipswich. It will be remembered that these gentlemen have urged rival claims in regard to the application of their respective processes to the Palace at Westminster; they were the first to undertake experiments in connection with the preservation of the stone actually forming part of the building; the results arrived at by them were capable of closer scrutiny and of more minute inquiry, and assumed, therefore, on this account a greater importance as compared with other and later suggestions which had not yet to the same extent been submitted to experimental trial.

During the month of August, 1859, the processes of Mr. Szerelmey and Mr. F. Ransome appear to have been first brought into direct competitive examination. At that period Sir Charles Barry sanctioned the treatment of the two bays on the river front, which were to be prepared each as an illustration of the capabilities of the process, with the view afterwards of applying on a larger scale that which should prove more successful in the preliminary trial. These specimens of protective workmanship have been reported on at the instance of the Government conjointly by Sir Roderick Murchison and Professor Faraday; been again inspected by the present Committee, and so frequently have formed a topic of discussion in the public journals, that it is needless now to remark more than the simple circumstance that the peculiar whitish appearance of Mr. Ransome's bay has been objected to, and that slight indications of efflorescence have made themselves apparent.

A still earlier application to the Houses of Parliament of the then recently patented process of Mr. Ransome dates as far back as the autumn of 1856, but the examples of that early period are not accounted by the inventor as successfully representing the capabilities of his mode of treatment from their having been the very first outdoor experiments of the kind, and executed without a full knowledge of all the requirements. The earliest, and at the same time satisfactory, example of the process is afforded by the Baptist Chapel, Bloomsbury, which was coated in the year 1858. The whole front of the chapel is of Caen stone, which, prior to the application of the process, is said to have been in a state of very rapid decay. Mr. Ransome refers particularly to the treatment of the Bath stone façade of the Royal Mews at Buckingham Palace as being the most successful example in London, but this having been executed so recently as 1860, cannot, we think, yet be considered as exhibiting a satisfactory indication of the value of his process. The Custom-house at Greenock, built of sandstone, is stated to be in every way a fair illustration. This was coated in 1859.

In regard to the nature of the process employed by Mr.

Ransome, we are in possession of the full particulars made known in the patent specification, and every information has most promptly been furnished by the inventor as to the materials employed and the mode of manipulation. The Committee appear also to have been assisted in their inquiries with reference to the solution of a problem of considerable experimental difficulty, viz., the determination of the depth to which the chemical preparation actually penetrates the substance of the stone. This point has not yet been properly ascertained, and we propose later to take an opportunity of resuming the discussion on this question, which will be more appropriately introduced after the nature of the materials themselves has been announced.

The object of Mr. Ransome's process is to deposit by precipitation within the pores of the stone an insoluble silicate of lime; this compound consisting wholly of imperishable mineral constituents, is intended to afford protection to the stone by rendering it less absorbent for water, and by preserving it likewise from the influence of the atmosphere. Such insoluble compound is formed by double decomposition between silicate of soda and chloride of calcium, applied as solutions and in this order to the surface of the stone. It will be seen, therefore, that besides the insoluble silicate of lime, an equivalent proportion of chloride of sodium must always be produced, and it appears probable that to this second product must partially be ascribed the tendency already referred to of causing an efflorescence on the outside faces of the stone. It was found to be possible to construct a block or kind of artificial stone from powdered materials, moistened alternately with these two solutions, and the property possessed by such blocks of resisting for a time the action of water, was conclusively demonstrated by the inventor. So great indeed is the degree of protective action communicated by the presence of the calcareous silicate, that limestone which has been thoroughly coated with this substance, is said to remain unacted upon when immersed in weak acids. In proceeding to the manufacture of "artificial stone," modes of operating have been resorted to for the formation of a compact mass, which are no longer similarly applicable to the walls of the Houses of Parliament; thus Mr. Ransome describes the interesting results obtained by exposing to heat the mixture of materials above referred to, whereby varieties of even greater insolubility were produced.

The objection which does not appear to have been conclusively answered in regard to this process, has reference to the impracticability of producing by double decomposition, especially when one of the secondary products is soluble in water, a condition of surface which is totally unaffected by the rain or moisture of the atmosphere; thus it would appear difficult so thoroughly to close the pores of the stone, as to prevent the extraction of the chloride of sodium by water coming in contact with the face of the block. If removed, cavities, gradually widening, must be formed in consequence, which would permit of a subsequent and progressively increasing action upon the stone, due to the admission of the atmosphere. Moreover, the silicate of lime itself is a precipitate, somewhat gelatinous in nature, and occupies more bulk when moist, or at the time of its formation, than when it eventually becomes dry and sinks in the pores of the stone.

NOTICES OF PATENTS.

391. *Manufacture of Paper from a Pulp obtained from Wood.* E. H. BARRÉ and C. M. J. BLONDÉL. Nantes, France. Dated February 16, 1861.

This invention relates to an improved method of effecting the disintegration of wood for the purpose of obtaining therefrom ligneous fibres suitable for the manufacture of

coarse paper. The patentees employ as the agent for facilitating the separation of the fibres nitric acid; but inasmuch as the strong acid would be liable to act too energetically upon the substance of the wood, it is preferred first to saturate the shavings or sawdust with water, and then to proceed to the treatment with nitric acid. In this manner the structure of the wood is softened, so that little mechanical aid is required to reduce it to the condition of supple ligneous fibre, and after a thorough washing, the material is ready for bleaching and general treatment like any ordinary paper pulp. But, with a view of economising the nitric acid, and at the same time obviating the inconvenience arising from the corrosive action of the acid in the wood upon the metal of the crushing rollers, it is preferred to convert the free nitric acid into nitrate of soda by adding a sufficient quantity of carbonate of soda immediately after the acid treatment. By evaporating the washing liquors a crude nitrate may be recovered, which, although containing organic matter and other impurities, will serve admirably for the preparation of new supplies of nitric acid by distillation with oil of vitriol. As the result of the oxidation by nitric acid of the resinous matters in the wood, a certain quantity of carbazotic acid is generated, which, on the addition of alkali, produces a dark brown colour; the pulp loses much of this simply by squeezing in a press, and afterwards can be thoroughly bleached by washing and the treatment with chlorine. As a means of expediting the course of these operations, the inventors prefer to employ heat in some of the processes, but several hours' crushing are requisite for the production of the finer kinds of pulp.

The addition of carbonate of soda to the woody matters while the acid is yet retained in their structural cavities, would serve an important end, which is not alluded to in the specification, although several matters of inferior moment are deemed by the patentees worthy of the full scientific explanation therein given. The principle of subjecting a vegetable structure to the alternate action of free acid and an alkaline carbonate was one originally proposed, and very successfully carried out, in connection with the manufacture of linen from flax. It was found that the carbonic acid gas liberated as the result of this action was capable of exerting a considerable amount of disruptive force, which was useful in tearing open the structure, and in facilitating the subsequent mechanical treatment for the separation of the fibres. The mode of proceeding now described differs only in the detail of substituting an expensive acid for that formerly employed.

408. *Preparation of Alkaline and Earthy Cyanides.* W. CLARK, Chancery Lane, London. A communication. Dated February 19, 1861.

This patent refers to the production from ammoniac of alkaline and earthy cyanides and ferrocyanides. The process of conversion is effected by passing a current of ammoniacal gas over a mixture of a carbonate of one of these bases with charcoal, pitch, tar, or other reducing agent, heated to dull redness in any suitable furnace. The employment of crude soda and potash, which contain a certain quantity of caustic alkali in addition to the carbonate, is specified as being particularly well adapted for producing this chemical reaction. A second claim relates to the preparation of cyanides or ferrocyanides by the reaction of a soluble sulphate upon cyanide of barium formed as above, whereby a precipitate of sulphate of baryta is produced. And, thirdly, the partial reconversion of this precipitated sulphate of baryta into carbonate by reduction, in the first place, with charcoal at a high temperature so as to form crude sulphide of barium, and passing carbonic acid gas through an aqueous solution of this product, when a certain quantity of carbonate of baryta will be regenerated and may be used in a repetition of the process.

The chain of chemical reactions called into play in the

transfer of nitrogen from crude ammoniacal matters into cyanides and ferrocyanides, is highly interesting, and likely to be susceptible of advantageous employment. From the frequency with which large proportions of sulphate of baryta are employed in the adulteration of pigments, one is almost tempted to believe that the lower commercial qualities of Prussian blue might be directly procured by the precipitation of ferrocyanide of barium with persulphate of iron.

424. *Manure.* T. RICHARDSON, Newcastle-on-Tyne.

Dated February 20, 1861.

WITH the twofold object of producing a more concentrated and soluble variety of superphosphate manure, Mr. Richardson submits to a supplementary drying treatment the ordinary product of the action of sulphuric acid upon bones, bone-ash, coprolites, apatite, guano, and other commercial forms of phosphate of lime. The process is carried out simply by exposing the mass to a degree of heat sufficient to drive off the excess of water.

There would undoubtedly be a reduction both in bulk and weight, as the consequence of the expulsion of twenty per cent. or upwards of water from the ordinary superphosphate manure, and if care be taken in guarding the material from the action of an unduly high temperature, no loss of valuable constituents is to be anticipated. The volatilization of small quantities of ammonia might be the result of over-heating.

440. *Doctors, Calico Webs, or Scrapers.* A. CROOKES and H. ROBERTS, Sheffield. Dated February 22, 1861.

THIS invention consists in a method of protecting from corrosive influences the steel or other metallic surfaces of scrapers, and other parts of machinery employed in the printing of textile fabrics. These portions of metal are to be coated with silver, copper, or tin, by galvanic or other agency, that metal being employed in each instance which most effectually resists the action of the colours, or other chemical preparations brought into contact with it.

A suggestion so extremely simple must surely be capable of general application in many similar instances.

Grants of Provisional Protection for Six Months.

1692. Richard Jolley, St. John's Street, Smithfield, London, "An improved apparatus for heating, cooling, or drying, infusing, extracting, or absorbing vapours and gases for manufacturing, medical, or domestic purposes, and for preserving liquids and solids alimentary or otherwise."—Petition recorded July 3, 1861.

1956. William Clark, Chancery Lane, London, "Improvements in bleaching and clarifying saccharine matters, and in apparatus for the same."—A communication from Edouard Théophile de Gemini and Edmond Oswald de Gemini, Boulevard St. Martin, Paris.—Petition recorded August 6, 1861.

2197. Gustav Bischof, jun., Swansea, Glamorganshire, "Improvements in extracting copper and silver from ores containing those metals."—Partly a communication from Dr. Gustav Bischof, sen., University of Bonn, Prussia.—Petition recorded September 3, 1861.

2240. George Norris, Brondesbury Terrace, Kilburn, Middlesex, "Improvements in the manufacture of soap."—Petition recorded September 7, 1861.

2303. John Reeves, New York, U.S., "Improvements in electro-magnetic engines for obtaining and applying motive power."

Notices to Proceed.

1277. Robert King and Kenneth Robson, Granville Street, Sheffield, "Consuming and destroying smoke as emitted from engine or other chimneys, and from all other flues from which smoke is emitted."—Petition recorded May 18, 1861.

MISCELLANEOUS.

Arsenical Paper-collars.—Under the signature of *Author* (City), a letter has been addressed to us respecting paper-collars, which are now so extensively worn by gentlemen. He states that some arsenical preparation is employed in their manufacture, and a case has come under his notice in which a person has been injured by their use.—*Lancet*.

Blondin and Leotard at the Polytechnic.—The scientific principles upon which Blondin and Leotard perform their wonderful feats of balancing, receive from Mr. Pepper, the popular lecturer of the Polytechnic, an exposition which creates surprise on the part of his numerous and gratified auditory that what they have looked upon with wonder should be so simple. The art of balancing is shown by a variety of models and experimental and pictorial illustrations to consist in the preservation of the centre of gravity, which is found, for the purpose of demonstration, by applying the plummet, and drawing a straight line from the apex to the base of the object whose centre of gravity it is desired to determine. So long as the line of direction falls within the base there is no chance of a tumble, as was shown by reference to the famous leaning tower at Pisa, and by experiments with the wooden swords used by conjurors. Blondin on his rope at the Crystal Palace was then introduced, through the medium of photography and the microscope, and the use of his long balancing pole explained to the audience. A practical exemplification of the art is given, on a rope stretched across the platform by John Snell, waterman and boat-builder, of Hampton, who lately performed the difficult feat of walking across the Thames on a common towing rope. Snell walks backwards as well as forward, and carries a boy on his back. The trapeze feats, which have made Leotard so potent an attraction at Cremorne and the Alhambra, are also explained, and their principle shown to be identical with that of the pendulum.

ANSWERS TO CORRESPONDENTS.

F. L. Auld.—No answer to the communication has yet reached us.

J. P.—Your letter has been received, but we scarcely think its contents applicable to our columns.

Radiant Chemical Action.—A correspondent asks for information on this subject.

H. G.—Some of the constituents of rock-oil have been used in making soap, but not to any great extent. Most of the oils are unsuitable.

Peroxide of Hydrogen is prepared by adding a paste of peroxide of barium and water to a solution of hydro-fluosilicic acid. Filter through gum-cotton and concentrate by evaporation over oil of vitriol *in vacuo*.

Reginald R.—1. Any good chemical work will describe the way to make chloride of nitrogen, but you had better not try, unless you are a most expert manipulator, for almost everyone who has yet worked with it has been injured for life by its explosive violence. 2. We do not know. 3. Oxide of iron. 4. Griffin's gas-blast furnace.

Bluish Colour to Gun-barrels.—A correspondent wishes to know what chemical application will impart a bluish colour to gun-barrels. There is a method of imparting it so successfully that the difference cannot be detected between those newly done and those which have the colour imparted to them in their manufacture.

Iodide of Potassium.—Perhaps some of our subscribers will favour us with a reply to the following:—"Sir,—Can you inform me of the method generally adopted by the large manufacturers of iodide of potassium in the production of that salt, viz., what description and size of vessels it is evaporated in, and how heated, by steam or sand-bath? What quantities do they generally produce at one operation? Do they make their iodide of iron solution in large or small quantities, and how?"

J. A. Bower.—An electric light will be indispensable in exhibiting the phenomena to a large audience at once, but the plan generally adopted at lectures is to illustrate the remarks by coloured diagrams, and to exhibit the phenomena afterwards by means of small spectroscopes. We believe spectroscopes can be hired for the night, but you had better enquire of a maker.

THE CHEMICAL NEWS.

VOL. IV. No. 99.—October 26, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Note on Fluorine, by T. L. PHIPSON, Phil. Nat. Doct. Brussels University, Member of the Chemical Society of Paris, &c.

In 1857—58 I began a series of experiments in Paris, with the view of obtaining if possible the element fluorine. I have been constantly prevented from obtaining fluorine in a state of absolute purity chiefly on account of the impurities contained in the fluoride of potassium, permanganate of potash, and other substances which I had not then means of preparing myself in any quantity. The Parisian fluoride of potassium generally gave chlorine in abundance; so did the permanganate of potash, which I used with hydrofluoric acid to oxidise its hydrogen.¹ Indeed, I am led to believe that the ozone obtained by M. Boettger with permanganate of potash was simply oxygen mixed with chlorine. Some years ago (in 1856) I thought I had obtained ozone from permanganate of potash; but on introducing a little nitrate of silver into the gas, chloride of silver was immediately formed, and the odour and action upon iodide of potassium ceased.

Concerning fluorine, all I can say at present with certainty is, that it constitutes a colourless, incombustible gas, having a peculiar odour similar to that of oysters, a great affinity for water, which it decomposes in daylight, and having no action whatever upon glass. It whitens litmus like chlorine. It can be obtained by heating hydrofluoric acid with nitric acid in a retort, when fluorine is evolved, together with a certain quantity of nitrous vapours. If this experiment is made over water, a few nitrous fumes are at first perceived and a gas is evolved, but as soon as a certain quantity of fluorine is produced, the water rushes into the retort as if into a void; the whole apparatus is filled so quickly that it is impossible to perceive the passage of the liquid. This accident can be prevented by means of a proper tube. When a solution of fluoride of silver is decomposed by a current of chlorine gas, the chloride of silver precipitated contains fluorine, and does not blacken under the influence of the solar rays; when exposed to the light it becomes of a light brown tint, which is permanent even in sunlight. In this experiment a large portion of fluorine remains dissolved in the liquid, but it gradually decomposes the water, evolving large bubbles of oxygen gas, which appear to rise from the precipitate at the same time and not before the glass becomes attacked by the hydrofluoric acid formed.

Another mode of obtaining fluorine consists in heating with precaution a mixture of pure fluoride of calcium,

¹ I have at last obtained some fluoride of sodium chemically pure. It was prepared by Mr. J. Christian, chemist to Messrs. Jackson and Townson, of Bishopsgate Street, London, and with it I intend to continue the experiments related in this paper, when circumstances will permit me to do so.

a solution of permanganate of potash, and sulphuric acid. The gas evolved has a peculiar odour, something like that of oysters; it whitens litmus, dissolves in water, and does not attack glass, neither is the glass retort corroded in which the operation takes place. If peroxide of manganese is used, so much hydrofluoric acid is produced that, on the contrary, the whole apparatus is soon spoiled. By passing the gas obtained as above into a solution of soda I obtained a crystalline salt, which I take to be fluorate of soda, but which I have not yet examined thoroughly.

When a mixture of nitric acid, solution of permanganate of potash, and fluoride of potassium dissolved in water, is abandoned to itself for some days, a peculiar and very beautiful crystalline compound is formed: it consists of numberless small, flat, crystalline plates, of a golden bronze colour and metallic aspect, very much resembling certain varieties of mica; indeed this compound contains potassium, manganese, fluorine, and silicium, elements which we find in micas, and I look upon these crystalline plates as a sort of artificial mica.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 208.)

Determination of the Relation between the Atomic Weights of Nitrogen, Chlorine, Sulphur, Potassium, Sodium, Silver, and Lead.—In order to ascertain the reciprocal relations between the atomic weights of these seven bodies, I have performed the synthesis of,—

1. Chloride of Silver,
 2. Sulphide of Silver,
 3. Nitrate of Silver,
 4. Nitrate of Lead,
 5. Sulphate of Lead,
- The analysis of,—
6. Chlorate of Potassa,
 7. Sulphate of Silver;
- and I have sought for the proportional number between,—
8. Silver and Chloride of Potassium,
 9. Silver and Chloride of Sodium,
 10. Silver and Chloride of Ammonium,
 11. Nitrate of Silver and Chloride of Potassium,
 12. Nitrate of Silver and Chloride of Ammonium.

By thus balancing simple bodies with simple bodies, simple bodies with compound bodies, and compound bodies with each other, I naturally arrive at results which can be controlled by each other.

Some of these determinations were undertaken many years ago; they have all been verified and completed during the last three years. I will commence the details of these researches with those which depend upon the metal silver, treating first of the preparation of pure silver, and the methods which I employed to assure myself of its purity.

Silver.—As silver has been the pivot, so to speak, of my researches, I have made every possible effort to procure this metal in a state of purity, and to make myself sure of its purity. For this purpose I have successively employed all the methods recommended to prepare pure silver. I have arrived at the certain conclusion, that all those which are capable of being executed on a large scale furnish an impure metal unless important modifications are introduced. All processes which depend upon the reduction of chloride of silver yield a metal containing copper and iron, unless, indeed, it has been redissolved three or four times successively in nitric acid, the solution, after diluting with twenty or thirty times its weight of water, being each time poured into aqueous hydrochloric acid, and the chloride of silver violently agitated in the liquid, as in the process of assaying. Experience has shown me that chloride of silver, free from copper and iron, can be obtained directly by pouring a cold solution of nitrate of silver diluted with thirty times its weight of water, into a slight excess of hydrochloric acid, washing the precipitate with cold distilled water, and then digesting the chloride, dried at the ordinary temperature, and finely powdered, in aqua regia. When well washed after this treatment, the chloride does not retain the slightest trace of either copper or iron; whilst, so long as the chloride of silver is in a curdy form, it retains in its pores, like coagulated albumen, some of the bodies which were dissolved in the liquid from which it was precipitated. Chloride of silver, however, when dried at the ordinary temperature and finely powdered, very easily yields to aqua regia foreign metals which contaminate it.

Whatever may be the purity of chloride of silver, it produces a metal which always contains silicium and iron when it is reduced by Gay-Lussac's method; that is to say, a mixture of chalk and charcoal.

The presence of these foreign matters is easily ascertained by dissolving a hundred grammes of silver in pure nitric acid in a platinum dish, and evaporating and fusing the nitrate. On dissolving the salt in cold water there is always a residue of silicic acid and sesquioxide of iron.

I have found as much as $\frac{1}{100000}$ ths of silicium in silver reduced from the chloride by Gay-Lussac's process. M. Allard, Director of the Mint at Brussels, has kindly caused to be prepared for me in the laboratories of the Mint large quantities of silver by this plan, as recommended for obtaining silver for assay. In spite of all the care taken by M. Schoonjans, whose great skill I have had occasion to notice, the silver so produced contained traces of copper, iron, and especially of silicium. Upon analysing with extreme care the silver obtained by this method, I found when operating upon 200 grammes that after one treatment it contained between $\frac{1}{100000}$ ths and $\frac{1}{100000}$ ths of impurities; after two treatments, between $\frac{1}{100000}$ ths and $\frac{1}{100000}$ ths; after three treatments, between $\frac{1}{100000}$ ths and $\frac{1}{100000}$ ths. A comparison of this silver with metal obtained by other processes has led me to absolutely the same result. Amongst the determinations of the reciprocal relation between silver and chloride of potassium, there are three which were made with silver prepared by Gay-Lussac's method, and the impurities then make themselves apparent in a very evident manner. M. Dumas has estimated at a ten thousandth part the impurities contained in the silver supplied to him by the Paris Mint.

Without confidently asserting it, I suspect that the presence of so large a quantity of silicium in the metal so prepared is due to the action which silver has upon

silicic acid. It is probable that at the temperature necessary for fusion silver reduces the silicic acid with formation of silicate and silicide of silver. Furthermore, the presence of carbon may favour the reduction of silicic acid and the formation of silicide of silver. One thing is certain, as I have frequently proved, that the vapour of silver attacks silicic acid and the silicates. White porcelain becomes coloured yellow or yellowish-brown, and increases very sensibly in weight, when there is directed upon it the vapour of silver disseminated by an oxidising flame.

Chloride of silver, purified by the above process, mixed with its own weight of pure dry carbonate of soda, containing a tenth part of pure nitrate of potash, when heated in a white unglazed porcelain crucible, with the precautions recommended by Berzelius for avoiding intumescence, yields an ingot of pure silver. This ingot, fused again with a tenth of its weight of pure nitre and borax, and then run into an ingot-mould lined with pipeclay, gives a bar of silver which retains scarcely any appreciable traces of foreign matters. This process requires great care, for when the mixture of chloride and carbonate is heated, if the temperature is raised too much at first, the mixture fuses, bubbles up, and is in danger of running over. I have, however, prepared in this way many kilogrammes of silver.

To effect with safety the reduction of chloride of silver in a white unglazed porcelain crucible, the latter should be placed inside a clay crucible. This is the way I have found most convenient for performing this operation:—I fill up the space between the two crucibles with calcined pipe-clay, powdered and mixed with 5 per cent. of fused and powdered borax. Under the influence of the heat, the borax fuses and solders the whole together. When the chloride of silver is reduced, the whole can be handled and the melted silver poured out as if it were one crucible. The great bulk which has to be heated before reaching the porcelain crucible prevents it cracking, and avoids loss of silver.

Besides this method of preparing silver, I have tried many other plans. One of them has been suggested to me by M. Liebig, who, indeed, has kindly prepared a certain quantity by this means for my researches. The process consists in reducing in the cold, by means of pure milk sugar, a pure concentrated ammoniacal solution of nitrate of silver, to which pure potash has been added until fulminating silver begins to be precipitated. After a short time a violet precipitate is formed, which is transformed into a mirror of silver, if the solution does not contain more than 10 per cent. of nitrate. If, on the contrary, it contains much more metal, the violet precipitate of silver remains. This precipitate, after being washed with water, is digested with aqueous ammonia, which removes the copper, if the silver contained any. When dried, it preserves its violet colour, and constitutes a peculiar modification of silver. Heated to 300° or 350°, the metal becomes incandescent, and then assumes its proper colour, being of a dead white. To reduce it to bars, I fuse it with a certain quantity of pure nitre and borax, and run it into an ingot-mould lined with pipe-clay.

I have prepared by this means more than three kilogrammes of pure silver, working upon a kilogramme at a time. I have always found the metal of a uniform purity, and identical with other pure silver. I say, with other pure silver, because it is not sufficient, as M. Péligot states in his memoir upon silver, to prove that the metal obtained by a certain process always has the same qualities, in order to be sure of its purity; it is

also necessary for it to be absolutely identical with pure silver prepared by other methods. Moreover, it may happen,—and this is the case in the reduction of pure chloride of silver by Gay-Lussac's process,—that the operation of reduction communicates to the metal as much impurity as has been separated by solution and precipitation.

I will now give the methods I have employed to obtain the silver with which I have controlled the metal used in my operations. I give them all, stating, however, that they are too tedious and furnish too small a quantity of silver to be used on a large scale.

The first plan consists in procuring the metal by the electrolysis of pure argentocyanide of potassium or of ammonium. I have prepared at least half a kilogramme by this plan, which is long and very costly. The deposit is made upon a surface of porcelain previously covered with a mirror of silver, by Liebig's method. For a positive electrode coke is used, obtained by heating the vapour of naphtha to redness. To obtain a nitrate of silver fit to prepare the cyanide, I dissolved in nitric acid the silver of $\frac{999}{1000}$ ths produced at the Brussels Mint. The solution was evaporated to dryness and the salt fused. After cooling it was powdered and digested in cold water, taking care not to dissolve it all, for otherwise oxide of copper would come into solution. The solution of silver was allowed to stand for three or four days, filtered through double filter-paper, then digested with an excess of oxide of silver, and allowed to remain at rest for a sufficient time. This solution is diluted with water until it only contains a thirtieth part of its weight of nitrate, and poured into pure aqueous hydrocyanic acid until cyanide is no longer precipitated. The cyanide is shaken in the liquid to finely divide it, and then washed with water acidulated with nitric acid, and finally with pure water. The cyanide is diffused in an amount of aqueous cyanic acid equal to that used in its precipitation, and then pure ammonia or potassa is added to the mixture until the cyanide is all dissolved. When undergoing electrolysis the positive pole of carbon is surrounded with cyanide of silver, contained in a linen bag, purified by means of hydrochloric acid. I thus return silver to the liquid as fast as it is removed by electrolysis. I have been unable to find any foreign body in this silver after it has been fused in an unglazed porcelain crucible with a mixture of purified nitre and borax.

I have likewise made use, to prepare pure silver, of the reaction between finely-divided phosphorus upon a one-per-cent. solution of nitrate of silver. This action is very slow, but the metal, after having remained for a long time in an excess of solution of silver, and then being digested in ammoniacal water, yields, after fusion with purified nitre and borax, silver absolutely pure. By this reaction I obtained the first pure silver that I ever possessed.

Finally, I have prepared silver by the action of heat upon the acetate, which had been re-crystallised at least ten times. This metal, after fusion with nitre and borax, has appeared not so pure as the preceding.

The process recently described by M. Cavanna gives a very pure metal, but it is impracticable on the large scale. M. Péligot has already made the same remark.

I have discovered a very simple plan for ascertaining the purity of silver. The pure metal remains melted in the air at a sufficiently high temperature to volatilise it without being covered by any scum or coloration, and without giving a coloured vapour. Silver containing no more than the five hundred-thousandth part of iron,

copper, or silicium becomes covered with a very strong and mobile scum when it is fused before a blow-pipe, fed with a mixture of illuminating gas or hydrogen and an excess of air. Silver containing scarcely appreciable traces of copper when volatilising in an oxidising flame always gives a coloured flame. This assay may be performed on charcoal or white-burned pipe-clay, or on porcelain, by means of a gas blow-pipe or a simple eolipyle. The scoria derived from the impurities in the metal always forms upon the surface of the flattened spheroid caused by the fusion. After cooling, the foreign matter is found adhering to the silver near the point of contact of the metal with the support.

All the specimens of assay silver that I have been able to procure have yielded scoria more or less evident. This amount of impurity, however, in no way interferes with the results of the assays made at the Mint.

I have already said that I have ascertained by direct analysis the quantity and quality of the foreign matters present in silver prepared by Gay-Lussac's process, as described. As this method necessitates the sacrifice of 200 grammes of silver in each assay, I have replaced it by the wet process of assay, as used in Mints, taking the precaution to increase tenfold the quantity of metal experimented upon. For this purpose I have devised an apparatus, of which a drawing is given in my Memoir. It consists of two distinct parts,—a large glass reservoir, filled with a normal saline solution, and a large pipette, terminated at the upper part by a tube of about five or six millimètres internal diameter, to which is cemented a stop-cock, and at the lower part by a tube of two millimètres internal diameter. This pipette is replenished from below, and is fixed in a case filled with water kept at a uniform temperature. Filled twice up to a mark scratched on the upper tube, it contains at 10°2 an amount of common salt sufficient to precipitate ten grammes of the purest silver I have been able to procure. I will not now enter into the details of these experiments. I will give an account of all the precautions which I have taken to ensure the almost absolute accuracy of the results, when speaking of the proportional relation between silver and chloride of potassium, sodium, &c.

Before leaving this subject, I must still mention the form in which I have employed the silver used in my experiments. All the metal has been fused and cast in an ingot-mould, lined with white pipe-clay. To detach the pipe-clay, the surface of the bars is rubbed with sharp white sand. They are then heated to dull redness and covered over with caustic potassa, which is kept in a fused state for at least a quarter of an hour. The adherent pipe-clay being in this manner attacked, the bars are suddenly plunged into water. The silicate formed on the surface being thus detached, the bars, after a second scrubbing with sharp sand, are fit for use. These bars, before being employed, are treated with boiling hydrochloric acid, washed first in cold aqueous ammonia, then in pure water, and finally heated to redness on plates of pure silver.

My experiments have rendered it necessary for the silver to be in the form of small lumps, from two to twenty-five grammes in weight, in the form of sheet, and in a certain state of division to make up weights previously calculated. I obtained the small lumps by cutting the ingots with a chisel on an anvil of polished cast steel. The turnings and filings obtained from the ingots by means of a lathe and file have furnished the finely-divided silver. To separate the iron adhering to the silver when cut with the chisel, lathe, or file, I digest these different forms of the metal in a close vessel for

twenty-four hours, and at a temperature of 60° to 80° , first with strong hydrochloric acid, and then with pure ammonia. The silver is finally washed with absolutely pure water, heated to redness on a plate of pure silver, and immediately put into bottles with well-fitting stoppers.

Having found that the silver, after having been rolled at the excellent mills belonging to our Mint, contained iron, which was absent before rolling, I took the precaution to have all the silver which I required in the form of sheet rolled between two plates of pure silver. It is only at the expense of the surfaces of the two plates which touch the cylinders of the mill that I have been enabled to protect the surface of the inner plate from contamination with all foreign metal. The plates of silver employed in the synthesis of the sulphide and in some of the syntheses of the chloride of silver were thus obtained. The laminated silver was likewise heated to redness in the air, and enclosed whilst very warm in a well-stoppered bottle.

I thought for a long time that, after having taken all the precautions detailed above, I had succeeded in obtaining silver absolutely pure. An experiment has, however, undeceived me. When performing the synthesis of nitrate of silver upon about 400 grammes of this metal, I have still found the presence of some traces of silicium, not, however, more than 2 parts in 100,000.

(To be continued.)

Analytical Notes, by M. H. ROSE.

Precipitation of Mercury in the State of Protochloride by Phosphorous Acid.—The best method for estimating mercury is to precipitate it in the state of protochloride by phosphorous acid. When the liquid contains free hydrochloric acid the temperature may be raised to 60° without the protochloride of mercury being reduced to the metallic state by phosphorous acid. The precipitated protochloride is not immediately formed in very weak liquids; it is necessary to leave the mixture undisturbed for twelve hours. The chloride deposits itself readily, especially when the liquid is sufficiently acid.

The precipitate is to be collected on a filter, washed with cold, or even hot water, and dried at 100° . This process is very applicable to cases where the liquid contains much nitric acid. Only the solution must then be diluted with a sufficient quantity of water.

The mercury can be easily separated by hydrochloric acid and phosphorous acid from copper, cadmium, zinc, and even bismuth, antimony, and arsenic. When the liquid contains bismuth a large excess of hydrochloric acid must be added to prevent the precipitation of oxichloride of bismuth. The mercury being separated in the state of protochloride, the bismuth is precipitated in the filtered liquid as oxichloride, then reduced to the metallic state by fusion with cyanide of potassium.

If the liquid contain antimony, this body is to be retained in solution with tartaric acid, which does not hinder the precipitation of the mercurial chloride.

Separation of Mercury and Lead.—The preceding method is not applicable to the separation of mercury and lead. A portion of the lead is precipitated in the state of chloride with the protochloride of mercury. A better plan for analysing a mixture of the salts of these two metals is to add sulphuric acid, and then alcohol forming about $\frac{1}{3}$ th of the volume of the liquid. If this does not contain sufficient hydrochloric acid, and if the proportion of sulphuric acid is insufficient, yellow sub-

sulphate of mercury may be precipitated, which is avoided by the addition of sulphuric acid. The sulphate of lead requires washing with weak alcohol acidulated with sulphuric acid.

Separation of Mercury and Silver.—Add a sufficient quantity of hydrochloric acid to the diluted solution. After the deposition of chloride of silver, decant the supernatant liquid; then heat the chloride precipitate with a small quantity of nitric acid, add some water and a few drops of hydrochloric acid, and then filter. Precipitate the mercury from the filtered liquid by phosphorous acid.

Reduction of Mercuric Chloride by Alkaline Formiates.—M. de Bonsdorff proposed to precipitate mercury as protochloride by alkaline formiates. This method may lead to completely erroneous results, as alkaline formiates do not invariably reduce bichloride of mercury. The affinity of mercuric chloride for alkaline chlorides is so great that when these bodies are present the ordinary reactions of mercury are not produced. When a liquid contains much hydrochloric acid, an alkali does not precipitate oxide of mercury from it. Sulphuric and nitric acids have the same effect. Such a mercuric solution, containing a large excess of potassa, is not precipitated by sulphide of ammonium unless the base has been previously supersaturated with an acid.

The same remark applies to the chlorides of other precious metals, particularly to chloride of gold. In presence of a considerable excess of hydrochloric acid or alkaline chlorides, a small quantity of salt of protoxide of iron or oxalic acid does not suffice to reduce chloride of gold. A large excess of the re-agent is necessary.

Estimation of Mercury in its Solid Combinations by Distillation.—In estimating mercury by distillation it is necessary, especially if the metal is in the state of chloride or sulphide, to take certain precautions, without which a portion of the sulphide or chloride would volatilise without decomposition. The operation is conducted by introducing into a glass tube capable of resisting fusion, closed at one end, and measuring from 35 to 50 centimètres in length, a column of bicarbonate of soda, then one of quick lime, and then a well blended mixture of a mercurial salt and quick lime, and finally a column of quick lime. The open end of the tube is drawn out and bent round so as to enter a small flask containing water. The tube is heated as if for an organic analysis, commencing at the open end and finishing with the bicarbonate of soda. The operation ended, cut the bent end of the tube. Collect all the mercury in the flask, dry with paper, and afterwards over sulphuric acid, and then weigh it.

There is no need to replace the quick lime by hydrate of lime. That would occasion all the inconvenience of an analysis of a sulphuretted combination of mercury. The water, acting on the sulphide of calcium, would form sulphuretted hydrogen, which, by dissolving in the water of the receiver, would in time transform a portion of the reduced mercury into sulphide. It is even advisable in this case to replace the bicarbonate of soda by carbonate of magnesia.

Combinations containing iodide of mercury are not entirely decomposed when treated as above. Biniodide and protoiodide are condensed in the extremity of the tube simultaneously with the metallic mercury.

To analyse these combinations recourse must be had to metallic copper, the operation being similar to that with quick lime.—*Poggendorff's Annalen.*

On Rubidium and Cesium,¹ by M. BUNSEN.

IN addition to the facts which we have already published on the subject of these two metals, discovered by M. Bunsen, by means of spectrum analysis, we add the following:—

The equivalent of rubidium is 85.36 ($H = 1$), more than double that of potassium; metallic rubidium is even more electro-positive than potassium,—it amalgamates easily and decomposes water in the cold.

The hydrate of rubidium, $RbO.HO + Aq$, is soluble in all proportions in water and alcohol. When heated it fuses in its water of crystallisation, becoming reduced to the state of $RbO.HO$, which is fusible at a higher temperature without losing weight. It acts like caustic potash, attracting water and carbonic acid from the air, and dissolves in water with disengagement of heat.

The carbonate, $RbO.CO_2 + Aq$, forms ill-defined crystals, of strong alkaline reaction, fusible by heat in their water of crystallisation, and becoming anhydrous. This salt is fusible, deliquescent, and attracting from the air a second equivalent of carbonic acid. The aqueous solution saponifies the skin exactly like carbonate of potash.

The bicarbonate, $RbO.CO_2 + HO$, forms brilliant prisms, unalterable in the air, having a slight alkaline reaction, and a saline taste not at all caustic.

The nitrate, $RbO.NO_3$, does not crystallise in rhombohedral prisms like saltpetre, but in prisms with a hexagonal base, which is a second form of nitrate of potash. It dissolves in 5 parts of water at $0^\circ C$, and in 2.3 parts of water at $10^\circ C$.

The sulphate, $RbO.SO_3$, is isomorphous with that of potash. At $7^\circ C$ it dissolves in 2.4 times its weight of water, and is, consequently, more soluble than sulphate of potash, which requires 10.4 times its weight of water to dissolve it. With sulphate of magnesia it gives a beautiful octohedral salt, and with sulphate of cobalt another double salt, in fine crystals, isomorphous with $CoO.SO_3 + KO.SO_3 + 6HO$.

The bisulphate of rubidium can be fused at a moderate temperature without decomposition, but at a higher temperature it is reduced to the neutral salt.

The chloride of rubidium is cubic, but crystallises with difficulty. It is fusible and volatilisable at the extremity of a platinum wire. From 1° to $7^\circ C$ it dissolves in 1.2 to 1.3 parts of water; KCl requires 3.4.

The double chloride of platinum and rubidium, $RbCl.PtCl_2$, is a crystalline powder composed of microscopic octohedra. It only differs from the double chloride of platinum and potassium by its less solubility; it only dissolves, indeed, in 1.8 times its weight of boiling water, whereas the potassium salt only requires 1.9 times its weight.

Cesium (from *cæsius*, sky-blue, on account of the colour of the ray which it produces in the spectrum). The double chloride which it forms with platinum is still less soluble than that of rubidium. Its equivalent ranks by the side of those of iodine and gold, 128.4, and is, consequently, one of the highest.

The hydrate of cesium, $CsO.HO + Aq$, is deliquescent. When heated it loses an equivalent of water, attacks platinum, volatilises in a spirit-flame, and easily dissolves in alcohol.

The carbonate, $CsO.CO_2 + Aq$, crystallises badly at $19^\circ C$. It dissolves in 9 times, and at $78.4^\circ C$ in 5 times its weight of absolute alcohol. It is very caustic, liquefies

in the air, and is transformed into bicarbonate. It is soluble in all proportions in hot water.

The bicarbonate crystallises in brilliant prisms.

The nitrate is anhydrous, like that of potash, with which it is isomorphous. It has the taste of saltpetre, as also has the nitrate of rubidium.

The sulphate of cesium is also anhydrous, and crystallises badly. At $2^\circ C$, 1 part of this salt requires 0.63 parts of water to dissolve it. It forms double salts with the magnesian sulphates, and these double salts are isomorphous with those formed with the same sulphates and the sulphates of potash and ammonia. With sulphate of alumina the sulphate of cesium forms a very brilliant octohedral alum.

The chloride crystallises in cubes, but it is distinguished from those of potassium and rubidium by being deliquescent, like the chloride of lithium. It is fusible and volatile at a comparatively moderate temperature.

Cesium is less abundant than rubidium, which, however it generally accompanies; the richest source is the mineral water of Durckheim. 10 kilogrammes of this water contain 2 milligrammes of chloride of cesium; and it has been from the residue of the evaporation of this water that I have prepared it, by precipitating it with bichloride of platinum, and submitting the alkaline chloroplatinate to successive exhaustions with boiling water until only the chloroplatinate of cesium was left, this, as we have seen, being the most insoluble of all, and possessing also the highest equivalent.

The lepidolites contain only traces of cesium, but, on the contrary, they are rich in rubidium, and it is from these that M. Bunsen has prepared the latter. The lepidolite from Rosena (Moravia) contains two-thousandths of its weight of oxide of rubidium, and that from Saxony appears still richer. Probably all the saline mineral waters contain it; that of Durckheim contains two-millionths, and the mother-liquor about four hundred-thousandths of chloride of rubidium. Commercial potash, however, does not always contain it.

On the Preparation of Nitrate and Nitrite of Ethyl,
by M. CAREY LEA.

Nitrate of Ethyl.—The tendency of alcohol to decompose nitric acid is so great that while we have any number of processes for obtaining nitrite of ethyl, Millon's is the only one known for forming the nitrate. But Millon's process fails much oftener than it succeeds (such, at least, has been my experience), in consequence of the proportion of urea directed to be used being much too small.

Millon's process, as quoted in Kolbe's "Organische Chemie," directs to distill equal weights of alcohol and of colourless nitric acid, specific gravity 1.401. Not to operate upon larger quantities than 120 to 150 grammes, and to add a little urea to the above quantity, at most ("höchstens") 2 grammes of urea.

Conducted as above, the operation succeeded with me but once out of four or five times. But by using a large quantity of urea, three or four times as much, or even more, success invariably follows, and there results a great saving of urea, because when the process fails, the materials, urea included, are entirely lost, whereas when conducted as just described, very little urea is consumed. It is only necessary to add fresh acid and alcohol, after the first proportion has been distilled to about one-fifth, and the process may be repeated almost indefinitely, with the occasional addition of a little urea to keep up

¹ *Annal. der Physik und Chem.*, t. cxlii., p. 337.

the proportion. The product in the second and subsequent operations is much larger than in the first.

Nor have I found it necessary to operate on the small quantities above mentioned, but have habitually used quantities amounting to 400 to 500 grammes, and should not hesitate to employ still larger ones. It is, however, more convenient to use the above quantity, and replace the material as fast as exhausted.

The facility with which nitrate of ethyl can be obtained by the above process must, I think, give it a decided advantage over the iodide of ethyl for the preparation of the ethyl bases, in the manner which I have recently described.¹ The lower equivalent of nitric acid as compared with iodine is a matter of considerable importance; 100 parts of iodine are capable of holding in combination something less than 23 of ethyl, whereas 100 parts of nitric acid combine with over 53 of ethyl,—a material point, as the iodine on the one hand and the nitric acid on the other are merely the vehicles for bringing about the ethyl substitution. In operations in pressure-tubes, where space is valuable, this is an important consideration; in an economical point of view also the advantage appears to be greatly on the side of the nitrate of ethyl.

Nitrite of Ethyl.—The action of nitric acid on alcohol in the preparation of nitrite of ethyl may be conveniently moderated by protosulphate of iron, and the nitric acid reduced by that agent instead of at the expense of part of the alcohol. The proportions which I have used are, nitric acid, specific gravity 1.37, 90 cubic centimetres; alcohol, of 90 per cent., 150 cubic centimetres; ferrous sulphate, 45 grammes. The product is very large; the distillate, on being shaken up with water, yields more than half its bulk of nitrite of ethyl. The latter is not perfectly free from aldehyde, but more so than that obtained by some other processes. The action is perfectly quiet to the last, and the distillation may be carried on rapidly.—*American Journal of Science*, No. 95.

TECHNICAL CHEMISTRY.

On the Artificial Colouring Matters derived from Aniline,
by M. E. KOPP.

(Continued from page 210.)

Believing in the identity of fuchsine and azaleine, MM. Persoz, de Luynes, and Salvétat endeavoured to separate the pure colouring matter from them in the following way:—They mixed each of the products with six or seven times its volume of alcohol, and then filtered. To each alcoholic solution they added pure ammonia as long as any precipitate was formed, and the liquor became colourless or of a light amber. They then filtered again to separate the precipitate, which in the case of fuchsine consisted of stannic oxide, and, in the case of azaleine, of resinous matter. The ammoniacal solutions contained the whole of the colouring matter. These were saturated with acetic acid, and then a saturated solution of bichromate of potash was added, which produced a red precipitate. This was collected and carefully washed. As long as the washings contain saline matters they run away uncoloured, except by the bichromate; but as soon as the saline matter is gone, the precipitate begins to dissolve. If, now, the washing be made with hot water, a solution is obtained charged with the colouring matter,

which is the same, whatever may have been its origin. Baryta, or lime, may be substituted for ammonia in the above experiment.

The authors of this report, without denying that the colouring matter may at times play the part of a base, contend that it quite as often, and perhaps oftener, plays the part of an acid. In fact, under the influence of acids the fuchsine, or rather fuchsic acid, always dyes, for under these circumstances it is free. But fuchsic acid, combined with powerful bases, such as ammonia, potash, and soda, forms colourless compounds, which only dye when an acid is present to displace the base and fit it to unite with the fabrics. Fuchsic acid, indeed, behaves under these circumstances exactly like carthamic acid.

The aqueous solution of the colouring matter prepared in the method before described behaves with re-agents in the following manner:—Caustic alkalies and the alkaline carbonates remove without destroying the colour, for it re-appears on the addition of an acid. Chloride of platinum at first produces only a cloudiness in the solution, but, after a time, a purple precipitate forms. Chloride of gold gives a deep purple precipitate immediately. Sulphocyanide of potassium, after some time, gives a precipitate like purple of Cassius. Stannous chloride in the cold almost decolorises the solution; warm, it gives a colour like wine lees, and a few drops of acetate of soda precipitates a rose-coloured lake. Stannic chloride in the cold modifies the substance, altering the colour, the change being more pronounced when a warm solution is used, the precipitate lake being of a dirty rose colour. Manganate and permanganate of potash produce no precipitate, but give a fawn colour to the solution. Hyposulphite of soda gives a violet shade to the colour. Chlorine at first blues and then decolorises the solution. Chloride of lime partially decolorises it in the cold, and completely when warm, the stannous chloride no longer reproducing the colour. Saturated solutions of alkaline salts precipitate the liquor, as soap is precipitated by some saline matters. Sulphurous acid decolorises the solution, but an oxidising agent carefully employed restores the colour.

The purified colouring matter from fuchsine contains no chlorine, and, whatever the origin of the red matter, it always contains about 70 per cent. of carbon and 13 per cent. of nitrogen.

The distinguishing characters of the red colouring matter, fuchsine, and the violet colouring matter, indisine, may be summed up as follows:—

Fuchsine is soluble in ammonia; indisine is not. Fuchsine forms a yellow solution in sulphuric acid; indisine gives a blue. Fuchsine is decolorised by sulphurous acid; indisine is not changed. Lastly, indisine, being the result of the oxidation of aniline, which is one of the derivatives of indigo, presents so many analogies with the last body, that, forgetting for the moment that indisine is a violet colour, it might be taken for indigo when witnessing in it all the phenomena of oxidation and reduction so easily effected with indigo.

M. Kopp objects to the use of bichromate of potash in the process for the purification of fuchsine and azaleine proposed by MM. Persoz, de Luynes and Salvétat, and thinks it possible that the action of this body may produce the identity which they seek to establish. Is it not possible, he asks, that the colouring matters not identical, yet extremely alike in composition and properties, may, under the influence of such an agent as bichromate of potash, be converted into one and the same compound, just as starch, dextrine, and various

¹ CHEMICAL NEWS, August 17, 1861, No. 89, Vol. IV. p. 38.

kinds of sugar are converted into oxalic acid by the action of an oxidiser? M. Kopp, therefore, prefers a method of purification in which only entirely neutral bodies, such as sulphide of carbon, benzine, and alcohol, are made use of. A process of this kind has been invented by M. Schneider for the purification of fuchsine and azaleine. To purify azaleine, M. Schneider first rubs together in a mortar, for about ten minutes, three parts of the rough product and three or four parts of cold water. The mixture is then allowed to stand for an hour, and then the supernatant liquor is poured away. This washing is repeated at least six times, the object being to get completely rid of the salts of mercury and aniline, more or less of which are always present. A little of the colouring matter is lost in this way, but not much. The azaleine must next be dried. It is then rubbed with about an equal quantity of sulphide of carbon for ten minutes. At first the colouring matter is very sticky and difficult to rub, but by degrees it becomes almost pulverulent, and then yields nothing more to the sulphide, which may be poured away. This operation is repeated several times. The mixture may then be thrown on a filter to drain. What remains on the filter is dried in the air, reduced to an impalpable powder, and then treated with pure benzole seven or eight times, or as long as the benzole becomes coloured. The powder is then dried again in the air. Lastly, 40 grammes of this dry impalpable powder are treated with 400 grammes of alcohol. A quarter of the alcohol is added at a time, and, after stirring together for about ten minutes, the solution is poured off into a large bottle, and another quarter of the alcohol is added, and so on, to dissolve the whole of the colouring matter. 440 grammes of cold distilled water is now added, the mixture well shaken, and then allowed to stand until the following day. All the foreign matters and some of the colouring matter are precipitated by the distilled water. After filtering, the solution of the now perfectly pure colouring matter is evaporated on a water-bath to about a sixth of its bulk, and then allowed to cool. The mother liquor is decanted on the following day, and at the bottom of the dish will be found a crystalline pellicle, scattered over with brilliant scales. The whole is dried, in the air for a couple of days, and finally on a water-bath.

The process for purification of fuchsine differs but little from the above. Fuchsine is much more soluble in water than azaleine; to avoid loss, therefore, it is necessary to use iced water in the first washing. The mass is then exhausted with alcohol, which leaves 30 or 40 per cent. of insoluble residue. The alcoholic solution is then evaporated or distilled to dryness, and the dry product treated as in the case of azaleine.

Passing from the red and violet-red colouring matters, M. Kopp proceeds to notice the reports of MM. Wilm and Scheurer-Kestner on aniline violet. This is obtained by the action of bichromate of potash (Perkins) or peroxide of manganese (Kaye) on a salt of aniline, or, preferably, by adding hydrochlorate of aniline to a solution of chloride of lime (Beale and Kirkham). The violet precipitate in these cases is accompanied by a brown colouring matter and a resinous body. The brown matter is the most abundant product, and the greater part of it remains in solution in the mother liquor; it dyes silk and wool a maroon colour without the intervention of a mordant; it is easily separated from the violet in consequence of its solubility in alkalies, in which the violet matter is insoluble.

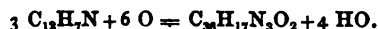
The resinous body is insoluble in water, weak alcohol, and acetic acid, but soluble in absolute alcohol, sulphide

of carbon, ether, and benzole. As the violet matter is insoluble in the three last named, they may be used to separate the resinous matter from a mixture of the two. Aniline violet itself is soluble in boiling water, alcohol, wood-spirit, acetone, aniline, and acetic, hydrochloric, sulphuric, and tartaric acids. Water, and especially when charged with neutral saline matters, caustic alkalies, or even metallic salts, precipitates it from these solutions. In the latter cases, the colouring does not combine with the metallic oxides to form lakes.

The alcoholic violet solution is decolorised by chlorine water, but the colour is restored by sulphurous acid; sulphurous acid and the sulphites do not decolorise it; but the colour is discharged by the combined action of zinc and sulphurous acid. Chlorine now restores the colour, but with some alteration; it is redder and no longer possesses the violet shade.

Heated on a plate of metal, dry aniline violet appears to volatilise and give red vapours; but crystals cannot be obtained by sublimation either *in vacuo* or under the ordinary pressure of the atmosphere. It seems to decompose into aniline (which carries away with it some of the violet matter unchanged) and a good deal of carbon. A solution of the pure violet matter in strong acetic acid yields prismatic crystals, which, when dried, have a more brilliant green shade than the dried amorphous mass from the evaporation of the alcoholic solution.

The analysis of aniline violet by Wilm yielded results from which he calculated its formula to be $C_{20}H_{17}N_3O_2$; and the reaction which takes place in its formation he represents as



The analyses of Scheurer-Kestner, however, gave results which led to the formula $C_{30}H_{14}N_2O_2$, according to which the reaction will be



On some Applications of Ebonite, or Hard India-Rubber,¹ by H. A. SILVER.

THE material known as hard india-rubber, vulcanite, horn-rubber, or, in its most perfect condition, as ebonite, was first obtained by Hancock, although its discovery was also effected by the independent researches of Goodyear. Ebonite consists of india-rubber, vulcanised with sulphur at a high temperature, which is continued for several hours. As in the case of caoutchouc, or pure india-rubber, the applications of this *matériel* were, until lately, extremely limited. It is principally met with in the form of combs, paper-knives, and penholders; but the peculiar properties it possessed render it available for far more important uses. Its acoustic qualities have been utilised in the construction of stethoscopes, and render it well adapted for ear-trumpets and for flutes, and several other wind instruments. Like horn or ebony it may be turned in a lathe, although in practice it is found preferable to produce it in moulds of any desired form. It is a most powerful, probably the most powerful negative ideo-electric; and, from not being liable to fracture, it advantageously replaces the glass plates of electrical machines. From this fact, and from its possessing no hygroscopic attraction for moisture, it may be inferred that it is also an excellent insulator of electricity. Apart from its application in the form of insulators for overground wires, it is found most useful

¹ Abridged from a Paper read before the British Association.

We concur in Mr. Galloway's opinion of the plan adopted by Fresenius. There can be no doubt about its being tedious and difficult. The objection to tabular statements of analytical plans entertained by the great German analyst, seems almost morbid. Whether they appear as tables or not, analytical operations can only be represented in tabular statements. Those given by Fresenius are merely spread over some two or three pages, somewhat vaguely, instead of being concentrated in a systematic and intelligible form on one page only. Fresenius' plan is less costly to print, but, at the same time, more difficult to understand. One can scarcely work through a few pages of his "System of Instruction in Qualitative Analysis" without meeting with such delightfully complicated directions as this,—"Test for cobalt and nickel, as directed, § 192, 1, b, β , $\beta\beta$, (149)."

Mr. Galloway characterises the ordinary system as difficult as well as defective; he says, "It is so difficult, that those who employ it generally teach one thing, useless in itself, in order that they may teach another; they teach the student first to look for one acid and one base—a limitation which is not adopted in actual practice—solely for the purpose of rendering the course in which they have [?] he has] to look for all the bases and acids less difficult." We do not think it quite useless for the student to be able to detect one acid and one base; the principle of progressive exercises is one universally adopted, even by Mr. Galloway himself, who recommends the student to examine each group by itself—why not, then, each individual member?

Our author proceeds thus:—"In the plan I have adopted, the properties of the different members of each group are contrasted by placing them in parallel columns; the advantage of this is, that the student, after he has performed the experiments, is able himself to devise methods for the detection and separation of the different members; it places him, in fact, in the position of a judge who has to decide some cause after having heard all the pleadings." The plan here referred to consists merely of a further development of the plan of tabular statements. Mr. Galloway speaks of the reason and judgment exercised by the student using these tables. We think that the behaviour of the various bases and acids ought to be ascertained in a less narrow way, as pointed out above, and that the tables of properties are not unlikely to assist in making the student an "analytical machine." A careful selection of reactions ought to be presented to the beginner under each base and acid, while the analytical schemes subsequently given, ought not to do much more than suggest how the general and special tests may be successfully applied in the detection of the substances met with in analysis. The tables showing the reactions of the various bases and acids are very full, and present, without doubt, much information in a small compass, but they are not peculiar to the present Manual, and have been largely employed in other works.

Those features of his Manual to which Mr. Galloway specially directs attention, do certainly enable a student to acquire the power of performing analyses more rapidly and more easily than the ordinary system does: whether this power rests upon as secure a foundation of knowledge, is a matter of doubt.

The present edition is brought down in every important particular to the state of analytical knowledge at the present time. The photo-chemical method of Cartmell and the spectrum experiments of Bunsen will be found described very clearly and fully. Many of the special tests recently introduced into the laboratory, such as the excellent method of separating cobalt and nickel by means of nitrite of potash, will be here found in their proper places. We do not find, however, any reference to the use of biniodide of lead as an oxidizing reagent, although it is found extremely useful, in conjunction with nitric acid, for detecting traces of manganese carried down in the

ammonia precipitate (Group iii.), and for ascertaining the presence of chromium in the same precipitate. In the latter case a small portion of the precipitate is boiled with biniodide of lead in the presence of a solution of potash, chromate of potash being thus formed. We have not been able in actual practice to carry out quite successfully Mr. Galloway's table for the analysis of Group iii.; we do not think that much reliance should be placed upon the solubility, when iron is present, of sesquioxide of chromium in cold soda-solution taken in connection with its subsequent precipitation on boiling.

With a few trifling exceptions, such as those we have noticed, the book is a thoroughly good Manual of Analysis, and can be mastered in a short time. It is highly appreciated in several large schools, and is found of very great use by Medical students.

Report of the Committee on the Decay of the Stone of the New Palace at Westminster.

THIRD NOTION.

IMMEDIATELY following the examination of Mr. Szerelmey, the Committee were engaged in taking the evidence of Mr. Frederick Ransome, of Ipswich. It will be remembered that these gentlemen have urged rival claims in regard to the application of their respective processes to the Palace at Westminster; they were the first to undertake experiments in connection with the preservation of the stone actually forming part of the building; the results arrived at by them were capable of closer scrutiny and of more minute inquiry, and assumed, therefore, on this account a greater importance as compared with other and later suggestions which had not yet to the same extent been submitted to experimental trial.

During the month of August, 1859, the processes of Mr. Szerelmey and Mr. F. Ransome appear to have been first brought into direct competitive examination. At that period Sir Charles Barry sanctioned the treatment of the two bays on the river front, which were to be prepared each as an illustration of the capabilities of the process, with the view afterwards of applying on a larger scale that which should prove more successful in the preliminary trial. These specimens of protective workmanship have been reported on at the instance of the Government conjointly by Sir Roderick Murchison and Professor Faraday; been again inspected by the present Committee, and so frequently have formed a topic of discussion in the public journals, that it is needless now to remark more than the simple circumstance that the peculiar whitish appearance of Mr. Ransome's bay has been objected to, and that slight indications of efflorescence have made themselves apparent.

A still earlier application to the Houses of Parliament of the then recently patented process of Mr. Ransome dates as far back as the autumn of 1856, but the examples of that early period are not accounted by the inventor as successfully representing the capabilities of his mode of treatment from their having been the very first outdoor experiments of the kind, and executed without a full knowledge of all the requirements. The earliest, and at the same time satisfactory, example of the process is afforded by the Baptist Chapel, Bloomsbury, which was coated in the year 1858. The whole front of the chapel is of Caen stone, which, prior to the application of the process, is said to have been in a state of very rapid decay. Mr. Ransome refers particularly to the treatment of the Bath stone façade of the Royal Mews at Buckingham Palace as being the most successful example in London, but this having been executed so recently as 1860, cannot, we think, yet be considered as exhibiting a satisfactory indication of the value of his process. The Custom-house at Greenock, built of sandstone, is stated to be in every way a fair illustration. This was coated in 1859.

In regard to the nature of the process employed by Mr.

Ransome, we are in possession of the full particulars made known in the patent specification, and every information has most promptly been furnished by the inventor as to the materials employed and the mode of manipulation. The Committee appear also to have been assisted in their inquiries with reference to the solution of a problem of considerable experimental difficulty, viz., the determination of the depth to which the chemical preparation actually penetrates the substance of the stone. This point has not yet been properly ascertained, and we propose later to take an opportunity of resuming the discussion on this question, which will be more appropriately introduced after the nature of the materials themselves has been announced.

The object of Mr. Ransome's process is to deposit by precipitation within the pores of the stone an insoluble silicate of lime; this compound consisting wholly of imperishable mineral constituents, is intended to afford protection to the stone by rendering it less absorbent for water, and by preserving it likewise from the influence of the atmosphere. Such insoluble compound is formed by double decomposition between silicate of soda and chloride of calcium, applied as solutions and in this order to the surface of the stone. It will be seen, therefore, that besides the insoluble silicate of lime, an equivalent proportion of chloride of sodium must always be produced, and it appears probable that to this second product must partially be ascribed the tendency already referred to of causing an efflorescence on the outside faces of the stone. It was found to be possible to construct a block or kind of artificial stone from powdered materials, moistened alternately with these two solutions, and the property possessed by such blocks of resisting for a time the action of water, was conclusively demonstrated by the inventor. So great indeed is the degree of protective action communicated by the presence of the calcareous silicate, that limestone which has been thoroughly coated with this substance, is said to remain unacted upon when immersed in weak acids. In proceeding to the manufacture of "artificial stone," modes of operating have been resorted to for the formation of a compact mass, which are no longer similarly applicable to the walls of the Houses of Parliament; thus Mr. Ransome describes the interesting results obtained by exposing to heat the mixture of materials above referred to, whereby varieties of even greater insolubility were produced.

The objection which does not appear to have been conclusively answered in regard to this process, has reference to the impracticability of producing by double decomposition, especially when one of the secondary products is soluble in water, a condition of surface which is totally unaffected by the rain or moisture of the atmosphere; thus it would appear difficult so thoroughly to close the pores of the stone, as to prevent the extrusion of the chloride of sodium by water coming in contact with the face of the block. If removed, cavities, gradually widening, must be formed in consequence, which would permit of a subsequent and progressively increasing action upon the stone, due to the admission of the atmosphere. Moreover, the silicate of lime itself is a precipitate, somewhat gelatinous in nature, and occupies more bulk when moist, or at the time of its formation, than when it eventually becomes dry and sinks in the pores of the stone.

NOTICES OF PATENTS.

391. *Manufacture of Paper from a Pulp obtained from Wood.* E. H. BARRÉ and C. M. J. BLONDÉL. Nantes, France. Dated February 16, 1861.

This invention relates to an improved method of effecting the disintegration of wood for the purpose of obtaining therefrom ligneous fibres suitable for the manufacture of

coarse paper. The patentees employ as the agent for facilitating the separation of the fibres nitric acid; but inasmuch as the strong acid would be liable to act too energetically upon the substance of the wood, it is preferred first to saturate the shavings or sawdust with water, and then to proceed to the treatment with nitric acid. In this manner the structure of the wood is softened, so that little mechanical aid is required to reduce it to the condition of supple ligneous fibre, and after a thorough washing, the material is ready for bleaching and general treatment like any ordinary paper pulp. But, with a view of economising the nitric acid, and at the same time obviating the inconvenience arising from the corrosive action of the acid in the wood upon the metal of the crushing rollers, it is preferred to convert the free nitric acid into nitrate of soda by adding a sufficient quantity of carbonate of soda immediately after the acid treatment. By evaporating the washing liquors a crude nitrate may be recovered, which, although containing organic matter and other impurities, will serve admirably for the preparation of new supplies of nitric acid by distillation with oil of vitriol. As the result of the oxidation by nitric acid of the resinous matters in the wood, a certain quantity of carbazotic acid is generated, which, on the addition of alkali, produces a dark brown colour; the pulp loses much of this simply by squeezing in a press, and afterwards can be thoroughly bleached by washing and the treatment with chlorine. As a means of expediting the course of these operations, the inventors prefer to employ heat in some of the processes, but several hours' crushing are requisite for the production of the finer kinds of pulp.

The addition of carbonate of soda to the woody matters while the acid is yet retained in their structural cavities, would serve an important end, which is not alluded to in the specification, although several matters of inferior moment are deemed by the patentees worthy of the full scientific explanation therein given. The principle of subjecting a vegetable structure to the alternate action of free acid and an alkaline carbonate was one originally proposed, and very successfully carried out, in connection with the manufacture of linen from flax. It was found that the carbonic acid gas liberated as the result of this action was capable of exerting a considerable amount of disruptive force, which was useful in tearing open the structure, and in facilitating the subsequent mechanical treatment for the separation of the fibres. The mode of proceeding now described differs only in the detail of substituting an expensive acid for that formerly employed.

408. *Preparation of Alkaline and Earthy Cyanides.* W. CLARK, Chancery Lane, London. A communication. Dated February 19, 1861.

This patent refers to the production from ammonia of alkaline and earthy cyanides and ferrocyanides. The process of conversion is effected by passing a current of ammoniacal gas over a mixture of a carbonate of one of these bases with charcoal, pitch, tar, or other reducing agent, heated to dull redness in any suitable furnace. The employment of crude soda and potash, which contain a certain quantity of caustic alkali in addition to the carbonate, is specified as being particularly well adapted for producing this chemical reaction. A second claim relates to the preparation of cyanides or ferrocyanides by the reaction of a soluble sulphate upon cyanide of barium formed as above, whereby a precipitate of sulphate of baryta is produced. And, thirdly, the partial reconversion of this precipitated sulphate of baryta into carbonate by reduction, in the first place, with charcoal at a high temperature so as to form crude sulphide of barium, and passing carbonic acid gas through an aqueous solution of this product, when a certain quantity of carbonate of baryta will be regenerated and may be used in a repetition of the process.

The chain of chemical reactions called into play in the

porties it appears to resemble Portland cement. A quantity of the finely-ground material is to be stirred up in water and quickly applied as a thin wash to the face of the decaying stone. After a short interval the water will have been absorbed, entering into chemical union with the constituents of the cement, and thus, by filling the cavities and setting hard in the pores of the stone, is said to offer a considerable degree of protection, chiefly by keeping out rain and moisture. According to the circumstances, a second, or even third treatment, at intervals, will be required to secure the best results. This method of protecting stone has been applied by Mr. Griffiths with great success to the walls of houses in the neighbourhood of Whitby, which, from having been constructed of York-shire freestone, an open porous sandstone, allowed the water freely to penetrate in rainy seasons. By a single application of the cement, the stone has been enabled to resist for ten years and upwards the stormy weather which occasionally visits the eastern coast.

In questioning the applicability of a coating of Atkinson's cement to the Houses of Parliament, the Committee have raised doubts, firstly, in regard to its power of sufficiently penetrating the substance of the stone—whether the solid particles of cement would not be separated as by a filter, and form merely a superficial protective crust? and secondly, as to the appearance and texture of the stone after treatment—whether it would not look like stucco? For these reasons it does not appear to be equally well adapted to surfaces of decomposing magnesian limestone and to the coarse-grained, porous sandstones of Yorkshire.

NOTICES OF PATENTS.

437. *The Application of Certain Vegetable Substances to the Manufacture of Paper Pulp.* J. H. JOHNSON, Lincoln's Inn Fields, London. A communication. Dated February 21, 1861.

THE vegetable productions which it is proposed to make use of in the preparation of pulp for paper making are the following—viz., the fir tree, pine, larch, and, in fact, all conifers, junipers, willows, and osiers, tamarinds, broom, and coparia; also the genus juncus, or rush and genista. Amongst herbaceous plants those of the mallow tribe and the bursaria, particularly the bursa pastoris and its varieties; the panicum Germanicum and the tall panicum lupinus, the sorghum, or millet; also those plants of the genus lepidium, or pepper wort, comprised under the order of cruciferae; also the dead leaves of the haricot and pea, and of the thistle. The general treatment to which the above-mentioned vegetable productions are subjected consists, firstly, in boiling them in an alkaline liquor and in draining off this latter as soon as the structures are tolerably softened, then bruising the same in any convenient machine for the purpose of separating the fibres, and afterwards carding, alternately subjecting the loosened fibres to the action of acid and alkaline baths to remove soluble matter and assist disintegration, washing and bleaching by the chloride of lime or any well known process, and finally pressing the pulp into cakes or compact masses easy of transport.

The general mode of treatment here described has during the last few months repeatedly formed the subject of patent claims. It is interesting to remark the identity of language employed in defining the various stages in the unalterable succession of operations which constitute the process for obtaining paper maker's pulp from wood and softer vegetable structures. We have recently had occasion to describe the following patents bearing on this subject:—Indian corn and tanner's refuse (the spent bark of oak or fir) have each been made to furnish ligneous fibre for the purpose of paper making. Specification No. 112,

C. Stevens,¹ details the process of converting wood shavings, and even that of fir, into pulp by boiling with alkali, mechanical tearing, bleaching, &c. And the patent No. 391,² dated but five days earlier than that now before us, refers particularly to the method of treating alternately with acid and alkali as means of expediting the disintegration of the fibre in wood.

454. *Coating and Protecting the Silvered Surface of Looking-glasses or Mirrors.* J. E. COOK, Greenock. Dated February 22, 1861.

FOR the purpose of protecting the mercurial film of looking-glasses from the action of the atmosphere, it is proposed to coat the outer surface of the tin foil with copal or carriage varnish, laid on with a brush in the ordinary manner; the varnish may be used alone, or mixed with any suitable colour. When dry, a solution of shellac is applied as a second coating.

By the employment of alcohol as the solvent in the second varnish, there will be no fear of disturbing the layer of copal previously applied. Such a double coating would probably diminish the tendency to oxidation, but the mirror would, on the other hand, become more liable to mechanical injury from sudden changes of temperature inducing unequal rates of expansion and contraction in the three films, all in absolute contact with each other and with the back of the glass.

456. *Preparation of Red Dyes.* J. MARTIN, Church Row, Limehouse. Dated February 23, 1861. (Not proceeded with.)

THE dyes which form the subject of this invention, are prepared from aniline by the action of nitrate of lead and monohydrated acetic acid. These are mixed together, heated, and subsequently diluted and filtered. For the extraction of the lead compounds from the product, the inventor employs oxalic or other organic acid, with the addition of common salt, which is said to heighten the brilliancy of the colour. Although the nitrate of lead is particularly specified, the inventor claims also the production of red dyes from aniline by the employment of other nitrates yielding oxygen on the application of heat.

A patent has some time previously been granted for the use of the nitrate of mercury as a means of effecting the conversion of aniline into red dye; this circumstance would invalidate the claim stated in the concluding part of the specification, which appears to have been drawn up in too general terms.

455. *Manufacture of Cast Steel.* ROBERT MUSHET, Coleford. Dated February 22, 1861.

473. *Manufacture of Cast Steel.* ROBERT MUSHET, Coleford. Dated February 23, 1861.

THESE inventions have reference to the production of superior qualities of cast steel by the employment of titaniferous iron ore, or other compounds of titanium, in conjunction with the materials ordinarily used in the preparation of cast steel. This admixture may be effected in two ways. Either the titaniferous ore above mentioned, or any native variety of titanic acid, may itself be introduced into the melting pot with the mixture of cast iron, granulated or refined pig iron, and pulverized iron ores, or any other form of oxides of iron and manganese with appropriate reducing agents. Or, secondly, the titanium ore may previously have been reduced by a suitable furnace treatment with deoxidizing agents, to the condition of an alloy of titanium and iron, and in this form added to the melted materials intended to be used in the production of cast steel.

¹ CHEMICAL NEWS, vol. iv., p. 134.

² CHEMICAL NEWS, vol. iv., p. 213.

With a similar object the latter of these patents refers to the employment of the special description of white cast iron termed "spiegel eisen," or of any analogous compound of iron, manganese, and carbon, and such material being alloyed by melting either with the titanium cast steel already described, or with any mixture of substances intended to be used in its production.

These specifications are in continuation of a number of similar patent claims already secured by Mr. Mushet, and which have on former occasions been noticed in our columns. They all refer particularly to the economical employment of the titaniferous iron sand, procurable in great abundance in New Zealand, which is now shown to be so advantageously used in the production of improved qualities of cast iron and steel.

Grants of Provisional Protection for Six Months.

1733. Thomas Cobley, Meerholz, Hesse, Germany, "An improved process for the manufacture of fluo-silicates of lead and baryta, and for the application of the same to various purposes in the arts and manufactures."—Petition recorded July 8, 1861.

1734. Thomas Cobley, Meerholz, Hesse, Germany, "An improved process for the treatment of silicates of metallic and non-metallic bases and other siliceous compounds, as slags, scorings, and such like, in order to recover the said bases, and also in the production of a fluo-silicic and silicic acids."—Petition recorded July 9, 1861.

1775. James Cane Coombe and James Wright, Bridge Street, Blackfriars, London, "Improvements in the manufacture of glass, pottery, porcelain, and other ceramic and plastic wares."—Petition recorded July 15, 1861.

2085. Andrew Stein, Edinburgh, Mid Lothian, N.B., "Improvements in distillation."—Petition recorded August 21, 1861.

2286. John Adams Knight, Symond's Inn, Chancery Lane, London, "An improved apparatus for rendering fatty or oleaginous matter, and delivering the same."—A communication from Wallington Lee, Esq., East 19th Street, New York, U.S.

2288. Richard Waller, Baker Street, Portman Square, London, "Improvements in machinery and apparatus for manufacturing and refining cane juice and other saccharine substances."

2326. Edward Alfred Cowper, Great George Street, Westminster, "Improvements in apparatus for freeing gases from dust and other particles of matter floating therein, and for causing vapours or gases to be absorbed by liquids."

2347. Rene Prudent Patrice Dagron, Paris, "An improved microscope to be used for exhibiting photographic views and productions."—Petitions recorded September 19, 1861.

2383. Charles Watt, Graham Street, Walworth, John Watt, Lorrimer Street, Walworth, Surrey, and Thomas Snaith Haviside, Cornhill, London, "An improved mode or method of bleaching palm oil."

2397. John Vaughan, Middlesborough, Yorkshire, "Improvements in treating gas produced by blast-furnaces in its passage from the blast-furnaces to other furnaces, stoves, boilers, or other heating apparatus where gas may be employed."—Petitions recorded September 25, 1861.

Notices to Proceed.

1291. Marc Antoine Francois Mennors, Rue de l'Echiquier, Paris, "Improvements in the coupling or connecting joints of pipes for the conveyance of liquid, fluid, or solid bodies."—A communication from J. B. Lavril, Paris.—Petition recorded May 21, 1861.

2032. John Cowderoy Martin, High Street, Barnes, Surrey, "An improvement in treating bones, and the manufacture of the products thereof."—Petition recorded August 15, 1861.

1322. Edward Henry Cradock Monckton, Parthenon

Club, Regent Street, London, "Improvements in obtaining and applying magnetic motive power, which invention is also applicable to other useful purposes."—Petition recorded May 25, 1861.

1338. Robert Michael Letchford, Old Montague Street, London, "An improvement in the manufacture of matches."

1353. Alfred Blake, Russell Place, Fitzroy Square, London, "Improvements in the process of brewing."—Petitions recorded May 30, 1861.

1364. Ebenezer Hartnall, Ryde, Isle of Wight, "An improvement in the manufacture of paper."

1411. Edward Charles Stamford, Worthing, Sussex, "Improvements in obtaining products from seaweeds."—Petitions recorded June 4, 1861.

1789. Richard Jones, Camden Town, London, "Improvements in safety lamps."—Petition recorded July 16, 1861.

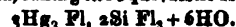
1983. James Hemingway, Robert Town, Yorkshire, "Improvements in machinery or apparatus for working coal, ironstone, or other minerals."

2268. Marc Antoine Francois Mennors, Rue de l'Echiquier, Paris, "An improved combination of chemical and mechanical processes for the conversion of fibrous vegetable matters into paper pulp."—A communication from Louis Achille Carteron, Dieuze, Meurthe.—Petitions recorded September 12, 1861.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Salts of Fluorine, Silicon, and Mercury.—Pinkener describes (*Poggendorff's Annalen*, Bd. cxi. 248) three compounds of the above bodies. The *silico-mercuric fluoride* is obtained in clear prismatic crystals when a solution of carbonate of the protoxide of mercury in hydrofluosilicic acid is evaporated. Washed with a little water, then pressed between folds of bibulous paper, and afterwards dried over sulphuric acid, these crystals on analysis gave results from which the author calculated the following formula, taking the equivalent of silicon as 21:—



Berzelius' *silico-mercuric fluoride* Pinkener has found to be an hydrated *oxyfluoride*. It is obtained in small acicular crystals on evaporating a solution of mercuric oxide in hydrofluosilicic acid. The composition he found to be



This salt is decomposed by water at the common temperature, forming an acid salt, which remains in solution, and depositing a yellow powder rich in oxide of mercury. The same solution concentrated until the needles begin to form, and then kept at a temperature never exceeding 15° C., deposit clear rhombohedral crystals, united like steps. These deliquesce in the air, and melt in their water of crystallization. Over sulphuric acid they effloresce. They are formed of the *silico-mercuric fluoride*, $\text{Hg Fl}, \text{Si Fl}_2$, and appear to contain six atoms of water.

Double Salts Formed by Iodide of Bismuth.—Lindau has obtained (*Poggendorff's Annalen*, Bd. cxi. s. 240) some double salts with iodide of bismuth, greatly resembling the double salts with iodide of antimony, described by Schaefer (*CHEMICAL NEWS*, vol. ii. p. 143). They are obtained by saturating a concentrated solution of the positive metals with iodide of bismuth, and evaporating the solutions.

Iodide of Bismuth and Sodium forms small red crystals, containing $3\text{Na I}, 2\text{Bi I}_3 + 24\text{HO}$, and apparently is amorphous, with the salt $3\text{Na I}, 2\text{Sb I}_3 + 24\text{HO}$.

Iodide of Bismuth and Ammonium forms beautiful rectangular prisms of a brownish red colour. The powder is vermilion red.

Iodide of Bismuth and Barium is obtained in small rhomboidal crystals of a bright red colour and brilliant lustre. The composition is $2 \text{BaI} \cdot \text{BiI}_3 + 18 \text{HO}$.

Iodide of Bismuth and Calcium has a similar composition to the preceding salt. It forms rhomboidal prisms of a deep red colour and glassy lustre. The powder is carmine red, but nearly black when the water of crystallisation has been driven off.

Iodide of Bismuth and Magnesium ($\text{MgI} \cdot \text{BiI}_3 + 12 \text{HO}$) forms oblique rectangular prisms of a pomegranate red colour. The water of crystallisation cannot be got rid of under a temperature of 175°C .

Iodide of Bismuth and Zinc ($\text{ZnI} \cdot \text{BiI}_3 + 12 \text{HO}$) resembles the preceding, but loses its water at 100°C .

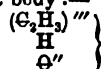
The author could not obtain salts with the iodide of bismuth and iodides of cadmium, iron, and silver. He succeeded in obtaining crystallised compounds with iodide of bismuth and bromides of potassium, sodium, ammonium, magnesia, and zinc, but has not yet examined these salts.

II. ORGANIC CHEMISTRY.

Modifications of Aldehyde.—Lieben (*Sitzungsber. der K. Akad. der Wissenschaften zu Wien*, Bd. li. s. 549) heated iodide of ethyl and aldehyde in a sealed tube to 100°C . The former remained unaltered, but the latter changed to an isomeric modification resembling paraldehyde. It boiled at 123° to 124° , and possessed a vapour density three times that of aldehyde.

Aldehyde, when well cooled, he found to absorb a considerable quantity of cyanogen. The solution, sealed up and left for some time at the ordinary temperature, formed paraldehyde, the cyanogen undergoing no change. When aldehyde is heated in a sealed tube with a strong aqueous solution of either formate of potash, acetate of soda, or the potassio-tartrate of soda, two layers are formed: the lower is the unaltered solution of the salt used; the upper is a slightly viscid yellow liquid, having a penetrating odour. This liquid is very unstable, but, after rectifying with great care, the author determined that it was mainly constituted of a new body, having the composition expressed by the formula $\text{C}_2\text{H}_5\text{O}$. Exposed to the air, it thickens; it reduces oxide of silver, is resinified by potash, and blackens with sulphuric acid. Nitric acid acts on it violently, causing the evolution of much gas, and forming some resinous matter. Heated alone in sealed tube to above 200°C . it changes to a nearly solid black mass and a little water. It is formed, the author thinks, by the elimination of water under the influence of the salt: $2 (\text{C}_2\text{H}_5\text{O} = \text{C}_2\text{H}_4\text{O} + \text{H}_2\text{O})$. Aldehyde heated alone undergoes no change.

Lieben proposes the following rational formula for aldehyde which, he says, lends itself to the interpretation of all the reactions of that body:—



Paraldehyde and elaldehyde he believes (with Geuther and Cartmell) to be identical.

Diamylphosphoric Acid.—Dr. Kraut (*Archiv. der Pharm.* Bd. cxvi. s. 133) obtains this acid by acting on a solution of bromine in fusel oil with phosphorus until the colour disappears. The product is then washed with water to remove the hydrobromic acid, and the oily residue of bromamyl and diamylphosphoric acid is shaken up with a soda solution. The mixture is then boiled to get rid of the bromamyl, and the solution of diamylphosphate of soda is supersaturated with dilute sulphuric acid, whereupon the mixture becomes milky, and oily drops separate. These are taken up by ether; the ethereal solution is washed, evaporated, and dried over sulphuric acid. The acid is not obtained perfectly pure in this way, some substance remaining mixed with it, the exact nature of which is unknown to the author.

Dr. Kraut examined several salts of the acid. **Diamyl-**

phosphate of Lime $2 \text{C}_{10} \text{H}_{11} \text{O} + \text{CaO PO}_3$ was obtained by adding water and milk of lime to the above ethereal solution of the acid. The excess of lime was removed by diluting with a good deal of cold water, passing carbonic acid through the mixture, boiling and filtering. The filtrate was evaporated over a water-bath. The salt forms long snow-white silky needles, which on being dried shrink up to a papery mass; this remains unchanged when heated to 180°C ., and when rubbed becomes electrical. When dropped into water it rotates with great rapidity. It dissolves as easily in cold as in hot water, and is so soluble that the cold saturated solution solidifies when heated.

According to Fehling, the diamylphosphates of the alkalies precipitate the salts of manganese, lead, copper, silver, and mercurous oxide. The cold saturated solution of the lime salt separates only some flocculi from the manganese, copper and silver salts, but gives a large precipitate with the lead and mercury salts. The baryta salt is more soluble in cold than hot water. On heating a cold saturated solution of the magnesia salt, it separates in asbestos-like needles, which settle into a fat-like mass the instant they are removed from the mother liquor.

Diamylphosphoric acid forms an acid and a neutral silver salt. When shaken up with moist silver oxide, separated from the excess, and evaporated, microscopic needles of the acid salt are obtained. These dried over oil of vitriol remain unchanged at 120° , and have the composition $2 \text{C}_{10} \text{H}_{11} \text{O} \cdot \text{AgO PO}_3 + 2 \text{C}_{10} \text{H}_{11} \text{O} \cdot \text{HO PO}_3$. The neutral salt is obtained by decomposing a solution of sulphate of silver in hot water with an equivalent weight of the baryta salt. It has the formula $2 \text{C}_{10} \text{H}_{11} \text{O} \cdot \text{AgO PO}_3$.

When submitted to dry distillation, the salts of diamylphosphoric acid yield a clear liquid, and leave a residue of a metaphosphoric acid salt scarcely blackened by carbon.

MISCELLANEOUS.

Adulteration of Food.—The Cheltenham Board of Guardians have appointed Mr. Horsley, the County Analyst, at a salary of ten guineas per annum, to analyse the provisions supplied for consumption in the Union Workhouse.

ANSWERS TO CORRESPONDENTS.

* * All Editorial Communications are to be addressed to the EDITOR; and Advertisements and Business Communications to the PUBLISHER at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

T. Falconer.—The solvents vary according to the mode of preparation. In some cases alcohol, in others dilute acetic acid, and in some few water will be found the best solvent.

D. Knight.—The best practical information with which we are acquainted on the subject of grinding lenses is in a pamphlet published by Secretan, of Paris, entitled, "De la Distance Focale des Systemes Optiques convergents." It costs about 2s. 6d. to 3s.

Red Ink to be used with Steel Pens.—J. M.—We do not know a good receipt. Those which we have tried have not answered our expectations. The suggestion shall be attended to if possible.

W. B.—The crust formed on the leaden top of your inkstand is probably carbonate of lead, formed by the acid and aqueous vapours from the ink. As a remedy use India-rubber varnish, or deposit silver on it electro-chemically.

J. Horsley.—It is withdrawn as you request. Our examination of the sample sent coincides with yours. We shall be glad to receive the full statement promised.

W. Stevenson.—Received.

A. H. B.—1. Allow it to digest in a mixture of alum, salt, and nitre, dissolved in water. 2. See our previous volumes. 3. The zinc plates need not be quite so large as the silver, but more than half the size.

A Subscriber.—Boil them with dilute hydrochloric acid and copper. Test the copper subsequently in the usual way. We do not think arsenic is present; it is scarcely ever used now for that purpose.

THE CHEMICAL NEWS.

VOL. IV. No. 100.—November 2, 1861.

THE PRESERVATION OF STONE.

IN another column will be found an abstract of the plan, which was submitted on 17th May last, by the Editor of this Journal, to the Committee on the Decay of the Stone at the New Palace at Westminster, with the view of hardening and preserving it from further decay. Since that period we have unremittingly pursued our experiments on the subject, both with the plan above referred to, and various others, which were suggested by reactions incidentally noticed in the course of the investigation, and it is to one of these offshoots of our experiments that we wish now to draw attention.

No one who has critically examined the various projects for hardening and protecting stone, which have during the last few years been made public, can have failed to remark the great importance, which, in the majority of instances, is attached to the action of silica. Thus, in reviewing the processes submitted to the Decay of Stone Committee, among the eleven proposals which have already been under consideration, no less than seven depend for their efficacy upon the action of this very substance under various conditions of employment. Advantage is taken of the mutual decomposition exerted between certain silicious compounds applied, and the materials constituting the building stone itself; or, otherwise, systems are founded upon the production of compounds from an alkaline silicate, and a soluble earthy salt successively applied. The known properties of silica and of the class of silicates have, no doubt, powerfully contributed to the formation and establishment of this opinion. The facility and cheapness with which they can be manufactured on the large scale; their inalterability under trying atmospheric influences; their all but complete indifference to energetic chemical reagents—all point to silica as the one fit material upon which the ingenuity and experimental resources of our chemists and practical men may be expended with the greatest promise of success. Let only a sufficient amount of silica be compelled to enter and occupy the pores of the stone and incrust all the exposed surfaces; let it be employed in a state of hydration or other suitable form in which it may gradually combine with the earthy constituents it there meets, and with its action unfettered by saline impurities the presence of which tend often to interfere mechanically with its successful employment, and the strong presumption is that the Houses of Parliament will endure, so far as the stone is concerned, as long as the Pyramids.

The difficulty which besets many of the processes of silicification is, that along with the needful silica, so much superfluous, and, indeed, injurious matter is introduced that the valuable qualities of the silica are in a great measure counteracted; the disintegration of the stone sometimes caused actually by the efflorescence of these extraneous substances, and the porous character necessarily induced as the consequence of the gradual removal of the soluble salts in juxtaposition with the silica, almost undoing the binding and hardening action

of this valuable material. From this train of reasoning, it became evident that if hydrated silica could but be deposited in the pores of a limestone or dolomite, without the assistance of potash or soda as a vehicle, and there be left slowly to enter into chemical union with the earthy bases of the stone, its action commencing unimpaired by the presence of extraneous salts, and its future efficacy undiminished by the progress of their removal,—that if this could be accomplished, there would be a far greater chance of ultimate success than has been offered by any plan yet made public.

It is well known that silica can, by appropriate means, be obtained in the form of a pure aqueous solution, and it was to this liquid that we accordingly directed our attention. This solution can be made in several ways:—

1. By dissolving sulphide of silicium in water, when sulphuretted hydrogen is given off, and the silica remains completely dissolved, and in such quantity that the liquid gelatinises when an attempt is made to evaporate it.

2. By precipitating silica in the gelatinous state from an alkaline silicate, by means of acetic or other weak acid, and, after well washing, heating it for some time under pressure, with a small quantity of water in a closed vessel. A liquid is thus obtained which gelatinises on addition of a saline solution.

3. By passing gaseous fluoride of silicium over crystallised boracic acid, and separating the hydrofluoric and boracic acids by digestion with a large excess of ammonia, a hydrate of silica remains, which, when well washed from the above acids, is very soluble in water. This solution gives no precipitate when boiled, but leaves silica as an insoluble powder on evaporation.

4. By the beautiful method recently pointed out by Professor Graham, in which advantage is taken of the new means of separating bodies by *dialysis*. A solution of silicate of soda, supersaturated with hydrochloric acid, is placed on one side of a parchment paper septum, pure water being on the other side; in a few days the hydrochloric acid and chloride of sodium will be found to have completely passed through the diaphragm, leaving the silica in aqueous solution, and so pure that acid nitrate of silver fails to detect chlorine in the liquid. This solution remains fluid for some days, but it ultimately gelatinises. We have generally adopted this last plan of preparing the aqueous solution of silica, although a stronger solution is obtained by the method first given.

When a pure aqueous solution of silicic acid prepared as above is allowed to soak into the pores of chalk or dolomite, a process of hardening rapidly occurs, which goes on increasing for several days, whilst, owing to its considerable depth of penetration, and to there being no soluble or efflorescent compounds to be removed, there is every probability that this hard silicious impregnation will afford permanent protection to the stone. We are now actively engaged in investigating the nature of the action which takes place, and already several curious and important results have been made out, from which we are led to anticipate that our experiments will ultimately be rewarded with complete success.

SCIENTIFIC AND ANALYTICAL
CHEMISTRY.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 218.)

Synthesis of Chloride of Silver.—I have determined the proportional relation between silver and chlorine,—
1st. By the direct combustion of the metal in chlorine, as has already been performed by M. Dumas.

2ndly. By dissolving silver in nitric acid, precipitating the nitrate by means of gaseous hydrochloric acid conducted to near the surface of the solution; evaporating the whole to dryness in the same vessel, and fusing the resulting chloride in an atmosphere of hydrochloric acid gas, afterwards replaced by air.

3rdly. By dissolving silver in nitric acid, precipitating by aqueous hydrochloric acid, washing the precipitate in water acidulated with nitric acid, fusing the chloride in an atmosphere of hydrochloric acid, and evaporating all the liquids out of contact of air, to collect the chloride carried down or dissolved by them.

4thly. By dissolving silver in nitric acid, precipitating the nitrate by a solution of pure sal-ammoniac, washing the chloride in acidulated water, and fusing it in an atmosphere of hydrochloric acid; then evaporating out of contact of air all the washing waters (after having passed through them a current of chlorine to destroy the nitrate of ammonia formed and the excess of sal-ammoniac employed), to collect the chloride carried down or dissolved.

These methods, which, however, have already been employed, have all given the composition generally admitted for chloride of silver; they present causes of error which I shall point out when detailing the results given by each.

I. Synthesis of Chloride of Silver by the Action of Chlorine on Silver heated to Redness.—To effect the combustion of silver in chlorine, I have taken a tube of Bohemian glass, bent in such a manner that the silver in lumps or sheet was contained in a portion very slightly inclined, so that the chloride as it was formed should be carried by the chlorine into a horizontal portion, which served as a receiver. This horizontal portion was bent upwards again very slightly to avoid loss of chloride. The tube and the silver were weighed separately. All that part of the tube which was to be heated was surrounded with a thin sheet-iron case, filled with pure magnesia. This case was not heated to redness until the pure dry chlorine which traversed the tube had been passing sufficiently long to completely drive out all the oxygen.

The complete chloruration of 100 grammes requires a slow and uninterrupted current of chlorine for at least fifteen hours. It is likewise necessary to take the greatest precaution to prevent any trace of silver escaping the action of the chlorine. Thus, out of five experiments that I made, two were vitiated owing to particles of silver being surrounded with fused chloride, and thus remaining unattacked.

The synthesis of chloride of silver by this method is liable to three sources of error or uncertainty. The first is the mechanical carrying away of the chloride of silver by the current of chlorine. Having found this out in the first experiment, I tried to obviate the inconvenience by allowing no more chlorine to pass over the silver than

it would absorb. I cannot describe without a drawing the arrangement of apparatus which I devised for this purpose; I give it in my memoir.

The second inconvenience is the absorbent power which the fused chloride has for chlorine. To obviate this source of error, I caused to pass in one experiment a current of air, and in two others a current of dry carbonic acid over the fused chloride, but after having previously noted the weight of the chloride.

Finally, the third source of error is the attack of the glass tube by chloride of silver, which is kept long fused in contact with it. There is thus produced an alkaline chloride, which volatilises in the current of chlorine. I am not quite certain whether this attack takes place with disengagement of oxygen or chlorine, but the augmentation of the weight of the tube is a constant fact.

To sum up, this method, which at first sight appeared so simple, leaves grave doubts as to its results, if these are not confirmed by those obtained by means of other methods.

Below is given the result of three experiments which I succeeded in bringing to a satisfactory conclusion:—

Synthesis of Chloride of Silver by the Combustion of Silver in Dry Chlorine.

Numerical Order.	Weight of the Silver in air.	Weight of the Silver reduced to a vacuum.	Weight in air of the Chloride of Silver fused in chlorine.	Weight in vacuum of the Chloride of Silver fused in chlorine.	Weight in air of the Chloride of Silver fused in a slow current of carbonic acid.	Weight in vacuum of the Chloride of Silver, after eliminating the dissolved chlorine.	100,000 parts of Silver produce	
							A.—Chloride saturated with chlorine.	B.—Chloride freed from dissolved chlorine.
I.	Grms. 91.4655	Grms. 91.462	Grms. 121.497	Grms. 121.5058	Grms. 121.4995	Grms. 121.4995	132.848	132.841
II.	69.870	69.86735	92.820	92.8273	92.8075	92.8145	132.861	132.843
III.	101.523	101.519	134.867	134.877	134.851	134.861	132.859	132.843

II. Synthesis of Chloride of Silver, by dissolving Silver in Nitric Acid, precipitating the Nitrate by means of Gaseous Hydrochloric Acid conducted to near the Surface of the Solution, evaporating the whole to Dryness in the same vessel, and fusing the resulting Chloride in an Atmosphere of Hydrochloric Acid Gas, afterwards replaced by a Current of Air.—Almost all my syntheses of chloride, and nitrate of silver and lead, have been made in the same apparatus. A drawing of this is given in my memoir. I will here describe it in a few words. The apparatus consists of a globe or matrass of Bohemian glass of a convenient size, according to the quantity of the metal to be dissolved, to the neck of which is adapted a flask of the same glass, the bottom of which is removed. To the narrow opening of this flask I adjust hermetically a system of bulb tubes, which serve to wash the gas traversing them, so as to retain the nitrate carried away by the nitrous vapours during the solution of the metal. The bulb-tube has a vertical branch leading into a small globe of water. The last trace of metal is thus arrested.

In order to attach the globe conveniently to the balance, I have arranged a system of platinum wires by which to hang it; these during the experiment do not come in contact with acids.

The globe, with the flask attached to it, being weighed, and the weight verified the next day,—a precaution which I have taken in all my syntheses,—I introduce into it the silver in small bars, and then pour in the nitric acid which has immediately before been distilled. Holding the neck of the globe almost horizontal, I adapt it to the flask with the tube, containing in its bulbs a convenient quantity of pure water, and fix the whole in this position. The globe is then carefully heated in an air bath. During the solution of the metal, I take care that the temperature never rises above 50° or 60°, otherwise the action is too energetic, and in spite of all precautions silver is carried through the condensing apparatus. Three or four hundred grammes of silver require at least thirty-six or forty-eight hours' action to dissolve in nitric acid of a strength of 40°, or even 25°, Baumé.

When the solution is complete, I remove the bulb-tube and the flask from the globe, taking care always to keep this in its original position—that is to say, with the neck almost horizontal. I then pass into it a slow current of hydrochloric acid, continuing the current until the liquid, set in motion by the rotation of the globe, becomes turbid. I then insert the neck of the globe as far as possible into the mouth of the receiver, and gently incline the whole of the apparatus, so that the vapour which condenses in the neck cannot run back. I heat the liquid in the globe by means of an air-bath to a temperature which approaches, but which must never reach, its point of ebullition. When almost all the liquid is volatilised, I again pass hydrochloric acid gas into the globe without allowing it to cool, and always keeping it in the inclined position. Dark red vapours are immediately formed, which soon give place to pure gaseous hydrochloric acid. I then evaporate it completely to dryness, and heat the chloride to its fusing point.

I have sometimes weighed the chloride dried at its fusing point, in order to ascertain the quantity of nitrate of silver which it retains when it is precipitated from a concentrated solution of this salt. This quantity is very variable, but its presence is a constant occurrence. In order to obviate the augmentation of weight which it causes, I filled the globe with hydrochloric acid, and kept the chloride in a state of fusion as long as orange-yellow vapours appeared; but taking the precaution not to raise the temperature beyond the fusing point, for fear of attacking the glass and decomposing the chloride itself. I then replaced, with the aid of heat, the atmosphere of hydrochloric acid by one of dry air.

From the moment of its formation until after weighing the chloride of silver was completely kept from the action of light; thus, in these experiments the chloride of silver remained of a pearly white. The globe has been but rarely attacked; at the most there were to be noticed here and there some yellowish-brown spots on the glass. When the operation is terminated, the globe is fitted with the flask, which serves as a stopper for it. In order to prevent moisture from penetrating during its cooling, I adapt a chloride of calcium tube to the flask. When it is quite cool I wash the globe with water, and, after having wiped it dry, I suspend it to the beam of the balance and allow it to remain at rest for several hours.

The acid liquid which volatilises has always been evaporated a second time in a porcelain dish to see if silver be present in the residue, which I have always found. When reduced to a twentieth of its volume, I have added to it the contents as well as the rinsings of the bulb-tube and the flask through which the gas

evolved during the solution of the silver was passed; the chloride obtained in this manner was weighed separately.

Two causes of error can affect the result, but they act in an opposite manner. One of them tends to increase the weight from impurity in the acids, or from the vessels being attacked. I have sufficiently explained in the introduction how I have removed this cause of error; I think I may safely say it does not exist. It is probable, on the contrary, that there is a slight loss of weight owing to the chloride of silver being carried away by the chloro-nitrous vapours which are produced during the fusion of this body in an atmosphere of hydrochloric acid to remove the nitrate of silver, whether this nitrate exists already in the precipitate formed in a concentrated solution of silver in nitric acid, or whether it has been formed by the direct action of the nitric acid on the chloride during the evaporation of the liquid acid.

Below I give the results of two experiments made by this method:—

Syntheses of Chloride of Silver.

Numerical Order.	Weight of the Silver in air.	Weight of the Silver reduced to a vacuum.	Weight of the Chloride of silver obtained from the silver carried off during the solution of the metal and the evaporation of the liquids.	Weight in air of the Chloride of Silver fused in hydrochloric acid gas.	Total weight of the Chloride of Silver reduced to a vacuum.	100,000 parts of Silver produce of Chloride of Silver
IV.	grammes 103.553	grammes. 108.549	grammes. 0.0345	grammes. 144.162	grammes. 144.207	132.849
V.	399.667	399.651	0.0940	530.787	530.920	132.846

III. Synthesis of Chloride of Silver by dissolving Silver in Nitric Acid, precipitating the Solution with a slight Excess of Hydrochloric Acid, washing the Precipitate, fusing the Chloride in Hydrochloric Acid Gas, and evaporating all the Liquids out of contact of air to recover the Chloride carried down or dissolved.—The solution of the silver was effected in an apparatus identical with the one previously described. The contents of the bulb-tube and the flask employed to condense the nitrate carried off by the gas, as well as the washing-waters of these two apparatus, were added to the contents of the globe. After complete cooling of the liquid, I carried the globe into a dark room, introduced a slight excess of hydrochloric acid, and shook the resulting chloride for at least twenty minutes to divide it, and to transform as completely as possible all the nitrate into chloride. At the end of that time the contents of the globe were heated to 100°. By the action of heat, the liquid became perfectly clear and the chloride had greatly contracted. The next day I decanted the liquid, with the greatest precautions. The globe, still remaining inclined, was then heated in a bath to 100°; there thus run from the chloride a fresh quantity of very clear liquid. I then poured into the globe, boiling water, very slightly acidulated with nitric acid; the chloride was then agitated to divide it afresh. The next day the liquid, which had become clear, was again decanted. The globe being placed in a very inclined position, was heated, and all the liquid produced by the contraction of the chloride under the influence of heat was decanted and added to the preceding waters. The chloride was thoroughly dried, then fused in an atmosphere of hydrochloric acid, which, as soon as fusion commenced, instantly eliminated chloro-nitrous vapours. The atmosphere of hydro-

chloric acid was then replaced by pure dry air. The chloride obtained was pearly white.

All the decanted solutions and the liquids obtained by drying the chloride were evaporated. In the experiment which I made in this manner on 99.9925 grammes of silver, the total volume of these liquids amounted to 2510 cubic centimetres. When evaporated they left a residue the total weight of which was 0.0063 grammes, containing 0.0035 grammes of chloride of silver, the rest being composed of traces of chloride of copper and of alkaline and earthy chlorides.

This experiment gave the following results:—

Synthesis of Chloride of Silver.

Numerical Order.	Weight of the Silver in air.	Weight of the Silver reduced to a vacuum.	Weight of the Chloride of Silver obtained from the evaporation of the washing waters.	Weight in the Chloride of Silver fused in hydrochloric acid gas.	Total weight of the Chloride of Silver reduced to a vacuum.	100,000 parts of Silver produce of Chloride of Silver
VI.	grmms. 99.9965	grmms. 99.9925	grammes. 0.0035	grammes. 132.825	grammes. 132.8382	132.848

IV. Synthesis of Chloride of Silver by dissolving the Metal in Nitric Acid, precipitating the Solution with Chloride of Ammonium, washing the Precipitate, and fusing the Chloride in Hydrochloric Acid Gas.—This determination was made under absolutely the same conditions as the preceding, with this exception, that the hydrochloric acid was replaced by sal-ammoniac, and that the washing-waters and that derived from the drying of the chloride, before being evaporated, were submitted to a current of chlorine to destroy the excess of sal-ammoniac and of the nitrate of ammonia which was formed. The evaporation of these left me an appreciable quantity of chloride.

A synthesis made in this way with silver prepared from the acetate gave me the following result:—

Numerical Order.	Weight of the Silver in air.	Weight of the Silver reduced to a vacuum.	Weight of the Chloride of Silver derived from the metal carried down and dissolved by the washing waters.	Total weight of the Chloride in air.	Weight of the Chloride reduced to a vacuum.	Chloride of Silver produced by 100,000 parts of this metal
VII.	grmms. 98.3175	grmms. 98.3140	grammes. 0.0360	grammes. 130.5925	grammes. 130.602	132.8417

Thus, my syntheses of chloride of silver have given the following results:—

1. By the combustion of silver in chlorine	100,000 parts of silver.	I. 132.841
2. By the precipitation of silver with hydrochloric acid without washing the chloride		II. 132.843
3. By the precipitation of silver with hydrochloric acid with washing the chloride		III. 132.843
4. By the precipitation of silver with sal ammoniac and washing the chloride		IV. 132.849
		V. 132.846
		VI. 132.848
		VII. 132.8417
		Mean.. 132.8445

Since the sixth determination, in which an increase of weight is impossible, has given from 100,000 parts silver 132.848 parts of chloride, I consider the mean of 132.8445 as being above the truth; and I think we ought to admit that, within the limits of experiment, 100,000

parts of silver produce 132.850 parts of chloride. Hence it results that the proportion which silver bears to chlorine is in the ratio of 100,000 : 32.850.

(To be continued.)

On the Adulteration of Butter, by JOHN HORSLEY, Analyst to the County of Gloucester.

A short time since the question was put by one of your correspondents as to whether there was any method of detecting lard in butter, which I have much pleasure in answering.

In order to distinguish between pure butter and that adulterated with lard and other substances, proceed thus:—

First satisfy yourself, by melting a portion of the suspected butter over a water-bath, and observing if there be any insoluble admixture of farinaceous matter, such as wheat-flour, potato-starch, arrow-root, or turmeric (said to be sometimes used), which the microscope and chemical tests will prove; then mix the melted butter in an evaporating-dish with four or five times its bulk of hot water, and allow it to stand for two or three hours to collect on the surface and solidify. Detach the resulting cake of butter, and place it on a piece of blotting-paper to dry, by the absorption of all adhering aqueous matter. If a piece of this prepared butter be introduced into a wide-mouth stoppered bottle, and surrounded with ether, at the temperature of 65° Fahr., it ought to entirely dissolve, forming a clear lemon-yellow coloured liquid.

On the other hand, the purest lard, which, on being melted, leaves no residue, is more or less insoluble in ether at that temperature, as a thick milky fluid results, which, on standing, deposits to a considerable extent. The same may be said of other fats, such as dripping, mutton suet, tallow, &c., the precipitates from which are of a much coarser and flocculent character than that from lard in ether. Hence we perceive there is a striking peculiarity about butter, which, if treated as above, enables us to readily determine its purity and the probable proportion of foreign fatty matters mixed with it. The solution of lard and other fats in ether is considerably influenced by temperature, for if the bottle containing them be held in the hand a short time liquefaction takes place, but on a reduction of temperature they are again precipitated. The character also of the various precipitates is remarkable, and gives us some clue to their nature,—the precipitate from lard being very fine and smooth, whilst that of dripping is granular and crystalline, and that from tallow long and thread-like, laying piled up one above the other. A solution of butter in ether, exposed to a less temperature than stated, yields beautiful stellar-like tufts of very fine acicular crystals.

The Laboratory, Police Station, Cheltenham.

On the Effects of Reducing Agents upon Nitrates of Ethyl, by M. CAREY LEA.

As ammonia under the influence of oxydizing agents is burned to nitrous and nitric acids, so these acids when acted upon by reducing agents, might naturally produce ammonia, or in the presence of a decomposing ethyl compound, might give rise to the formation of substituted ammonias. The experiment, however, seems to prove that while ammonia is a constant result of this reduction, it varies greatly in quantity according to the particular

reducing agent employed, and that substituted ammonias are not produced, at least not in recognisable quantities. The following were the reactions observed:—

Reduction by Protochloride of Tin.—Alcoholic solution of protochloride of tin added to nitrate of ethyl causes violent effervescence, but no disengagement of nitrous fumes. The solution of protochloride was added until reaction ceased. The whole was then distilled with caustic potash and the resulting gases conducted into dilute chlorhydric acid, which by evaporation left only a film of substance—this gave, on examination, distinct indications of ammonia.

Reduction by Sulphydic Acid.—A large quantity of sulphydic acid was conducted through nitrite of ethyl. Slight effervescence was produced and much sulphur deposited. The liquid filtered from the sulphur proved to contain a large quantity of ammonia, but no ethylamine, di-, or tri-ethylamine, although the mode of examination employed would have detected a very small quantity if present. After the removal of the ammonia, the solution heated with PtCl_4 gave a small quantity of a brown precipitate of a platinum salt, which when heated glowed for a long time, showing the presence of much carbon, and finally left a residue of reduced platinum. The amount obtained was too small for investigation, but 20 or 25 milligrammes of Pt salt were obtained from the decomposition products of 50 grammes of nitrite of ethyl.

This result, therefore, as far as the production of ammonia corresponds with that obtained by M. Emile Kopp,¹ although, as that chemist employed sulphydrate of ammonia instead of sulphydic acid, it does not appear that the production of ammonia by the reaction itself, could be positively established.

Reduction by Ferrous Acetate.—Nitrite of ethyl was added to about three times its bulk of ordinary acetic acid, and sufficient alcohol to make them mix. To this mixture iron filings were added. A powerful action set in without aid of heat. Deutoxyde of nitrogen was evolved in considerable quantity. When action had ceased, the resulting liquid was filtered, evaporated with chlorhydric acid and distilled with caustic potash. The distillate contained only traces of ammonia.

The effect of the reducing agents on nitrite of ethyl is therefore very different. Only with sulphydic acid was any considerable quantity of ammonia produced, and only with ferrous acetate was deutoxyde of nitrogen evolved.—*American Journal of Science*, No. 95.

Chemical Researches on the Assay of Silver by the Moist Way, by G. J. MULDER, *Math. Mag. Phil. Nat. Medicinæ, Artis Obstetriciæ et Pharmaceuticæ Doctor, Professor of Chemistry at the University of Utrecht, Grand Cross of the Order of the Netherlands Lion; of the Luxembourg Order of the Couronne de Chêne; of the Swedish Orders of Wasa and Polar Star, &c., &c.*

(Continued from page 206.)

In order to enable us to survey the whole phenomenon, we must start from this point of neutrality, leaving aside for the present the question whether this is precisely the point of true chemical neutrality.

In the liquid brought to the point of neutrality, solution of silver and salt produce a precipitate which is of the same strength in each case, the precipitate produced by silver taking, however, a longer time ere it makes its

appearance.¹ The liquid, therefore, which is precipitable in this way, by means of solution of salt and silver, undeniably contains nitrate of silver and chloride of sodium at the same time; for if the clear liquid which we say has reached the point of neutrality—i. e., in the first given instance after addition of ten, and in the latter instance after addition of thirty drops of the decimal solution of silver—did not contain silver in solution, chloride of sodium would not yield a precipitate of chloride of silver; and if, on the other hand, chloride of sodium were not present, nitrate of silver, on being added, could not give rise to a precipitate.

The clear liquid, therefore, brought to the so-called neutral point, contains nitrate of silver and chloride of sodium together in solution, combined in a peculiar manner, but not so intimately that the proper chemical affinity is quite suspended; for we find that if the clear liquid be divided into two equal portions, the addition of chloride of sodium to the one-half produces a precipitate of chloride of silver, while addition of solution of nitrate of silver produces a precipitate of chloride of silver, subtracting the chlorine from the chloride of sodium. The following table will illustrate this point:—

	I. The one half of the liquid brought to the point of neutrality.	II. The other half of the liquid brought to the point of neutrality.
Contains . . .	NO_2AgO ClNa	NO_2AgO ClNa
Added to it . .	NO_2NaO ClNa	NO_2NaO NO_2AgO
Produces insoluble	ClAg	ClAg
Retains in solution	ClNa NO_2NaO	NO_2AgO NO_2NaO

Although we have not entered into anything very strange, it will be as well to discuss more explicitly the simultaneous existence of NO_2AgO and ClNa . Let us return again to the experiment with three grammes of silver. Let us suppose that we have obtained the whole of the solution clear, after addition of thirty drops of solution of silver and subsequent shaking and settling of the precipitate, and, further, that we have divided the solution into two equal parts. Now, it is asked, what quantity of decimal solution of salt and decimal solution of silver can each portion of liquid bear in order to reach the limit of precipitability. It is difficult to solve this question by experiment, but from all that has been mentioned already, the answer to this question cannot be deemed a difficult one; for in order to reach the ultimate limit of precipitate, in the one half of the liquid of the solution of one gramme of silver, there will be wanted five drops of the decimal solution of salt, while in the other half of the same solution five drops of decimal solution of silver will be wanted. In the case, however, where three grammes of silver have been dissolved, the one half of that solution will require

¹ It is worthy of notice that the colour of the precipitate produced by the addition of nitrate of silver to solution of salt is slightly grayish; while, on the other hand, the precipitate produced by chloride of sodium in solution of silver has a perfectly white colour, the same may be observed when the experiments alluded to are made,—viz., the mutual precipitation. The precipitate in both cases is of different physical quality—only when formed in so greatly-diluted solutions as are here spoken of—from the ordinary precipitate of chloride of silver. This difference in the appearance of the precipitate is not merely due to the greater state of division owing to its formation in a very much diluted solution; but invariably the precipitate will be found characteristic and different from what it normally is as soon as one has reached that limit where a precipitate ceases to be formed, even when that limit is cautiously sought for while using the standard solutions one cannot fail to find this to be the case.—M.

fifteen drops of the decimal solution of salt, the other half fifteen drops of the decimal solution of silver.

In order to reach the limits of mutual precipitation, one gramme of silver dissolved required 20 drops; half of this is neutralised by 10 drops. If the last 20 drops added were solution of salt, then the 10 required will be silver. If the whole mass of liquid be divided into two equal parts, in the one-half 5 drops of salt, and in the other half 5 drops of silver solution will find material for precipitation. When we had to do with the solution of 3 grammes of silver, we first required 60 drops, which were neutralised by 30 drops of the reagent, and the mass of liquid being divided into two equal parts, 15 drops of decimal solution of salt, added to the half portion, and 15 drops of decimal solution of silver to the other half, will find material for precipitation in each of the glasses which contain the liquids just alluded to.

It may, perhaps, not be out of place to elucidate these matters by bringing them in the shape of a table, as follows:—

	1 gramme of Silver	3 grammes of Silver
	Require [after having been precipitated by solution of salt to the ultimate limit, temper- ature being from 15° to 17° C.] cubic centimetres of decimal solutions.	
	c. c.	c. c.
In order to be carried to the ultimate limit of the precipitate of silver, solution of nitrate of silver	1	3
In order to reach the point of neutrality, requires addition of solution of common salt	0.5	1.5
If the mass of liquid is now equally divided into two test glasses, each glass will exactly take of solution of nitrate of silver and of solution of common salt	0.25	0.75

I now return to what I have called mutual precipitation. In case the explanation given of what exists in the liquid when the point of neutrality² has been reached is correct, then it will not be difficult to explain how it is possible that—when one commences, for instance, from the ultimate point reached in making a silver assay, viz., after addition of solution of salt precisely in the quantity necessary for precipitation,—decimal solutions of silver and salt may be added, *vice versa*, until the limit of precipitability shall have been reached, and then back again, even an unlimited number of times, provided the chloride of silver does not change its colour.

In order to give this explanation in full, I will start from that moment when, in making the experiment, one has reached, on addition of decimal solution of salt, the limit of a visible precipitate. The liquid under experiment then contains what is mentioned in Table 1, viz., ClNa , NO_3NaO . And this being so, decimal solution of silver will yield a precipitate of chloride of silver. Let us take up the experiment again where we have 3 grammes of silver solution temperature 15° to 17° C., and let us add decimal solution of nitrate of silver, drop by drop, until 30 drops shall have been added, then there exists in that liquid what will be indicated below at A, while if we go on adding decimal solution of nitrate of silver, so that we pass over the neutral point (and this will happen with from 31 to

60 drops), there will take place in the liquid what will be found below in Table B.

A.—From 1 to 30 drops of nitrate of silver,—

The liquid contains . . .	Cl Na $\text{NO}_3 \text{NaO}$
Added	$\text{NO}_3 \text{AgO}$
Produces insoluble . . .	Cl Ag $\text{NO}_3 \text{AgO}$
There remains in solution	Cl Na $\text{NO}_3 \text{NaO}$

Neutral Point.

B.—From 31 to 60 drops of nitrate of silver,—

The liquid contains . . .	$\text{NO}_3 \text{AgO}$ Cl Na $\text{NO}_3 \text{NaO}$
Added	$\text{NO}_3 \text{AgO}$
Produces insoluble . . .	Cl Ag $\text{NO}_3 \text{AgO}$
There remains in solution	$\text{NO}_3 \text{NaO}$

Between A and B is the place of the neutral point, at which nitrate of silver, as well as chloride of sodium, yield an equally strong precipitate.

I will now give a sketch of what will exist after B shall have been treated with 60 drops of decimal solution of salt (solution of 3 grammes of pure silver, temperature 15° to 17° C.), when one requires the ultimate limit of precipitability:—

C.—From 1 to 30 drops of solution of salt,—

The liquid contains . . .	$\text{NO}_3 \text{AgO}$ Cl Na $\text{NO}_3 \text{NaO}$
Added	Cl Na
Produces insoluble . . .	Cl Ag $\text{NO}_3 \text{AgO}$
There remains in solution	Cl Na $\text{NO}_3 \text{NaO}$

Neutral Point.

D.—From 31 to 60 drops of solution of salt,—

The liquid contains . . .	$\text{NO}_3 \text{AgO}$ Cl Na $\text{NO}_3 \text{NaO}$
Added	Cl Na
Produces insoluble . . .	Cl Ag Cl Na
There remains in solution	$\text{NO}_3 \text{NaO}$

I hope I have herewith proved the *de facto* existence of mutual precipitation, and that this matter will be pretty clear now.

The following scheme may suffice for elucidation:—When the ultimate limit has been reached, the liquid, provided nothing more be added, contains dissolved:—

For Salt.	For Silver.
Cl Na	$\text{NO}_3 \text{AgO}$
$\text{NO}_3 \text{NaO}$	$\text{NO}_3 \text{NaO}$

Whence it is evident that in the first case, where silver meets common salt, it will yield a precipitate; while, on the other hand, chloride of sodium meeting with silver will produce a precipitate. From the facts just alluded to, it is quite evident, that he who makes an assay by adding solution of salt as long as a perceptible precipitate is produced, adds more salt than will be required for the silver according to equivalent proportions.

In case this were not true, the whole explanation given would be incorrect; and that explanation is no hypothesis, but the simple expression of facts.

(To be continued.)

² Point of neutrality means that where decimal solution of silver and decimal solution of salt yield an equally strong precipitate.

TECHNICAL CHEMISTRY.

Description of a Gas Furnace for General Chemical Use, by JOHN JOSEPH GRIFFIN, F.C.S.

I HAVE already published descriptions of several gas furnaces, adapted to special purposes.¹ The present notice refers to a simple form of gas-burner, which, aided by suitable fittings, can be used as a convenient source of heat for most operations of the chemical laboratory and lecture table. It will boil a quantity of liquid, exceeding two gallons at once; it will raise a 4½ inch fire-clay crucible to full redness; it will fuse anhydrous carbonate of soda in greater quantity than is required for the analysis of a siliceous mineral; and it will melt small quantities of sterling silver. This amount of power is sufficient for most chemical operations that are not metallurgic.

The Gas Burner.—Fig. 1 represents the gas-burner

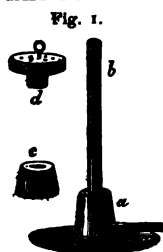


Fig. 1.

of this apparatus. The gas is supplied by the horizontal tube, whence it passes through a set of small holes into the box *c*, in which it mixes with atmospheric air that enters freely by the holes shown in the sketch. The gaseous mixture passes up the vertical tube *b*, and is inflamed at the top, where it burns with a single, tall, blue flame, which gives no smoke, very little light, but much heat. In this condition, the apparatus differs from "Bunsen's Gas-burner" only in size. *c*, represents a thin brass cap, which fits the air-box *a*, but moves easily round it. *d* is a flat, cast-iron box, with many holes round the margin, and a few small ones on the top. This box fits loosely on the upper part of the tube *b*, and when it is placed upon it, and the gas is lighted, the flame produced consists of a series of radiating jets, forming a horizontal circular flame of about 7 inches in diameter.

Fig. 2.

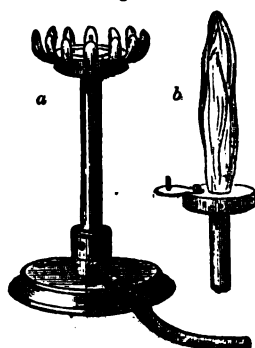


Fig. 2.—*a* shows a ring of horizontal flames thus produced, and *b* shows the single vertical flame.² The ring of flame is suited to the purposes of boiling and evaporation; the single flame to ignition and fusion. The height of the apparatus represented by Fig. 1 is 12 inches; the bore of the tube *b* is 1 inch; and the diameter of the fire-box *d* is 4 inches.

Bunsen's gas-burner, whatever its size, is subject to two defects: sometimes the flame burns white and smoky, and sometimes it blows down, the gaseous mixture explodes, and the gas then burns with a smoky flame in the air-box *a*, Fig. 1. The remedies for these defects are as follows:—If the flame is white only when the gas is turned on very full, the remedy is to lessen the supply of gas; but if the flame continues to burn white at the top when the gas is gradually turned off,

and the mass of flame slowly sinks, then the holes which deliver the gas from the supply pipe into the air-box are too large, and are placed too directly under the centre of the vertical tube *b*, Fig. 1, and these defects must be corrected in the instrument. Finally, when the flame blows down, it is because the supply of atmospheric air is too large in proportion to the supply of gas, and their relative proportions must be altered. To effect this alteration, the cap *c* is to be turned round on the air-box *a*, so as partially to close the holes, and thus lessen the supply of air. If, when the gas is alight, the flame needs to be lowered, first the supply of air is to be lessened, and then the supply of gas. If the flame is to be enlarged, first the supply of gas must be increased, and then the supply of air. In short, to prevent the flame blowing down, the gas must always be first placed in excess, and then have the proper quantity of air adjusted to suit it by means of the regulator *c*.

Arrangement for Boiling and Evaporation.

Fig. 3 represents this gas furnace arranged for boiling and evaporation. *a*, is the gas-burner, Fig. 1; *b*, an iron stool with three legs; *c*, a furnace body, or iron jacket, lined with plumbago or fire-clay. Fig. 4 shows the jacket and lining in section, and marks the position of the fire-box, *d*, of the gas-burner. This furnace is 14 inches high, and 9 inches in diameter. The three brackets fixed on the upper part of the jacket serve to support the vessel that contains the liquid that is to be boiled or evaporated. A porcelain basin of 16 or 18 inches in diameter can be thus supported. It is important to allow between the jacket *c* and the evaporating basin plenty of space for the escape of the heated air, which ascends from the interior of the furnace. When the evaporating basin is of small diameter, it may be supported on iron triangles, placed in the furnace *c*. Fig. 4 shows that, around the vertical tube of the gas-burner, *a*, there is in the bottom of the furnace, *c*, a circular opening, which is of 2 inches diameter, and through which air passes freely, partly to feed the flame and partly to be heated by the flame, and be directed upwards in a continuous current upon the lower surface of the basin that is to be heated. The flame within the furnace burns steadily. No side currents of air agitate it. No part of it touches, or must be permitted to touch, the basin, which should receive its heat solely from the mass of ascending hot air. The gas-burner thus arranged, and supplied by a gas-pipe of ½ inch bore, burns about 33 cubic feet of gas in an hour, and the flame which it produces, acting upon water contained in an open porcelain evaporating basin, will heat from 60° to 212° F.,—



Fig. 3.

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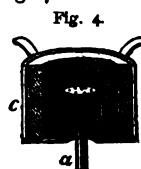


Fig. 4.

1 quart in 5 minutes
1 gallon in 15 "
2 gallons in 30 "

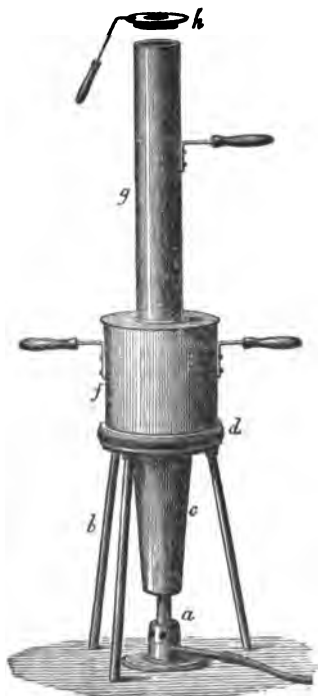
and when the water boils it is driven off in steam at the rate of more than a gallon of water per hour. The method is consequently applicable to distillation on a small scale, and to numerous operations in pharmacy

¹ See CHEMICAL NEWS, vol. i. p. 27, and vol. iii. p. 333.

² Fig. 2 represents a small variety of this gas-burner, in which the head is not removable, but the efflux of the mixed gases is regulated by a sliding valve, which is represented by *b*.

Arrangement for Heating to Redness a Large Fire-clay Crucible.—When a large crucible is to be heated to redness—as, for example, when oxide of copper is to be dried for use in an organic analysis,—the gas-burner is to be used without the fire-box *d*, Fig. 1, and is to be arranged with the furnace fittings that are represented in perspective by Fig. 5, and in section by

Fig. 5.



the lower part of Fig. 8, *a*, *b*, *c*, *d*. Letter *a* represents the gas-burner, Fig. 1; *b* is a tall iron stool; *c*, a chimney which collects atmospheric air to feed the flame, and leads it up close to the vertical tube *b*, by which contrivance the air is warmed and the tube cooled; *d* is a furnace-sole or plate of fire-clay; *f* is a reverberatory dome, the interior of which is best shown in the section, Fig. 6; *e* is a cast-iron ring or trivet, represented more clearly in Fig. 7; *g* is an iron chimney,

Fig. 6.

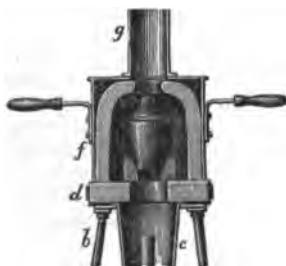


Fig. 7.



24 inches long, and 3½ inches wide; and *h*, a damper, to lessen the draught when small crucibles are to be heated. The height of this apparatus, from *a* to the top of *f*, is 24 inches; and the external diameter of the dome *f* is about 8 inches. The crucible, which may be from 4½ to 4¾ inches in height, is placed on the iron ring *e*, and

that on the clay sole *d*, and it is then covered by the dome *f*. The gas should be lighted after the crucible is placed in its position, and before the dome is put on. The dome and chimney are then to be added, and the operation allowed to proceed. With a crucible of the above size, the damper *h* is not required; but it must be used when the crucible is under 4 inches in height, otherwise the draught occasioned by extra space within the dome causes the flame to blow down. The damper must be put on the chimney before the chimney is put on the dome. The iron ring, Fig. 7, or *e*, Fig. 6, suits crucibles of different sizes, according to the side of it which is turned uppermost.

The Figures show that a crucible mounted in this furnace can lose very little heat by radiation or conduction, and hence it is that a small gas flame produces a powerful effect. In half-an-hour a 4½-inch clay crucible, filled and covered, can be heated to full redness. The progress of the ignition can be easily examined by lifting up the chimney *g* and the dome *f* by their respective wooden handles. But the action of the furnace can also be judged of by a peculiar roaring noise which it produces. If the gas and air are mixed in due proportions, the roar is regular and continuous. If there is too much gas, the roar is lessened. If too much air, the roar is increased, but is rendered irregular and intermittent. The greater the noise, the greater the heat in the furnace, but when the roar becomes spasmodic, the flame is on the point of blowing down. To prevent that occurrence, the proportion of air must be lessened or that of gas increased.

Arrangement for Heating Platinum Crucibles, as in the Fusion of Silicates with Anhydrous Carbonate of Soda.—The following arrangement is convenient when small crucibles are to be strongly heated:—Anhydrous carbonate of soda in quantities exceeding 1000 grains can be thus readily fused, and small quantities of sterling silver can be melted in a clay crucible. It is also available for ignitions or fusions in small porcelain crucibles. Fig. 8 represents the arrangement of apparatus as seen in section.

Fig. 8.



a is the gas-burner; *b*, the stool; *c*, the air-chimney; and *d*, the furnace sole, as already explained. *f* is a cylinder of fire-clay, 5 inches high, and 4 inches diameter; *k* is a plumbago or fire-clay furnace, in which is placed a small cast-iron ring, similar in form to that represented by Fig. 7, and on this ring the platinum crucible is adjusted; *l* is a fire-clay or plumbago reverberatory dome; and *g* is the chimney that forms part of the furnace represented by Fig. 5. The crucible being adjusted, the gas lighted, and the dome and chimney put on, the lapse of 12 or 15 minutes, according to the

quality and pressure of the gas, suffices for the fusion of 1000 grains of carbonate of soda in a platinum crucible.*

It will be noticed in Fig. 8 that the crucible is placed very high above the orifice of the tube *a*, at which the gas is inflamed. The distance is, in fact, about 10 inches, the point of maximum heat in the flame being at nearly that distance from the burner, more or less according to the pressure and the quality of the gas. The gas which rises from the burner *a*, though mixed with as much air as it will bear without becoming explosive when lighted, does not contain sufficient oxygen to burn all the carbon present in it. The flame produced is, consequently, quite superficial. The gaseous mixture only burns on the surface where it is in contact with fresh atmospheric air, and it requires time to take up the requisite amount of oxygen. The draught produced by the joint action of the two chimneys *c* and *g* carries the flame rapidly to a great height before the point of complete combustion is attained.

When the highest degree of heat is not required, the reverberatory dome *l* may be omitted. It must also be dispensed with when the crucible that is to be heated is of comparatively large size, because it is then liable to lower the temperature of the furnace by impeding the draught.

The effects that have been ascribed to the various arrangements of this gas-furnace can be produced with gas supplied by a pipe of a $\frac{1}{4}$ -inch bore, and at a moderate pressure, giving from 30 to 40 cubic feet per hour.

The principles of heating by gas, which have led to the construction of this furnace, may be summed up as follows:—When a crucible or other solid body is to be heated, it is to be wrapped in a single flame at the point of maximum heat, and loss of heat by radiation and conduction is to be prevented by the interposition of non-conducting materials (plumbago or fire-clay); and when liquids are to be boiled or evaporated, particularly when they are contained in vessels of glass or porcelain, the flame is to be broken up into numerous horizontal jets, and these are to be made to supply a large and regular current of highly-heated air, by which alone, and not by the direct application of the flame, the vessel that contains the liquid is to be heated. In both cases provision must be made to secure a sufficient draught of air through the furnace, because every cubic foot of gas requires for combustion 10 or 12 feet of cubic air, and the gases which have done their duty must be rapidly carried away from the focus of heat. If the steam, the carbonic acid gas, and the free nitrogen, which constitute the used up gases, are not promptly expelled, fresh gaseous mixture, in the act of producing additional heat by combustion, cannot get near the object that is to be heated, and the heat so produced out of place is wasted.

Casella's New Mercurial Minimum Thermometer.

A GOOD minimum thermometer has long been a desideratum to the scientific meteorologist. Hitherto, alcohol has been employed, but the constant sources of error arising from the use of this fluid, owing to its sluggishness, unequal expansion, and tendency to distil over into the upper part of the tube, rendered it very desirable to employ the same fluid—mercury—for registering extreme cold that is always used for estimating

heat. Several contrivances have already been suggested for effecting this object. Cumbrous arrangements of tubes were devised by men of science, which were most skilfully made by Mr. Casella, and still the difficulty remained almost as great as ever, until the construction of such an instrument was pronounced by one of the first authorities in England to be a physical impossibility.

The idea at length occurred to Mr. L. M. Casella that by a judicious adaptation of the parts of the instrument, so that the two forces, adhesion and gravitation, should be opposed to each other, the object so much desired might be attained; and, after many fruitless efforts, the application of this theory was crowned with success, and the result is embodied in the beautiful instrument now before us.

The peculiarity consists in the attractive or adhesive influence obtained by the junction of the small chamber (*a*, *b*), with the adjoining bent tube or chamber (*d*), as shown in the enlarged view (Fig. 1), the action of which is as follows:—On the instrument being set for observation, in a horizontal position, with the back plate (*e*) suspended on a nail, and the lower part supported on a hook (*f*); the bulb end may be raised or lowered, until the bent part (*d*) is full of mercury and the chamber (*a*, *b*) is quite empty; at this point the mercury in the bore of the tube indicates the exact temperature of the air at the time. On an increase of temperature the resistance of capillarity in the bore renders it easier for the mercury to expand into the small chamber (*a*, *b*) than along the narrow bore of the tube (*c*); whilst a return of cold will, for the same reason, cause its recession from this chamber only, until it reaches the flattened surface (*b*), to which the mercury adheres, as to a fixed point; when any further diminution of heat withdraws the mercury down the bore to whatever degree the cold may attain, where it remains until further withdrawn by increased cold, or till reset for future observation.

By this means, cold may be registered to any fraction of a degree observable on the most delicate standard, and no vicissitudes of climate or transit can in any way disarrange the instrument.

Solder for Brass Instruments.—An alloy of 78·26 parts of brass, 17·41 of zinc, and 4·33 of silver, with the addition of a little chloride of potassium to the borax, is recommended by Mr. Appelbaum, as the best solder for brass tubes which have to undergo much hammering or



FIG. 1.

FIG. 2.

* If this quantity of fused carbonate of soda is permitted to cool and consolidate in a platinum crucible, the salt is liable to expand and

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

*Ordinary Meeting, October 15, 1861.**Dr. J. P. JOULE, President, in the Chair.*

Mr. John Whalley was elected an ordinary member.

A Paper was read by G. V. VERNON, Esq., F.R.A.S., M.B.M.S., "*On the Irregular Barometric Oscillations at Geneva and on the Great St. Bernard, and their relations to the Mean Temperature and the Fall of Rain.*"

The object of this Paper was to deduce the effects produced upon the irregular oscillations of the barometer by considerable difference of elevation. The station at Geneva is the observatory under Professor Plantamour, and is 1335 feet above the sea. The station on the St. Bernard is the Hospice, and the observations made there are also under the direction of Professor Plantamour; its height is 8173 feet above the sea, and, consequently 6838 feet above the Geneva station.

The maximum amount of oscillation occurs at Geneva in January, and the minimum in August. The maximum at the St. Bernard occurs in December, and there appears to be two minima, one in June and the other in August.

Upon the whole, the amount of oscillation appears to diminish as the mean temperature increases, the minimum amount of oscillation occurring somewhat later than the maximum temperature.

The maximum number of oscillations occurs at Geneva in August; and there appear to be two minima, one in February and another in November. At the St. Bernard the maximum number occurs in August, and a single minimum in November.

The mean daily amount for the year at Geneva is 0.1069 inches; annual amount, 39.0719 inches: at the St. Bernard—daily amount, 0.0865 inches; annual amount, 31.5941 inches.

The total annual number of oscillations is, at Geneva, 1713; St. Bernard, 1572.

	Geneva.	St. Bernard.
Six winter months . . .	84.4 . .	76.2 . .
Six summer months . . .	86.9 . .	81.0 . .

1848 was the year of maximum oscillation, and 1857 of minimum oscillation.

A number of oscillations above the average at Geneva is accompanied by a less amount of oscillation than a number of oscillations below the average. At the St. Bernard the exact converse of this holds good.

In January, February, and December, a temperature above the average, at Geneva, is accompanied by a greater amount of oscillation than when the temperature is below the average. During the remaining months of the year the converse of this exists. For the year, a temperature below the average is accompanied by a larger amount of oscillation at Geneva than when the temperature is below the average.

At the St. Bernard, a temperature above the average, in the months of August and September, gives a larger amount of oscillation than a temperature below the average; during the other months of the year the converse of this holds good. For the year, a temperature below the average gives the largest amount of oscillation, as at Geneva.

A number of oscillations in excess of the average at Geneva, in the months of January, March, June, July, August, and November, is accompanied by a larger amount of oscillation than when the number is below the average; during the rest of the year the converse holds good. For the entire year, an increased number of oscillations is accompanied by an increased amount of oscillation.

At the St. Bernard, a number of oscillations above the

average, in the months of January, February, March, April, May, August, September, and November, is accompanied by a larger amount of oscillation than when the number of oscillations is below the average. In the remaining months of the year the converse of the above holds good. The mean of the year gives the same results as Geneva.

A rain fall above the average, at Geneva, is accompanied by a larger amount of oscillation, in every month of the year, than when the rain fall is below the average.

At the St. Bernard, a rain fall above the average, in the months of February, March, May, July, August, November, and December, is accompanied by a larger amount of oscillation than a rain fall below the average; during the remainder of the year the converse of this exists.

On the mean of the year, a rain fall above the average is accompanied by a less amount of oscillation than a rain fall below the average. This result is curious, being the direct converse of what was obtained for Geneva.

The conclusions which may be drawn from this investigation are the following. As we ascend in the atmosphere, the amplitude of the irregular diurnal oscillations of the barometer diminishes, more especially in the winter months; the summer months having an amount of oscillation not differing much from that of less elevated stations in nearly the same geographical position.

Excessive rain fall at stations of moderate elevation appears to be accompanied by a larger amount of oscillation than when the rain fall is below the average. At more elevated stations it appears as if this law was reversed, and, if found to be so, will be a very important fact.

Temperature below the average for the season greatly increases the amount of oscillation.

NOTICES OF BOOKS.

Report of the Committee on the Decay of the Stone of the New Palace at Westminster.

FIFTH NOTICE.

UNDER another head may be classed those processes in which hydrofluoric and hydrofluosilicic acids are employed alone or in combination with certain mineral bases. The suggestion of Professor Field and Dr. Bernays is founded on the employment, firstly, of hydrofluosilicic acid alone for the purpose of ensuring a thorough penetration of the stone, which, in the case of dolomite, is said to amount in depth to about an inch of stone penetrated in the course of half-an-hour. In the treatment of moderately compact descriptions of calcareous building-stone the hydrofluosilicic acid is applied as before, but subsequently washed over with a dilute solution of ammonia for the purpose of depositing gelatinous silica in the pores of the stone, and at the same time inducing the formation of a difficultly soluble compound, the fluoride of calcium. In the course of manufacturing the acid on any considerable scale, large quantities of flocculent silica are obtained as a secondary product, which it is proposed to economise by dissolving in potash, and using such solution in combination with the ingredients previously enumerated.

According to the degree of concentration of the acid employed, so will the effervescence, due to its action on the earthy carbonates of the stone, be more or less appreciable. Dr. Bernays prefers to use a very weak acid for the first application, and in that case the evolution of carbonic acid gas is scarcely perceptible, and cannot be supposed to present a barrier to the free absorption of the chemical agents into the substance of the block. The efficacy of the process does not appear to depend upon the production of a hard, superficial coating, but rather to encrust all the exposed faces, and to some extent even below the surface of the stone with a durable protective layer composed of

silica and fluoride of calcium in intimate union. It is asserted that the nature of these materials particularly qualifies them for resisting the action of those atmospheric influences to which dolomite seems especially sensitive. In the experiments of Dr. Bernays and Professor Field, blocks of Caen stone are described as having been taken to represent the material operated upon; but, strange to say, they make no mention of the modifications which would be introduced into the chemical reactions by the presence of the large proportion of carbonate of magnesia in dolomite—that kind of building-stone for which this special plan of treatment has been particularly devised.

In some respects similar to the foregoing is the process suggested by the Editor of this Journal. This refers to the employment of a preparation obtained by acting upon clay, kaolin, or Fuller's earth (silicate of alumina), with hydrofluoric acid. The chemical decomposition called into play when these ingredients are brought together is very complex, but we have ascertained that silico-fluoride of aluminium is formed as the chief product. The acid properties, although partly neutralised, are still capable of giving rise to an effervescence by contact with chalk; and to avoid too copious an evolution of carbonic acid from the limestone, dolomite, or other building material submitted to its action, it would in some cases be deemed advisable previously to correct any excessive degree of acidity by the cautious addition of an earthy base. A single wash merely of this solution is recommended to be applied direct to the surface of the stone. The principle upon which the employment of this compound is based cannot be fully explained while chemists are in doubt respecting the complex changes brought about by the mutual reaction between fluorine and silicon compounds with lime, magnesia, and alumina as bases. The products are, however, distinguished in all cases by their insolubility, and it is proposed to take advantage of the precipitation of such substances in the pores of stone as a means of protecting the exposed surfaces from the destructive influences of the surrounding atmosphere. According to one view, the result of employing a wash of the silico-fluoride of aluminium upon a calcareous stone will be the formation of silicate of alumina and fluoride of calcium; or otherwise if account be taken of the tendency of fluoride of aluminium to form double compounds by union with other fluorides, a kind of kryolite, in which sodium is replaced by calcium, may possibly be produced. Which ever view regarding the chemical constitution of the product be correct, it is our aim to produce through the agency of the substances described an insoluble mineral coating calculated to afford a considerable amount of protection to the surface of the stone.

A third process of the fluorine class is the subject of a provisional patent granted to Messrs. Jesse Rust and Company, who have, through the principal member of their firm and Mr. John Mossop, submitted to the Decay of Stone Committee the full particulars relating to their invention. This consists in the employment of two washes, the first an aqueous solution of caustic baryta, and the second hydrofluosilicic acid, by which double treatment the comparatively insoluble silico-fluoride of barium is formed by precipitation in the pores of the stone; or, according to a second opinion suggested by Mr. Rust, "the fluo-silicic acid combines with the baryta, forming fluoride of barium, which is insoluble in the acid itself, and the silica is set free. This being likewise insoluble, assists in closing the stone and in rendering it impervious to water." With regard to the preparation of the materials required in the process, a statement is made substantially to the following effect:—For the more economical manufacture of concentrated hydrofluosilicic acid, Mr. Rust departs from the usual system of decomposing gaseous fluoride of silicon by water. From silicate of soda he precipitates, by the addition of an acid, "free silica in the form of a pulp." By now passing hydrofluoric acid gas

through this product it is gradually dissolved, and the process is terminated when all is again taken into solution, which is much more readily accomplished than when sand is employed as the source of silica. "With regard to caustic baryta, I am making large quantities. We are glass-makers, and I take a glass pot, broken, that will not serve for glass again, clean it out thoroughly, and put in common native carbonate of baryta. I then expose it in this glass pot to the fire for various periods, say for two or three days, and it comes out perfectly caustic. Of that a great quantity is dissolved in hot water, and in operating upon the stone I use the solution hot, because a larger quantity of baryta is thereby held in solution."

Limestones coated according to the mode of treatment just now described, are said to be altogether protected from the action of weak acid; it is not stated which acid was used in the experiments, and there may be a considerable difference between sulphuric and muriatic acids in this respect; but it appears that whilst one portion of a stone which had been only partially treated was wasted away, the other (coated) portion well resisted the corrosive action. There is one circumstance which we cannot refrain from noticing in connection with the evidence offered by Mr. Rust. Whilst fully admitting the injurious effects likely to arise from the introduction of common salt into the pores of the stone, there is every probability that the method indicated for the preparation of the hydrofluosilicic acid would lead to its contamination by this or another equally injurious soda salt. The "silica pulp" would require very protracted washing to remove all the chloride of sodium adhering to it from its mode of formation, it would, in fact, be found difficult to separate the whole of the salt without sacrificing a considerable proportion of the soluble silica. All the salt left in the silica must reappear in the hydrofluosilicic acid, and consequently be eventually transferred to the block of stone.

The process of treatment suggested by Mr. Spiller, forms an isolated example of the application of phosphoric acid for the purpose of arresting decay in stone. According to this proposition, it is recommended to employ a solution of superphosphate of lime, which, applied as a single wash, has a very considerable power in hardening the exterior surfaces of all building stones that contain carbonate of lime as their principal constituent. With the object of effecting a more thorough penetration of the stone, it is in some cases considered advantageous to employ first a moderately dilute solution of the superphosphate, and to follow up this treatment with a second wash of the same, applied this time in a more concentrated form. A slight effervescence is apparent as the result of the chemical action of the superphosphate upon the carbonate of lime in the stone, and the change in the mineral constitution by which this is accompanied consists in the gradual transformation of a soluble acid phosphate into two equivalents of neutral phosphate of lime, which from their slow production and sparing solubility are separated in a crystalline condition, and go far towards filling up the smaller interstices of the stone. The porosity of a small block is shown by this means to have been diminished by about one-half; the piece of stone which originally absorbed eight per cent. of water, was after treatment capable of taking up only four and a-half per cent. From comparative experiments upon Bath and Portland stones, and upon common chalk, the increased degree of hardness becomes very apparent; the last showed this effect in the most conspicuous manner, it being found impossible to scratch with the finger-nail a surface of chalk hardened in the manner described. The action of the several corrosive agents which the treated stone is liable to encounter in the atmosphere, has not yet been fully ascertained; but so far as laboratory experiments indicate, there is every promise that a considerable amount of protection will be afforded by the coating of phosphate of lime.

NOTICES OF PATENTS.

Grants of Provisional Protection for Six Months.

2370. Charles Stevens, Charing Cross, London, "Improvements in the manufacture of steel, and in the hardening of metals."—A communication from Joseph Théophile Luce Rousselot, Paris.

2386. George Davies, Serle Street, Lincoln's Inn, London, "Improvements in the process of preserving provisions, and in the apparatus employed therein."—A communication from Alexander Lecomte, Montmartre, Paris.—Petitions recorded September 24, 1861.

2396. Thomas Richardson, Newcastle-upon-Tyne, "Improvements in the manufacture of muriate of iron for the purification of coal gas."

2410. Victor Sosthene Lete, Paris, "Improvements in aerated or sparkling white wines."

3412. William Clark, Chancery Lane, London, "Improvements in the manufacture of peat."—A communication from Auguste Marcellin Manrique Comte de Lara et du Ren, Boulevard St. Martin, Paris.—Petitions recorded September 26, 1861.

2440. Frederick Walton and Richard Beard, jun., British Grove Works, Chiswick, Middlesex, "Improvements in the manufacture of varnishes applicable to the waterproofing and coating of fabrics and other uses."—Petitions recorded September 30, 1861.

2456. William Maltby, De Crespigny Park, Camberwell, Surrey, "Improvements in the manufacture of starch and starch gum."

Notices to Proceed.

1382. William Albert Shepard, Pall Mall, London, "Improvements in obtaining products from coal, and in apparatus employed therein."—Petitions recorded June 1, 1861.

1392. Marc Antoine Francois Mennons, Rue de l'Echiquier, Paris, "An improved combination of metals for the production of a white alloy resisting the action of vegetable acids."—A communication from Benoni Ferdinand Trabuc, Nîmes, Gard, France.

1396. Henry Herbert Hazard, Nelson Terrace, City Road, London, "Improvements in cartridges."

1397. Alexander Prince, Trafalgar Square, Charing Cross, London, "Improvements in the manufacture of gas, and in the apparatus connected therewith."—A communication from Messrs. Schaeffer and Walcker, Berlin, Prussia.

1406. Heinrich Gottlieb Bernhard Roeber, Silvertown, Essex, "Improvements in the manufacture of insulators for telegraph wires, and in materials and machinery for coating telegraph wires."

1473. Andrew Brown, Waterloo Road, Liverpool, "Improvements in obtaining fresh water at sea by means of distilling apparatus combined with the cooking stoves, or otherwise."

1512. Robert Jobson, Dudley, and Cromwell Fleetwood Varley, Fortress Terrace, Kentish Town, London, "Improvements in posts or supports for telegraph wires."—Petitions recorded June 12, 1861.

1548. Thomas Routledge, Eynsham Mills, near Oxford, "Improvements in the manufacture of paper."

1616. Richard Howson, Middlesbrough-on-Tees, Yorkshire, "Improvements in barometers."—Petitions recorded June 24, 1861.

1640. John Cowan, Barnes, Surrey, "Improvements in apparatus for reburning animal charcoal."—Petitions recorded June 26, 1861.

1722. William Pask, Lydney, Gloucestershire, "Procuring a colouring matter from the refuse of ironstone called colour spert."—Petition recorded July 6, 1861.

2395. Alfred Vincent Newton, Chancery Lane, London, "Improvements in the construction of and mode of working

telegraphic apparatus."—A communication from Giovanni Caselli, Florence, Italy.—Petitions recorded September 25, 1861.

CORRESPONDENCE.

On the Estimation of Nitrogen in Manures and Other Substances.

To the Editor of the CHEMICAL NEWS.

SIR,—In the CHEMICAL NEWS of October 12 are published some remarks by M. Carey Lea on my process for the estimation of nitrogen or ammonia by means of chloride of zinc.

I regret that such a task falls to my lot as that of pointing out the errors of an eminent scientific man; but, in order to vindicate myself, it seems necessary that I should do so. The process upon which he has so freely given his opinion has evidently never been tried by him, his opposition being altogether upon theoretical grounds, and by his non-practical conclusions has undoubtedly done me an injustice. He remarks, "The proposal of a new mode of analysis such as that of Mr. Walker is a new proof how much it is to be regretted that any chemist should offer a new mode of analysis without having first controlled it by the analysis of one or more specimens of substance of known composition."

I perfectly agree with him that it would be matter for regret that any chemist should propose a new mode of analysis without having done all, and even more than he suggests.

The very fact of its having been practised by me for two years before being published, is surely proof enough that ample time had elapsed to give opportunity of testing its accuracy; as, during that time, upwards of one hundred estimations were made. It did not occur to me at the time to give along with the process the comparative results, as I considered that anyone trying it would test its accuracy for himself; but, in order to satisfy the gentleman who writes against the process that he has no foundation for accusing me of such inconsistency as recommending a process as accurate without first having satisfied myself that it was so, I give a few results, copied from my analysis-book.

I have taken the results obtained by the platina process as the standard of comparison:—

No.	Superphosphate of Lime.	Ammonia per cent.	process.	
			By Zinc	By Platina
No. 1	Superphosphate of Lime.	Ammonia per cent.	0.54	0.64
" 2	"	"	0.66	0.78
" 3	"	"	0.28	0.33
" 4	"	"	0.21	0.27
" 5	Dissolved Bones.	"	2.12	2.00
" 6	Manure.	"	3.33	3.15
" 7	"	"	1.33	1.20
" 8	"	"	3.68	3.42
" 9	"	"	4.63	4.62
" 10	Hoof and Horn Cuttings.	"	14.85	14.58

Five of these analyses by the platina process (Nos. 2, 3, 4, 5, and 9), and two by the zinc process (Nos. 5, and 9) were done by other chemists.

Analytical chemists, who know the difficulty of obtaining exactly similar results, even by the same process, will agree with me that those before us show a very trifling discrepancy, and surely justify me in saying, that I was sufficiently guarded in my statements, when recommending the process, and they likewise show that no dependence whatever can be placed on mere theoretical demonstrations, such as those M. Carey Lea has treated us to; that gentleman most emphatically declares that, "The necessary result must be an under-estimation of the ammonia, leading to errors of such magnitude as would render the process wholly unfit even for the roughest technical analysis."

It is a pity that he should have committed himself in

this way, as I can most emphatically assert that an under-estimation of the ammonia is quite exceptional, and when it does occur is mostly with substances very poor in ammonia, and even then of comparatively trifling amount: the tendency of the process is rather to slightly over-estimate it, taking as the standard of comparison the results obtained by the platina process.

Following up M. Carey Lea's process of reasoning, he takes a neutral solution of chloride of zinc, adds one drop of ammonia which occasions a precipitate, then adds, I presume, about a thousand drops of solution of chloride of ammonium when the precipitate is re-dissolved. True enough all this occurs; but why add solution of chloride of ammonium? it forms no part of the process recommended by me. This experiment appears to be the sum total of evidence against the process and should only pass for what it is worth. It comes to this: To what extent is oxide of zinc soluble in chloride of ammonium? I would have liked very much had M. Carey Lea informed us. His remarks made me curious on this point especially as the practical results do not at all agree with his theory, and I have consequently made some experiments with the view of answering the question. I took three equal quantities (Nos. 1, 2, and 3) of a solution of chloride of zinc specific gravity 1.012 and to each added equal quantities of solution of pure caustic ammonia. From No. 1 I collected the precipitate, washed, dried, ignited, and weighed. I found the weight to be 6.15 grains. From No. 2 I collected the precipitate and washed thoroughly. I then made a hole in the filter and washed the precipitate into an empty test glass, then added a solution of chloride of ammonium of known strength agitating the mixture well upon each addition until only a slight trace of the precipitate remained undissolved. By this method I ascertained that it required 450 grains of chloride of ammonium to dissolve 6.15 grains of oxide of zinc. To No. 3 I added solution of chloride of ammonium of the same strength as that added to No. 2, and found that it required 560 grains to dissolve 6.15 grains of oxide of zinc.

The solution of chloride of ammonium contained 100 grains per alkalimeter, and had a sp. gr. of 1.03. From these results may be deduced what influence the presence of chloride of ammonium will have in the analytical process, and as the oxide appears to be less soluble in a mixture of chloride of zinc and chloride of ammonium than in chloride of ammonium alone, it is but fair to test it by that standard.

The average amount of ammonia contained in manures is about 3 per cent., and 30 grains are generally taken for an analysis; this will produce a precipitate of oxide of zinc weighing 2.14 grains, and chloride of ammonium in solution weighing 2.82 grains, which will dissolve 0.03 of a grain of oxide of zinc equal to 0.04 per cent. of ammonia.

This trifling discrepancy surely cannot be enough to stigmatize the process as "grossly inaccurate," as often far greater differences are observable when two analyses of the same substance are made by the platina process, which as far as I can learn, is generally considered to be very accurate although tedious.

I have only, in conclusion, to reiterate my former statement made nearly twelve months ago, with this difference, that I have practised the process for nearly three years instead of two, and as it gives accurate results, I can with confidence recommend it.—I am, &c.

JAMES WALKER.

Crawford Street Chemical Works, Glasgow.

Estimation of Alcohol in Wine.

To the Editor of the CHEMICAL NEWS.

SIR,—In your last Number Mr. Horsley gives a method of determining the alcoholic contents of wines, which, from the absence of any acknowledgment, I infer he wishes

to pass off as his own. It should be known, then, that this method is to be found at page 157 of the translation of Mulder's "Chemistry of Wine," by Dr. Bence Jones. It was given, you may remember, at page 36, vol. 1 of the CHEMICAL NEWS, and excited some discussion at the time.

I am, &c.

SUUM CURIAUX.

P.S.—The special application is, of course, Mr. Horsley's. October 24.

Quality of Water at Leeds.

To the Editor of the CHEMICAL NEWS.

SIR,—I have been given to understand that some of the waters supplied to Leeds or its vicinity, and used for dyeing and other similar purposes, are so impregnated with iron as to be very objectionable. Could you or some of your readers inform me what are the sources of the Leeds water supplies, which of them furnish water containing iron, what analyses have been made of the latter, and what are the means used (if any) to get rid of the impurity in question?—I am, &c.

J. B. M.

MISCELLANEOUS.

Chemical Society.—The first meeting of the season of this Society will take place on Thursday next, November 7, at Burlington House. Chair to be taken at 8 p.m.

Nitrates in Well-water.—In reference to Dr. Haince's paper on "Nitrates," the following analysis of a well-water at Tewkesbury, analysed by Herapath, has been forwarded to us:—

Chloride of calcium . . .	37.02
Nitrate of lime . . .	45.06
" potash . . .	42.09
Sulphate of soda and magnesia .	86.97
Chloride of sodium . . .	21.76
Carbonate of lime and magnesia	73.30
Sulphate of lime . . .	40.09

346.29

Photo-Electric Apparatus.—A Trappist named Delatol-Sevin, of the Abbaye de la Grâce-Dieu, has invented a new pile, much stronger, and at the same time much cheaper, than the pile of Bunsen. By means of his photo-electric apparatus he produces an electric light as cheap as gas, and with his thermo-electric pile he supplies calorific on economic terms hitherto unknown. Several of these apparatus have been constructed, and one is at full work in the Abbaye of La Grâce-Dieu. Manufactories for the public are shortly to be established in Paris and at Lyons. The apparatus for producing gas will not be given to the public until after the Exhibition at London next year, but that for heating buildings will be made public on the 16th of December next. The inventor has been authorized to make public experiments with his system of lighting on the Place Saint Jacques in Paris, and on the Place Bellecourt at Lyons.

Curious Lunar Phenomenon.—F.M., writing from High Wycombe, on the 17th October to the *Morning Star*, says, "Can I claim a corner for the following account of a remarkable and extraordinary appearance I witnessed on the disc of the moon on the night of the 14th, about nine o'clock, with an achromatic telescope, magnifying power about 170? In examining the cup-like cavities situated near the bright spot Kepler, on the irregular edge of the planet, I observed four smaller spots, in the shape of an irregular square, standing out in bold relief in the dark sky, surrounded by or emitting a beautiful prismatic luminous light, twinkling like the bright star Sirius. Having from time to time observed the moon

pretty nearly in all its phases, I never saw before the cup-like cavities of that orb as I did then, and in that particular spot, sparkling like some variegated coloured gems. At the time I could not account for it. I thought probably my glass deceived me; but recollecting that it was a pure achromatic lens, free from colour, I could not come to any other conclusion than that the spectacle was four active volcanoes in operation. The luminous light I observed was the reflection of external fiery matter pouring down the sides of the central mountains out of the mouth of the volcanoes. I merely state what I saw, and venture my humble opinion, subject to correction, drawing my conclusions. I always supposed the moon devoid of an atmosphere, but if the above, as I assume, were volcanoes, as a matter of course taking that body to be of the same organisation and the same matter as our earth, an atmosphere must be necessary for combustion in that body as well as for the latter. Perhaps, Mr. Editor, some of your numerous readers can explain the phenomena I witnessed."

Discovery of Gold in the Valley of the Saskatchewan.—Mr. A. K. Iebister, author of a paper on the Hudson's Bay Territories, says:—"I have just received a letter from the Red River colony, in the Hudson's Bay territory, announcing the discovery of a valuable gold field by a party of American miners from California, on the north branch of the Saskatchewan. This intelligence I see confirmed in recent papers published at St. Paul's, Minnesota, and it may therefore be relied on as accurate. Two gentlemen from California have recently arrived in that town with a considerable quantity of gold collected by themselves along the upper waters of the Saskatchewan, and they describe the yield and quality of the metal as promising to be quite equal to that of British Columbia, which I need hardly say occupies the opposite flank of the Rocky Mountains, and may therefore reasonably be inferred to be of a similar geological character, while we know that it is watered by streams which have a common origin in the same auriferous range with the northern tributaries of the Saskatchewan."

Action of Poison on Wild Animals.—For the purpose of poisoning them strychnia is used. I have tried aconitine, atropine, and corrosive sublimate without success. The two former may not have been pure enough, though I obtained them from the first chemical works in England and at a very high price. The only poison that I have found strong is strychnia. One or two grains of this are mixed with a little tallow, forming a small ball, and covered with a coating of grease outside to prevent the animal from tasting it. A quantity of pounded dried meat and morsels are strewn about so that the animal after swallowing the poison may be detained a sufficient time for it to operate. The distances which animals go before they die vary greatly; in some instances they fall directly, in others they run several miles with the same dose, and arranged in like manner. This I attribute to several causes; to their fatness, and to the quantity of food in their stomachs, as lean and hungry foxes die much more quickly than others. The medium in which the poison is given also causes a great difference. When put up in fresh meat a very long time elapses before it operates. Wishing to preserve a specimen of the Hare-Indian dog for the Smithsonian Institution, I resolved to kill the animal by poisoning. Two grains of strychnia of the first strength were administered in a piece of fresh meat; at the end of two hours the animal was as well as ever. I then administered one grain more mixed with grease, in two minutes the spasms began, and in five the animal was dead. The first symptoms were a restlessness and contraction of the pupil of the eye, and a flow of saliva from the mouth, violent cramps then ensued, the head shook violently, like a paralytic person, the legs were drawn up,

and the spine took a circular shape, a lull of a few seconds then ensued, when after an attack of great violence the animal died. On dissection the blood vessels of the head and neck were found very full of black and clotted blood, such as I have seen in the jugular vein of a person who had died of apoplexy. There was no inflammation of the stomach, and the fatal bait was found in the throat entire. Once seen, the symptoms of poisoning by strychnia are easily recognised, and I would be certain now of passing a correct opinion on a case of the kind. Dogs take a longer time to expire than either wolves or foxes; the latter dying most quickly; in fact according to the ratio of the wild nature of the animal who eats it will be the quickness and violence of its death.—B. R. Ross, in the "*Canadian Naturalist*."

On the Natural Dissemination of Gold.—Mr. Eckfeldt, the principal assayer of the United States Mint at Philadelphia, has lately made several interesting examinations tending to show the very wide distribution of gold. Passing over the evidence respecting its presence in various galenas, in metallic lead, copper, silver, antimony, &c., we recite the following, perhaps the most curious result of all:—Underneath the paved city of Philadelphia there lies a deposit of clay, whose area, by a probable estimate, would measure over three miles square, enabling us to figure out the convenient sum of ten square miles. The average depth is believed to be not less than fifteen feet. The inquiry was started whether gold was diffused in this earthy bed. From a central locality, which might afford a fair assay for the whole, the cellar of the new market house in Market Street, near Eleventh Street, we dug out some of the clay at a depth of fourteen feet, where it could not have been an artificial deposit. The weight of 130 grammes was dried and duly treated, and yielded one-eighth of a milligramme of gold; a very decided quantity on a fine assay balance. It was afterwards ascertained that the clay in its natural moisture loses about fifteen per cent. by drying. So that, as it lies in the ground, the clay contains one part gold in 1,224,000. This experiment was repeated upon clay taken from a brickyard in the suburbs of the city, with nearly the same result. In order to calculate with some accuracy the value of this body of wealth, we cut out blocks of the clay, and found that, on an average, a cubic foot as it lies in the ground, weighs 120 pounds, as near as may be; making the specific gravity 1.92. The assay gives seven-tenths of a grain, say three cents' worth of gold to the cubic foot. Assuming the data already given, we get 4,180 millions of cubic feet of clay under our streets and houses, in which securely lies 126 millions of dollars. And if, as is pretty certain, the corporate limits of the city would afford eight times this bulk of clay; we have more gold than has yet been brought, according to the statistics, from California and Australia. It is also apparent that every time a cart-load of clay is hauled out of a cellar, enough gold goes with it to pay for the carting. And if the bricks which front our houses could have brought to their surface, in the form of gold leaf, the amount of gold which they contain, we should have the glittering show of two square inches on every brick.

ANSWERS TO CORRESPONDENTS.

Gum Water.—W. C.—The best receipt with which we are acquainted for good gum water which will not crack off nor turn sour is:—Best gum arabic, half pound; loaf sugar, 8 medium sized lumps; creosote, 5 drops; water, to make up the bulk to 20 fluid ounces.

Des Cartes' Principia.—A. K. K.—We have not met with an English translation of this work.

J. Walker.—Received with thanks.

Mr. Bowditch.—The slip is received with thanks. It is very interesting, if true.

E. B.—In our previous Numbers.

Books Received.—Elementary Treatise on Physics, by Professor A. Ganot. Translated and Edited by E. Atkinson, Ph.D., F.R.S. Part I. London: H. Baillière, Regent Street. "The Canadian Naturalist and Geologist" in exchange.

Technologist.—Next week.

the chloride of copper, either by dissolving the oxide of copper or the native carbonate in hydrochloric acid, or by double decomposition with sulphate of copper, chloride of barium, &c. &c. Evaporate and crystallise the solution of chloride of copper thus obtained, mix it with sand, and dry it absolutely (probably in a reverberatory furnace). Introduce the dried mixture into retorts similar to those employed in the manufacture of gas. If these retorts are of cast-iron, they should be covered with a coating of earth mixed with charcoal, to isolate the metal. The chloride, greatly heated, decomposes into chlorine and sub-chloride.

The protochloride, the residuum in the preparation of chlorine, is not lost; it may be reconverted into chloride by the oxidising action of the air in presence of hydrochloric acid. The reconverted chloride is treated in the way just pointed out; and so the circle of reactions go on indefinitely.

It will be seen that this process corresponds with that of M. Dunlop-Tennant. It is atmospheric oxygen which, by reproducing bichloride of copper in this process, and binocide of manganese in Tennant's, effects the oxidation of the hydrochloric acid. The advantages of M. Laurens' interesting reaction are the following:—A saving of half the hydrochloric acid, and regeneration of the comburant by a direct and single operation. The objections to the process are the dearth of copper and the many chances of loss of the metal, either as chloride during its decomposition, or of protochloride or chloride in some of the unavoidable removals; moreover, it is to be feared that handling large quantities of chloride of copper the dust arising from it in a chlorine manufactory would be likely to injure the workmen's health. In any case, this interesting process is highly to be recommended for laboratories, for furnishing dry chlorine readily, without the intervention of a fragile and cumbrous apparatus.—*Rép. de Chimie.*

TECHNICAL CHEMISTRY.

On the Induration and Preservation of Stone,
by Messrs. BARTLETT BROTHERS and Co., Camden Town.

AMONGST the many efforts to effect the preservation of stone, we respectfully ask you to chronicle our own in the pages of your journal. It would ill become us to disparage other processes, or attempt to laud our own invention in your columns. We propose, therefore, to confine ourselves to a full description of the invention, the materials we use to accomplish our end, and the behaviour of these materials separately and in combination. Enough has been said in this journal to show, from logical and chemical reasoning, that silica must form a principal feature in all efforts to produce an eligible material for purposes of induration. Silica, then, is the principal material in our process, and that in the form of silicate of potash. Of this material in itself we need say but little; but, lest silicate of soda and potash should here be deemed synonymous, we would anticipate our remarks by saying that in this process they are by no means identical, the silicate of soda producing results both unsatisfactory and valueless. The second material is the aluminate of potash. Of this, in the form of an aqueous solution, we find it stated by all chemical authorities to the present time that it precipitates an hydrate of alumina, difficultly soluble in excess of the precipitant; and thus we found it behave after forty-eight hours, precipitating most bulkily in heavy solutions, and as much so in proportion

in light specific gravities. We need not say how valueless was such a solution commercially, nor tell the difficulties attending the discovery of a simple remedy, so simple as the determination of the particular specific gravity at which it would not precipitate. The behaviour of the aluminate with water is remarkable, seeing that one atom of alumina held in perfect solution by say three atoms of water should be precipitated by the addition of a fourth, or by the withdrawal of one of the three atoms. Such facts we leave to the investigation of those who may be pleased to pursue them further. Suffice it to say, then, that the aluminate used is prepared from a fused compound of alumina and potash, which product, being highly deliquescent, is easily soluble in water, and so nearly neutral as to contain in its best form of manufacture but two per cent. of free potash.

In the combination of silicate of potash and this aluminate of potash consists the process, the consideration of which affords some most interesting details. When silicate of potash, sp. gr. 1.250, and aluminate of potash, sp. gr. 1.200, are mixed together, an instantaneous decomposition takes place, and the result is a solid mass, consisting of silicate of alumina and some free potash. This hardens with extraordinary rapidity, and is a most beautiful example of the great affinity of silica for alumina. But take a solution of a lighter specific gravity, and we find that decomposition does not instantaneously take place. On the contrary, the liquidity of the two solutions in combination is retained for a time, only, however, proportioned to the quantity of water with which it is diluted. Thus a specific gravity of 1.150 will last as a solution ten hours, while at 1.200 it solidifies immediately. The cause of this arrest of the decomposition—if, indeed, it is arrested—and the peculiar part the water plays in this interchange of elements, will yield a field of interesting inquiry. With the results, however, we have now more immediately to deal; and

Firstly, the time so necessary for manipulation is given in the use of these materials which in themselves secure an insoluble product by their mutual decomposition. Secondly, that the agent and re-agent being mixed in one solution, there can be no fear of the one or the other being in excess or unneutralised, as in the use of acid or second solutions. Thirdly, the product resulting from this combination of silicate of potash and alumina is insoluble in dilute sulphuric and hydrochloric acids. And lastly, if Ansten, Bath, and Caen stone or chalk be pounded, this product will recombine them, thus showing that its chemical affinities are in favour of the material with which it is proposed to impregnate the stone. Finally, in the works of Fuch's and the brochures of Kühmann the foreshadowing of this process may be seen; and in the Report of the Commission appointed to investigate the causes of the decay of the Houses of Parliament, there are mentioned, though not in conjunction, the very materials proposed to be used by this process. We trust that our researches in this direction may prove so successful as to benefit the public at large, and afford its quota of like satisfaction to ourselves.

Sugar in the Urine of the Fox.—It will be of some interest to physiological chemists engaged on the hepatic and sugar question, to learn that De Vintschgau (*Sitzungsber. der K. Akad. der Wissenschaften zu Wien*, Bd. iii. s. 523) has found small quantities of sugar in the urine of the fox, a purely carnivorous animal. The method he used to detect it was that of Brücke.

PHARMACY, TOXICOLOGY, &c.

On the Analysis of Stains, supposed to be caused by Blood, by THOMAS D. TOASE, Esq., F.C.S., F.S.A., Jamaica.

It is a matter of the highest importance to know if stains, supposed to be caused by blood, can be correctly analysed; and also if it can be determined what animal the blood came from, but more especially if it is human blood. These questions generally bear reference to judicial cases, and often involve a matter of life or death.

It is the more necessary to examine these questions, as certain authorities have asserted that not only can blood stains be analysed correctly, but that even the animal from which the blood came can be firmly established.

I will examine, one by one, the tests given us by these authorities.

If the spots are upon some fabric, they must be cut out, but if not, they must be carefully scraped off. In either case, they must be placed in some clear water, keeping the spots in the water, but near the surface. If these spots are blood spots, a heavy stream will fall to the bottom of the water. This is the method given by my late much lamented master, M. Orfila, the celebrated toxicologist. I mention this, as Dr. Taylor, in his late work on Medical Jurisprudence, would seem to take the credit to himself of originating this simple mode.

Supposing that the heavy stream falls, there is certainly an indication of blood; but if there is no stream, an indication of the presence of blood is wanting, chemically speaking.

If the heavy fluid is formed, then take the pieces out of the water carefully: draw off the supernatant water with a pipette, so as to leave only what may be presumed to be blood; with care this can be easily managed (Orfila). Put this fluid into six test-tubes, and a portion on a glass slip to be examined later under the microscope; these portions to be tested in the following manner:—

The quantities, of course, are so small that it would be useless to examine the spots for fibrine, important though such an examination would be, if possible. On an average, in a healthy body, blood is said to contain only two parts in a thousand of fibrine. Seeing the smallness of the quantities, the attention of the analyst must be directed to the albumen and the iron. It will be useless to seek for the characteristic colouring matter of the blood (hematosine) after the lapse of some days, these spots being, in all probability, several days old before they pass into the hands of the analyst.

Before examining the tests, I must draw special notice to this fact, in the two tests I shall first mention, the spots must be fresh, as these two tests do not show themselves on blood that has been kept for some time. Indeed, in one case mentioned by Mr. Taylor, some true spots of blood which had been operated upon, refused to answer to these two tests after twenty-four hours in warm weather. I shall, however, mention them:

1st.—If the stains are recent, take a little of the red fluid that has been collected, and mix it with some water; if this fluid dissolves readily in the water, and impart to it a rich red hue, the colouring matter of blood (hematosine) is indicated. I cannot but consider this test as valueless, not only because freshness is a necessity, but because other colouring matter may pro-

duce the same tint when dissolved in water, and the supposed blood-stains may be only stains produced by other colouring matter, not hematosine.

2nd.—To this solution add a few drops of weak ammonia, taking special care to add very little ammonia (a glass rod dipped in a weak solution of ammonia, shaken and dipped in the red solution, will be the safest). If the red solution becomes crimson or green, it is not blood; but if the solution remains unchanged, then the evidence is still stronger that this colouring matter is the hematosine of blood. This is certainly a most valuable test, if there are no red colouring matters, other than hematosine, which are unaffected by ammonia.

But I cannot dwell too strongly on the fact, that these two tests, indicating the presence of the colouring matter of blood, require that the stains be fresh.

In the following tests freshness is not necessary.

3rd.—Take some of the thick liquid, collected from the stains; add a quantity of concentrated ammonia, and see if the colour now changes to a brown tint; if so, I think we have reason to suspect iron, but we are far from having established its presence, to prove which we must apply all the known tests for iron, which our small quantities to be tested will not allow. We must, therefore, rest contented with a mere indication. And even should we determine the presence of iron in the stains, that would not prove, of itself, that the stains were blood-stains. It is evident that this test possesses but little value, seeing that we can produce the same reaction from other stains besides blood-stains.

4th.—Boil a little of the red fluid, and see if it coagulates, if the colour is destroyed, and if a flocculent brown precipitate is formed. The coagulation will indicate albumen, but that is all.

5th.—Take the flocculent brown precipitate, filter and dry it, and see if it becomes a black resinous substance, insoluble in water; if so, then add some caustic potash, boil again, and see if a green coloured solution is formed. This would lead us again to suspect iron.

6th.—Now take some of the suspected fluid, add to it some strong nitric acid, and if the fluid coagulates albumen is again indicated.

But I must protest against these tests; they are un-chemical and unsatisfactory; we can obtain from them, at most, but indications of iron, albumen, and, if the spots are fresh, hematosine. I have purposely employed the term "indications," for I most emphatically assert, that no chemist has a right to say "such and such is the composition of such a body," if each material composing that body has been determined on the faith of one or two tests, and these tests the very reverse of conclusive; such reasoning could not be admitted in a chemical laboratory. As an analytical chemist, I would not do so in any commercial analysis, fearing a mistake on my part, leading to future exposure by some brother chemist, as in a case I well remember: a chemist had relied on two tests, and on them determined that a certain substance contained lead; another chemist examined the same substance more carefully, and found that it contained bismuth, and not lead. If a chemist dreads the exposure of a mistake, caused by an incomplete analysis of a mere commercial substance, how much more would he fear swearing a fellow creature's life away on mere indications?

For I assert, that to make a complete analysis of a fluid, suspected to be blood, that the iron, the albumen, the hematosine, and the fibrine should each be separated and each examined by all the known tests for each; and

if one test fails, I maintain that the analysis is incomplete, according to laboratory practice.

But there is another important aid, the microscope, which determines the presence and the form of the corpuscles, if present, this is a most valuable test; but it is simply a microscopical test, unsupported by a satisfactory chemical analysis.

I, therefore, come to the conclusion that, considering the small quantity of matter to be acted on, we can only obtain presumptive evidence that blood is the cause of these stains. I must again remind the reader that the determination of these questions may involve a matter of life or death, and that the accused has a right to the benefit of a doubt, the more especially as such an examination as I have just described, made in a chemical laboratory, where only commercial interests are concerned, would not be received.

I am loath to leave this subject without giving a striking proof of how elementary our knowledge of blood is. Fowne's work on Chemistry, a text book in very general use, contains this assertion:—"The colouring matter of blood contains albumen, and coagulates by heat, and by the addition of alcohol; this albumen cannot be separated, and all attempts to isolate the hematine, or red pigment, have consequently failed. From its extreme susceptibility of change nothing is known of it in a state of purity." Now, Muller, in his "Manual of Physiology," says quite the reverse:—"When blood, coagulated by alcohol, is boiled in this re-agent, the hematine is dissolved, and we thus succeed in freeing it from the whole of the adherent albumen."

We have now to determine if human blood, submitted to a microscopic examination, can be distinguished from the blood of any other animal. The circular form of the globules confines our attention to mammiferous animals, to the exclusion of the dromedary and the llama. This subject has already been so satisfactorily discussed by Taylor, that I cannot do better than subjoin the following quotation:—"The only microscopic distinction between the blood of man and domestic animals, consists in a difference in the size of the blood globules. This, however, is only an average difference, for the globules are found of very different sizes in the blood of the same animal. In making use of this criterion, it would be necessary to rely on the majority of the corpuscles seen in a given area, and under the same power of the microscope. The corpuscles in man, the dog, the rabbit, and the hare, are nearly of the same size. In the blood of the sheep and goat, they are smaller than in that of any other animal. According to Gulliver, the measured diameter of the globules in human blood varies from $\frac{1}{1000}$ th to $\frac{1}{2000}$ th of an inch. From the examination of various specimens of human blood, I (Taylor) have found the average diameter of the globules to be the $\frac{1}{1500}$ th of an inch, the maximum size being the $\frac{1}{1000}$ th and the minimum $\frac{1}{2000}$ th. According to Gulliver, $\frac{1}{1500}$ th in the ox. In the cow, according to my measurement, $\frac{1}{1000}$ th to $\frac{1}{1500}$ th. In the sheep, according to Gulliver, $\frac{1}{1500}$ th; according to my measurement, $\frac{1}{1500}$ th to $\frac{1}{2000}$ th. In the goat, according to Gulliver, $\frac{1}{1500}$ th.

"These measurements apply to recent blood, which has not been allowed to dry in animal and vegetable stuffs. In this case a distinction might be made between the blood of a human being and the sheep. When blood is dried on clothing, and it is necessary to extract the corpuscles by means of a liquid of a different nature from the serum, we cannot rely on slight fractional differences, since we cannot be sure that the globules,

after having been dried, will ever re-acquire, in a foreign liquid, the exact size which they had in serum. Medical evidence must, therefore, be based, in such cases, on a mere speculation."

Many other considerations might be added, but they appear superfluous after the very conclusive summary I have just quoted.

NOTICES OF BOOKS.

Report of the Committee on the Decay of the Stone of the New Palace at Westminster.

SIXTH NOTICE.

THE processes which have been submitted to the Decay of Stone Committee naturally arrange themselves under two distinct classes, those which have for their object the production of a mineral coating upon the surface of the stone, and secondly, those in which the protective agent belongs to the organic division, and consists either of bituminous or oleaginous materials alone, or applied in combination with but small proportions of mineral ingredients. In the course of our previous notices we have briefly described all the suggestions and proposals which come within the first or mineral division. It remains, therefore, to proceed to the consideration of some five or six processes in which materials are employed, all of the organic class, but differing very considerably in regard to their probable power of endurance. The chemists on the Committee, in their report of June 17, have been careful in distinguishing between the properties of certain organic substances, which, although they may fall under the same natural division, exhibit, nevertheless, very different characters. In reference to this point, they aptly remark, that "of the second division of processes—namely, those which are only calculated to afford protection of a temporary character—we would recommend that, in addition to the experiments already made in this direction, further trials be instituted of some of the more promising materials of this particular description. This recommendation is based upon the consideration that substances included under the appellation of Organic differ essentially in their powers of resisting the destructive action of the atmosphere. Who ever is acquainted with the nature of organic substances cannot fail to appreciate the different degrees of stability under atmospheric influence exhibited by gluten, gelatin, or starch (which we find enumerated among the proposed protective agents), and by bees' wax and paraffin, not to speak of many of the fossil gums, which exhibit a degree of permanence approaching that of mineral substances. The materials which we would recommend for selection to be tried in comparison with linseed oil are paraffin, bees' wax, and some of the more permanent gums and resins, applied in the form of solutions in volatile solvents."

Of these processes, the first which received application to the Houses of Parliament was the "patent vulcanising and preserving process" of Mr. Daines. This mode of treatment consists in applying a mixture of linseed oil and sulphur, which is merely brushed over the surface of the stone, three or as many as five coats being given according to circumstances. The sulphur does not appear to be employed in larger amount than is capable of dissolving in the oil, so that none is actually laid on the stone in the solid form. A certificate to this effect, signed by Mr. Dugald Campbell, is handed in as part evidence. This document tends to show that the sulphur is not merely deposited on the surface, but penetrates the substance of the stone fully as deep as the linseed oil; that, in fact, no separation of ingredients occurs.

As an indication of the early period at which the Westminster Palace showed the first signs of decay, it is important to note that the sanction of Sir Charles Rennie

having been obtained, Mr. Daines commenced his operations upon the building as far back as the year 1854, "shortly after the execution of the masonry, and when it was quite clean." A comparison of the work coated at that time or soon afterwards with other portions of the exterior not so protected, forms the subject of a favourable report signed, among other architects, by two members of the present committee, by Dr. Rutledge, and by Mr. Edward Macmurdo, chemist, of the firm Davy, Macmurdo, and Company.

The general appearance of the prepared surfaces of stone is said to be cleaner and brighter than the adjacent untouched portions of the work. In one part only—that first treated—an unsatisfactory discoloration is complained of; this is, however, sought to be explained by the circumstance that "the American potash used in cleaning the surface had been mixed in new oak pails." In reference to this preparatory treatment, we cannot but think that the use of a material which would have a tendency to impart to the stone surfaces a deliquescent character, was highly injudicious, and that such a step would be calculated to prevent that thorough contact and penetration of the stone by the preservative agent which must be the chief aim of this and similar processes. As the only further subject of remark which occurs to us to mention in regard to Daines' process, is the well ascertained property of sulphur, when in a finely divided condition, to undergo oxidation, becoming converted, indeed, into nothing short of sulphuric acid by exposure to the atmosphere. Flowers of sulphur when tested with moistened blue litmus paper, will always be found to communicate an acid reaction, and water filtered through them will usually afford with chloride of barium an immediate indication of sulphuric acid. Now, as the general result of the chemical examination of the efflorescent matters which are constantly forming on the decayed surfaces of dolomite was shown by Mr. Warrington (*vide* Mr. Ransome's evidence) to consist of 90 per cent. of sulphate of magnesia, and only small quantities of the sulphates of lime and soda, it must, we think, be in every way an unsatisfactory proceeding to apply to the stone of the Houses of Parliament that substance which when oxidised, is known to be peculiarly active in inducing disintegration. The presence of linseed oil, though it may lessen, is not likely to overcome altogether this tendency of sulphur to undergo oxidation.

A Manual of Chemistry, Descriptive and Theoretical. By WILLIAM ODLING, M.B., F.R.S. Part I. London: Longman and Co. 1861.

SECOND NOTICE.

In a former number we noticed the chief characteristics of Mr. Odling's Manual; we now propose to criticise with greater fulness, not so much the Author's method of handling his subject, as some of the more novel or striking of his views and statements,—those, in fact, which point out the work as no ordinary production. The Author, indeed, fully acknowledges his obligations to previous writers who have adopted the Unitary Notation; such, for instance, as Laurent, Gerhardt, Kekulé, and Hofmann. While he has elaborated, supplemented, and modified the general theory of the first-named chemists, he has applied several of the happy suggestions of the Belgian Professor to that department of the science generally known as inorganic chemistry. But, after all, the work is marked by great originality, and we heartily welcome the first instalment of it as a most important addition to the chemical literature of the country. The book is so well and clearly written, and is evidently the result of so much honest labour and careful thought, that it is very difficult to discover any flaw at all in it. If we were called upon to point out what we considered defects, we could cite

nothing but a slight vagueness occurring occasionally in the description of reactions, or of manufacturing processes. The book also is scarcely suited for the beginner; it plunges too precipitately into the arcana of the science. But the system of the book is so truly scientific, the style so attractive, and the advocacy of the new notation so earnest and consistent, that the most practised chemists cannot fail to be interested in its perusal.

The method of describing the elements, according to their relations to the typical element, hydrogen, is now applied to general chemistry by Dr. Odling. Professor Kekulé had already shown how admirably it was calculated to throw light upon the constitution of many organic bodies, hitherto regarded as anomalous. Professor Odling has found suitable places for many compounds generally viewed as of uncertain formulae or doubtful relationship. For example, some of these are now viewed as aldehydes, and as bearing the same relations to their respective oxacids as common aldehyde bears to acetic acid. They are for the most part known in the form of chlorine substitution-products, and although these have been in some cases obtained from the original aldehydes by replacement of chlorine for hydrogen, yet they are usually formed by indirect processes. When the chlor-aldehydes react with water, a characteristic result follows. They exchange Cl for the compound radicle, HO, so as to form HCl and the normal oxacids to which they correspond. Among such aldehydes the following bodies occur:—

Sulphurous chlor-aldehyde,	Cl ₂ SO
(Bichloride of thionyle).	
Sulphuric chlor-aldehyde,	Cl ₂ SO ₂
(Bichloride of sulphuryle).	
Phosphorous chlor-aldehyde,	Cl ₂ P
(Terchloride of phosphorus).	
Phosphoric chlor-aldehyde,	Cl ₂ PO
(Oxychloride of phosphorus).	
Sulpho-phosphoric chlor-aldehyde,	Cl ₂ PS
(Sulpho-chloride of phosphorus).	

The radicles thionyl and sulphuryle appear again in the bodies known as thionamide and sulphamide;—

Thionyle-diamine	(SO)"H ₂ N ₂
Sulphuryle-diamine	(SO ₂)"H ₂ N ₂

The curious substance discovered by Williamson, namely, chlorhydrosulphurous acid, H Cl SO₂, is viewed as intermediate between the corresponding chloraldehyde and the biatomic acid, the sulphurous.

Nitric chloraldehyde is ClNO₂, and may of course be viewed also as chloride of nitryle. Nitrous chloraldehyde is ClNO, and also takes its place among the other azotyle compounds in the following series:—

Chloride of azotyle,	(NO) Cl
(Chloro-nitrous gas).	
Bichloride of azotyle,	(NO) Cl ₂
(Chloro-nitric gas).	
Sulphate of azotyle,	(NO)HSO ₄
Nitrous acid,	(NO)HO
Nitrous anhydride,	(NO) ₂ O

We find a judicious view of compound radicles which the author describes as not necessarily existent in bodies, but as molecular groupings capable of being transferred from one body to another. By accepting this modified doctrine we shall still be able to regard bodies as related to one another in constitution, when they are known to be related to one another in derivation and behaviour.

By an enlargement of the ordinary idea of double decomposition, the author is enabled to refer most chemical changes to that symmetrical mode of action. Such changes are the direct union of two elements—the breaking up a compound into its elements—and the liberation of a single element in the free state.

The author illustrates the relation of seriated heterology

THE CHEMICAL NEWS.

VOL. IV. No. 101.—November 9, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Presence of Vanadium in an Aluminous Mineral from the South of France, by M. H. SAINTE-CLAIRE DEVILLE.

IN June, 1858, M. Meissonnier, engineer at the mines at Marseilles, sent me some specimens of an iron mineral which was very difficult to treat on account of the infusibility of its slag. This substance, obtained from Baux, near Arles, consisted of small round grains buried in a perfectly crystallised pure limestone.

The analysis which I made of it, after having separated the limestone with weak hydrochloric acid, gave me the following results:—

Alumina	55.4
Oxide of iron, carbonate of lime, and water	44.6
	100.0

I then recognised it as the mineral to which M. Berthier gave the name of Bauxite. It is a hydrate of alumina which M. Dufrenoy has ranked with gibbsite, or rather diasporé. The analysis of M. Berthier, performed on a mineral from Baux, gave him the following figures:—

Alumina	52.0
Oxide of iron	27.6
Water	20.4
Chromium	traces
	100.0

It is very true that fused alkalis become coloured yellow when heated with bauxite, but we shall see a little further on that the colouring matter in this case is vanadium, and not chromium.

From other specimens of this curious substance which M. Le Chatelier found in the Departments of Gard and Var, where it is very common, we found that we had met with the richest mineral of alumina that had yet been discovered. Indeed, some specimens contained as much as from 60 to 65 per cent. of alumina.

The plan which I at first adopted to dissolve this mineral is very simple. It has become, in consequence of the labours which M. Le Chatelier, M. Jacquernart, M. P. Morin, and I have undertaken to extract the alumina and prepare aluminium from it, a complete system of manufacturing industry. I may say that in the factory of Salyndres, where a manufactory of aluminium is now established, this process is daily employed to produce large quantities of aluminate of soda, soluble alumina, and aluminium.

I prepare a concentrated solution (4.5°) of pure caustic soda, which I introduce into a silver crucible. To this I add a weighed quantity of the aluminous mineral previously freed from chalk by digestion with weak

hydrochloric acid in washing, and I slowly evaporate it to the consistency of a thick syrup. It may, however, without inconvenience be evaporated to dryness. This may be useful in a quantitative experiment.

Upon adding water aluminate of water is dissolved, and a silico-aluminate of soda is left with oxide of iron, insoluble in water but soluble in acids. I will examine these two substances successively.

A. Silico-Aluminate of Soda and Oxide of Iron.—When the solid pulverulent matter, insoluble in water, is taken up by nitric acid, it almost entirely disappears, leaving a few grains of considerable hardness.

Corundum.—These grains, which resist the action of acids, dissolve in bisulphate of potash. They consist chiefly of alumina, with traces of iron and titanium. They are corundum.

Silica.—Upon evaporating to dryness the acid liquid and taking up the residue with water containing a little nitric acid, silica is left behind, which may be washed, ignited, and weighed. It soon disappears in weak hydrofluoric acid.

Titanium.—But this silica is not pure, for upon digesting it with sulphuric acid containing 50 per cent. of anhydrous acid, and evaporating until it begins to fume, a soluble substance is extracted from it which is separated by evaporating to dryness and ignition in a platinum crucible. This slightly yellow, flaky substance is then insoluble in the acid. Heated to redness with carbonate of potash, it is transformed into a brilliant saline body, the crystals of which separate from the fused mass at the moment of solidification. On treating the fused mass with water, a titanate of potash is left insoluble in water, but easily dissolved in hydrochloric acid. A portion of this solution treated with a slip of zinc yields a violet blue liquid, in which a dark blue precipitate soon forms, which is gradually whitened by the oxygen in the air. The hydrochloric solution treated by ammonia gives a white precipitate, which, when heated in the reducing flame with a globule of tin, produces a very dark violet blue colour, almost black. It is, therefore, titanous acid.

Soda.—The acid liquid, separated from the silica and titanium, is evaporated to dryness, and heated on a sandbath to about 200°, and then taken up by water, slightly ammoniacal. A nitrate is dissolved out, from which the ammonia is driven off by a gentle heat and the nitric acid by evaporation with excess of hydrochloric acid in a porcelain dish. The solution upon evaporation leaves well-defined cubes of chloride of sodium.

Iron.—The ammoniacal liquid leaves a considerable residue of a strong yellow colour, which dissolves entirely in hydrochloric acid. The iron may be easily separated from it by any well known process.

Alumina.—After the iron, the presence of alumina may be easily shown by methods which need not be detailed here.

Upon making a quantitative analysis by the above

process, and separating the iron and alumina by means of gaseous hydrochloric acid; I found the following results. The mineral was attacked by potash:—

	Oxygen.
Silica	41.9
Titanic acid	9.0
Alumina	25.6
Potash	23.5
	100.0

Excess, 1 per cent.

It is seen that this is the proportion of oxygen in Labrador feldspar. Only two or three equivalents of water must be added, which make it a sort of amorphous zeolite. The water is very difficult to estimate, for this body, like zeolite, loses water in a dry atmosphere and absorbs it again in a moist one, so that its normal composition is on this account very difficult to give accurately.

B. Substances Dissolved in the Soda.—When bauxite is treated with hot concentrated alkali, and when the insoluble matters which I have just mentioned are separated, the solution contains only alumina, which may be separated from the soda by any well known process.

C. Aluminate of Soda.—This solution has a constant composition when prepared with an excess of bauxite; but a definite compound can be obtained with greater certainty by attacking the aluminous mineral in excess with carbonate of soda at a high temperature, as is done every day at Salyndres, and by prolonging the operation until the calcined mixture no longer effervesces with acid. There is thus obtained a neutral solution, taking into consideration the saturating capacity of alumina, and the composition of which is given by the following analysis, which was performed on a specimen obtained with caustic soda and alumina in excess. This solution, treated with hydrochloric acid, evaporated to dryness, gave marine salt, which was weighed, and insoluble alumina, which was ignited and weighed. I found the following proportions of soda and alumina:—

Alumina	53.9	2 Al_2O_3	52.9
Soda	46.1	3 NaO	47.1
	100.0		100.0

which seem to be invariable in all the experiments since made on specimens prepared with carbonate of soda.

However, when the alumina-soda lyes are very concentrated, they deposit a very dense hydrate of alumina, from which the soda cannot be entirely removed by washing. The analysis quoted above was made with a liquid containing about $\frac{1}{4}$ th of its weight in alumina, and upon lyes marking 10° on the hydrometer. In all cases the aluminate $2 \text{Al}_2\text{O}_3, 3 \text{NaO}$ appears to be the most stable of all alkaline solutions saturated with alumina. The others may be considered as super-saturated when they deposit spontaneously.

Vanadium.—Some aluminate of soda which I kept for a long time in a well stoppered flask, deposited at the end of some months octohedral crystals of the greatest regularity, which might be taken for alum. These crystals, of which I had only very few, were at first looked upon as an aluminate of soda corresponding to M. Frémy's aluminate of potash, which also crystallises in the regular system, but they lose 48.7 per cent. of their weight by heat. Treated with hydrochloric acid, they disengage chlorine and become of a deep red colour.

This characteristic showed me for a certainty the presence of vanadium. Indeed, before the blowpipe they give with phosphorus salt, and in the reducing flame the beautiful green tint of vanadium, becoming yellow in the oxidising flame and orange yellow in the presence of nitre.

Finally, when dissolved in water and treated with sulphide of ammonium, they give the characteristic colour of alkaline sulpho-vanadates. These crystals, although they show in so marked a manner all the reactions of vanadium, do not really contain much; they are almost entirely composed of tribasic phosphate of soda, corresponding to the salt already known, and which, according to Mr. Graham, contains 56 per cent. of water and three equivalents of soda for one equivalent of ordinary phosphoric acid. According to this hypothesis, which I have not verified exactly, these crystals would be composed of,—

Neutral phosphate of soda	95.4
Neutral vanadate of soda	4.6
	100.0

The vanadic acid was estimated by dissolving the salt in a little water, placing a piece of sal-ammoniac in the solution, washing the precipitated vanadate of ammonia with sal-ammoniac dissolved in water, igniting and fusing the vanadic acid before weighing it.

The other elements of the salt were estimated in the following manner:—In the neutral solution from which the vanadate of ammonia has been separated a little ammoniacal nitrate of magnesia is poured. The precipitate of phosphate of magnesia and ammonia is filtered off at the end of some days, calcined and weighed. The liquid containing the nitrate and chloride of ammonium, with the nitrates of magnesia and soda, is poured into a flask and boiled down with an excess of nitric acid, which burns the ammonia and drives off the chlorine, so that there only remains soda, magnesia, and the excess of nitric acid. The latter is driven off by evaporating to dryness in a platinum dish. Re-dissolve in a small portion of water, and place in the liquid an excess of crystallised oxalic acid. Evaporate again. During this operation the greater part of the nitric acid is driven off by the oxalic acid. Moisten the residue with a concentrated solution of pure tartaric acid, containing only a few centigrammes of acid, dry and calcine, when there is obtained a mixture of magnesia and alkaline carbonates, which are separated by water. The alkalies are estimated in the ordinary manner.

This analysis, performed upon very small quantities of material, and by a method of estimating the phosphoric acid in which I have no great confidence, gave me evidently insufficient results. No simple formula can be deduced from it; it must only be regarded as an approximation. If I can procure a larger quantity of the crystals I will perform the analysis again. These are the results:—

Vanadic acid	4.4
Phosphoric acid	10.4
Soda	32.8
Potash	1.7
Water	48.7
Loss and fluorine?	2.0
	100.0

It may be remarked that, according to the observation, the phosphate of soda, 3NaOPO_3 , containing 56 per cent. of water, and which very probably enters into

the composition of this salt, is insoluble in a concentrated solution of caustic soda; so that all the phosphorus of the aluminous mineral is found in these crystals, of which the quantity is always very small. I had forgotten to say that their production may be hastened by pouring alcohol carefully on to the surface of the aluminate. Care must be taken not to mix the two liquids, as alumina containing soda will then be precipitated, which will interfere with the experiment.

2. Preparation of Vanadium.—The process which I have always adopted to extract vanadium from aluminous minerals consists in treating them with half their weight of concentrated soda lye, to which a little nitrate of soda has been added. After being uniformly mixed, the mass is dried, heated to redness in an earthen crucible, and then extracted with boiling water. The moderately-concentrated solution is then placed in a series of Woolfs bottles, through which a very rapid current of sulphuretted hydrogen is passed. A green coloration is soon developed, whilst the alumina precipitates, and then the liquid becomes of a rose violet, and finally the colour of very deep permanganate of potash. It only assumes this tint after a very prolonged current of sulphuretted hydrogen has been passed through the liquid, the excess of which passing through the first bottles begins to dissolve in the succeeding ones. Then filter and wash the alumina, which is absolutely pure (except perhaps a little phosphoric acid), very dense, and wanting the translucency which is usually possessed by gelatinous alumina as obtained by precipitation with ammonia from an acid liquid. It may be washed with great readiness. The washing water need only be collected so long as they are of a red colour. The sulpho-vanadate of soda has such a considerable tinctorial power that a few centigrammes of vanadium suffice to impart to a litre of water a deep claret colour when in the state of an alkaline sulpho-vanadate.

The red liquid is heated with hydrochloric acid until it is exactly saturated or at most very slightly acid. A brown precipitate mixed with sulphur is thus formed, which is allowed to settle at a gentle heat until the liquid is quite clear and free from sulphuretted hydrogen. The solution is then decanted and the sulphide is collected on a small, thin paper filter, and afterwards burned in an oxidising atmosphere to transform the sulphide of vanadium into fused vanadic acid. The quantity of vanadium which colours these sulphuretted liquids so strongly is really very small; for after the most delicate analyses and the most scrupulous search, I have only been able to find in aluminous minerals the following quantities:—

Mineral from Revest, near Toulon	0.0009
Mineral from Calabria	0.0006

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 230.)

Synthesis of Nitrate of Silver.—In order to determine the proportional number of silver with respect to the nitrate of this metal, I have performed the synthesis of the nitrate of silver, as has been already done by MM. Turner, Penny, and Marignac.

I have performed these syntheses in Bohemian glass vessels, and in a platinum retort. For this purpose I have made use of all the arrangements mentioned when describing the synthesis of chloride of silver, to retain

during the solution of the metal the nitrate of silver mechanically carried off by the nitrous gas; except, indeed, in one single case, where I effected the solution, the evaporation, and the fusion in a platinum retort, when it was impossible for me to pass the gases through a liquid to deprive them of the mechanically suspended solution of silver. I made use of vessels of different materials in order to examine the action which the fused nitrate had upon the glass.

The silver being dissolved, which requires from twenty-four to forty-eight hours, according to the quantity of metal employed, I incline the globe and adapt the neck to a receiver, and proceed with the complete evaporation of the solution. To effect this, I raise the flask to a temperature approaching, but always below, the boiling point of the contained liquid; this takes from forty-eight to seventy-two hours. When evaporated, I raise the temperature of the nitrate to its fusing point, and keep it there until it is of a constant weight; this sometimes is very tedious. To hasten its drying, and especially to drive away the last traces of nitric acid which the salt retains, I pass into the vessel a current of air which has been first deprived of organic matter by passing through a Bohemian glass tube heated to redness, and containing oxidized copper, and afterwards dried by means of chloride of calcium. I was obliged to pass the current of air over a long column of red-hot oxide of copper before drying it, because I found that the air of my laboratory, or even the external air simply filtered through cotton and then dried, slowly, but continuously reduces nitrate of silver heated to its fusing point, and still more quickly when fused, with elimination of nitric acid.

When the weight of the nitrate of silver was perfectly constant, having made a note of it, I again increased the temperature to fuse the salt, and kept it in a state of fusion in a current of air deprived of organic matter and water, until its weight became absolutely constant. In many experiments I have kept nearly 500 grammes of nitrate in a fused state, from eight in the morning until ten at night, without in the least diminishing its weight.

In spite of the constancy of weight of the fused nitrate, I wished to assure myself that this salt lost nothing when it was fused a second time *in vacuo*. I have thus found that the fused salt does not possess any appreciable tension of vapour, for after having five times exhausted the air, and each time replaced it by pure air, the globe containing 472.416 grammes of nitrate lost nothing in weight.

As the fused nitrate is very slightly hygrometric, a property which the crystallised salt does not appear to possess, I adjusted to the globe during its cooling a tube containing chloride of calcium, or acid pumice-stone, to prevent the access of moisture; besides which, the globe was hermetically closed, except only during the weighing.

Then the cork which closed the neck of the flask, serving at the same time to close the globe, was replaced by another cork, which was slightly grooved, so as to allow the air in the flask to be of the same density as the external air.

The nitrate obtained in every experiment in which I made use of purified air was perfectly colourless in the fused state, and of a pearly white, with a radiating fracture in the solid state; it was always neutral to test paper.

Of the eight syntheses which I give below, four were performed in globes of Bohemian glass, two in retorts of the same glass furnished with a receiver fastened to the

retort, and with a bent tube likewise fastened to the receiver to retain the silver carried off. The last two, VII. and VIII., were executed in platinum.

In No. VII. the solution of the metal was itself effected in a platinum retort. In this experiment I was unable to pass the gases resulting from the solution of the silver in the nitric acid through a liquid to retain the metal carried off. This synthesis, therefore, is necessarily faulty. In the eighth experiment the silver was dissolved in a glass vessel, and the solution with the washing waters of the apparatus were evaporated in the platinum retort. The nitrate was there dried, fused, and kept in fusion for six hours. The two operations performed in platinum have enabled me to appreciate how marked is the action of the air of the laboratory on fused nitrate of silver. I have taken great pains to prevent its decomposition by organic matter.

The identity of the result obtained in Experiment VIII. with the result furnished in Experiments I., II., III., IV., V., and VI., shows that nitrate of silver can be fused in glass without either attacking or decomposing it. Moreover, I have always directly verified at each experiment made in glass, that the weight of the globe was identically the same after as it was before the synthesis: at the most, there might be noticed an increase

of weight of three or four milligrammes, due to the silicic acid deposited on the sides and derived from the silicium in the silver.

I have stated above that the nitrate simply dried at its fusing-point, still loses weight when it is actually fused. The average loss is a twelve thousand five hundredth of its weight.

Pure crystallised nitrate of silver, dried for six months under a bell-jar with concentrated sulphuric acid, lost in an experiment tried on a tolerably large scale a four-thousandth part of its weight upon fusion. This salt, which would be considered by all chemists as anhydrous, loses, therefore, three times as much water as the salt obtained by direct synthesis and dried at its fusing-point. It appears to me probable, however, that all the loss experienced by the crystallised salt upon fusion should not be attributed to the disengagement of water; it may be that the air condensed by the small crystalline plates has something to do with it.

Although I think that it is most rational to consider the fused nitrate as a definite compound, I give in the following table the weights of the salt in both cases; besides, whichever hypothesis is taken, the results agree within extraordinarily narrow limits, and present an ensemble which does not admit of two interpretations.

Syntheses of Nitrate of Silver.

Numerical Order.	Weight of the Silver in air.	Weight of the Silver reduced to a vacuum.	Weight of the Nitrate of Silver carried off by the gases.	Total weight of the Nitrate of Silver dried at its fusing point.		Total weight of the Nitrate of Silver kept in fusion until its weight was constant.		Nitrate of Silver produced by 100 000 parts of metal.	
				A.	B.	A.	B.	A.	B.
				In air.	Reduced to a vacuum.	In air.	Reduced to a vacuum.	Calculated from the salt dried at its fusing point.	Calculated from the weight of the salt kept in fusion until its weight was constant.

First Series.

Syntheses made in Bohemian Glass.

	Grammes.	Grammes.	Grammes.	Grammes.	Grammes.	Grammes.	Grammes.	Grammes.	Grammes.
I.	77.2715	77.2684	0.0155	121.5	121.6919	121.6585	121.6749	157.492	157.474
II.	99.9965	99.9925	0.0085	157.4815	157.5025	157.4565	157.4770	157.510	157.481
III.	199.9940	199.9860	0.0080	314.9270	314.9690	314.8910	314.9330	157.485	157.477
IV.	300.0070	299.9950	0.0105	472.3665	472.4303	472.3395	472.4030	157.476	157.471
V.	300.0110	300.0000	0.0130	472.3690	472.4320	472.3480	472.4110	157.478	157.470
VI.	404.9720	404.9970	0.0000	637.6030	637.7550	637.5630	637.7130	157.471	157.463

Second Series.

Syntheses made in Platinum.

	Grammes.	Grammes.	Grammes.	Grammes.	Grammes.	Grammes.	Grammes.	Grammes.	Grammes.
VII.	200.0080	200.0000	0.0000	314.8880	314.9210	314.8590	314.9010	157.460	157.450
VIII.	200.0080	200.0000	0.0000	314.9460	314.9770	314.9080	314.9380	157.488	157.469
Mean ¹ . . .								157.4857	157.472

Synthesis of Sulphide of Silver.—Up to this time M. Dumas is the only person who has attempted to perform the synthesis of sulphide of silver.

To ascertain the relative proportions of its elements, he has directly sulphurised silver by means of pure silver, which was passed in excess in the state of vapour over the red-hot metal. The mean proportion which he obtained, was one which corroborated Prout's law. In following this method, I made two series of experiments, the first, comprising three syntheses, was effected by

leading an excess of pure sulphur, in the state of vapour, over the metal heated to dull redness in a Bohemian glass tube surrounded with magnesia to protect it from the action of the gas flame; in the second experiment I replaced the sulphur by pure and dry sulphuretted hydrogen. In each case I drove out the excess of sulphur by a current of pure dry carbonic acid.

As in a preliminary experiment I ascertained the formation sometimes of traces of sulphurous acid, sometimes of sulphuretted hydrogen, I suspected the presence of oxygen and hydrochloric acid in the carbonic acid which I prepared from hydrochloric acid and marble, passing it first through a hot solution of bicar-

¹ The result of the VII. th synthesis being inexact, for the reason mentioned in the text, I have not taken account of it in calculating the mean.

bonate of soda, and then through chloride of calcium tubes. I therefore took the precaution to pass the carbonic acid before drying it completely,—

1. Through a hot solution of bicarbonate of soda;
2. Over dry bicarbonate of soda, contained in two U-shaped tubes;
3. Through a glass tube, 90 centimètres long, heated to redness throughout, and filled in its first half with copper reduced by hydrogen, and in its second half with a mixture of oxide of copper and of copper reduced by hydrogen.

The apparatus for the disengagement of carbonic acid is terminated by two U-shaped tubes, filled with chloride of calcium, and followed by two others containing pumice stone moistened with sulphuric acid.

The suspected causes of error were found to be real; indeed, although I took the precaution to allow the carbonic acid to disengage for three hours before heating the tube containing copper and oxide of copper, I found unequivocal traces of oxidation of the metal for a length of 5 or 6 centimètres when I heated the tube to dull redness; and, moreover, I found a very appreciable quantity of subchloride of copper formed, an evident proof that the current of gas carried over with it traces of oxygen and hydrochloric acid. As I only made use of a slow current of carbonic acid, I hope that I completely eliminated the above-named causes of error; but if I had to recommence these syntheses I should replace the carbonic acid by nitrogen, which is so easily obtained pure by means of air and red hot hydrogen-reduced copper.

The silver employed in my experiment was rolled between two plates of pure silver. The sulphide of silver produced was beautifully crystallised. To be quite certain of the result, I weighed the sulphide of silver twice: the first time, after having heated it to very dull redness in a current of carbonic acid, and the second time after having heated it in the same current sufficiently high to soften the glass and fuse a portion of the sulphide of silver.

In each case the weight of the sulphide was identically the same, the weighings of the sulphide having been performed in the five experiments three times in air and twice *in vacuo*. The results agree so perfectly that it is scarcely possible to imagine a greater precision in experiments. Thus, in spite of the prejudice which I conceived against my first syntheses on account of their disagreeing with those of M. Dumas, I now have not the least doubt of the accuracy of these determinations.

These are the results of the five experiments:—

Syntheses of Sulphide of Silver.

Numerical Order.	Weight of the Silver in air.	Weight of the Silver reduced to a vacuum.	Apparent weight of the Sulphide of Silver.	Weight of the Sulphide reduced to a vacuum.	Sulphide of Silver produced from 100.000 part of this metal.
I.	grammes. 59.425	grammes. 59.4225	grammes. 68.247	grammes. 68.24823	114.854
II.	104.143	104.139	119.606	119.6078	114.853
III.	191.917	191.9094	220.412	220.4158	114.854
IV.	150.0058	150.000	(2) 172.287	172.2765	114.851
V.	249.061	249.076	(2) 286.078	286.061	114.849
			Mean . . .		114.8522

(2) The weighings of these sulphides have been made *in vacuo*.]

(To be continued.)

Notes on Nitrification, by R. HAINES, M.B., Professor of Materia Medica, Grant Medical College, Bombay.

As I have already touched upon the question of nitrification, it may not be out of place to indicate the extent to which our knowledge of this process has been advanced by the most recent researches.

Nitric acid, it is well known, exists in the atmosphere, and is brought down with rain as nitrate of ammonia, together with an excess of ammonia in the form of carbonate. From the fact that Cavendish and others obtained nitric acid by passing electric sparks through moist air, the opinion has been hastily adopted, and diffused by means of the ordinary elementary works on Chemistry, that electric discharges in the air are influential, if not principal, causes of the production of this compound.

The exact and elaborate researches of our countrymen, Lawes, Gilbert, and Way, have demonstrated the fallacy of this notion. Large quantities of rain collected in the country during every month of three years, over which the experiments extended, the rain of thunder-storms, the condensed water of fogs and mists, snow and hail, were separately examined by new and refined methods of analysis. The final result may be given in Mr. Way's own words:—

"It appears to me that all we can legitimately prove from these results is, that neither in the case of nitric acid nor ammonia is the quantity present in the air sensibly increased by such influences as electricity, heat, &c.; for, so far as the water of rain is concerned, these compounds of nitrogen are evidently inversely proportionate to the quantity of water falling. In the case of thunder-rain, the fall being slight, we have a large proportion of nitric acid, although, even then, not much greater than is found in the water of fogs and mists when the influence of heat and electricity is not supposed. Where, however, a fair quantity of rain falls, accompanied with thunder, we find only the average proportion of nitric acid."

On the other hand, that nitric acid is produced naturally by the oxidation of ammonia, without the aid of other nitrogenous matter, was proved many years ago by the French Academicians, who, by hanging up a basket of purified chalk, saturated with carbonate of potassa, for a few months, over a vessel containing putrefying bullock's blood, obtained a large quantity of nitrate of potassa.¹ By raising the temperature, this change takes place with increased rapidity. Dumas has shown that by passing moist air mixed with ammonia over chalk moistened with a solution of potassa, at a temperature of 212° F., there is found, in the course of a few days, a considerable quantity of nitrate of potassa.² Here, again, chalk appears to be a necessary agent in the transformation; the importance of chalk in both natural and artificial nitre grounds has been already alluded to. M. Boussingault has been engaged in a very extended series of experiments on the proportion of nitrates contained in natural waters of all kinds, from lakes, rivers, springs, and wells, and also in every variety of soil.³ The results are most interesting, but I cannot do more than glance at the more prominent of them. As to the soils, few were altogether destitute of saltpetre. In the poor soil of dry and exposed forest land, the proportion is at its minimum; in a few cases no

¹ Knapp's "Chem. Techn.," l. 359.

² *Ibid.*, p. 358.

³ *Comptes-Rendus* for January 26, 1857, p. 106. Reprinted in the *Chemist*, N. S. IV., for 1857, p. 497.

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nitrate could be detected. In cultivated lands, especially those in which carbonate of lime was naturally contained, or to which it had been artificially added, the nitrates were in very large proportion; in the earth of hot-houses, strongly manured, and kept at a high temperature, it was at its maximum. Argillaceous marl, though uncultivated, always contained much. In the chalk quarries at Meudon, near Paris, the upper strata contained nitrates in considerable quantity; in the lower strata they were altogether absent. With regard to natural waters, their richness in nitrates was found to be intimately connected with the nature of the rock strata through or over which they had passed; in lakes surrounded by primitive (syenitic) rocks the merest traces of nitre were found; in waters flowing from the sandy and quartzose rocks of the Vosges, scarcely stronger traces; but in rivers that rise in or flow over calcareous soils, as the Seine, the proportion was very large, on an average equivalent to 15 parts of nitrate of potassa in a million of water, but varying from 6 to 62 parts in a million. His experiments also confirmed those of other observers as to the vastly-increased quantity of nitrates in the well water of large towns. In the water of a great number of wells in Paris there was never less than the equivalent of 206 parts of nitre in a million (= 1.80 grs. in one imperial pint), rising, in particular cases to so high as 1000, 1500, 2000, and in the highest 2165 in a million, equal to about 18.94 grains in an imperial pint,—nearly approaching the largest quantity found in the wells at Nassik. These estimations were performed by the direct method, or rather by two direct methods, which were always employed comparatively,—namely, the decoloration of indigo, and that of Pelouze, by a proto-salt of iron and permanganate of potassa. (In Mr. Way's experiments, the proportion of nitrates being exceedingly minute, the process was different; the water was evaporated to dryness, the residue re-dissolved, filtered, and re-dried for several times in succession, whereby all the soluble organic matter became insoluble; the pure salts were then heated in a combustion-tube with oxide of copper, and the nitrogen estimated by Dumas's direct method; from this the equivalent quantity of nitrate of potassa was obtained by calculation.) In most of the above cases, the occurrence of nitric acid is explicable, and in all probability, rightly accounted for in the way I have already stated: the ammonia present in the air is brought in contact with oxygen highly condensed, perhaps in a liquid state, in the interstices of porous substances, as chalk, and oxidised to nitric acid and water; thus, $\text{NH}_3 + 8\text{O} = 3\text{HO} + \text{NO}_3$ —the excess of ammonia yielding the base, whose affinity for the nitric acid predisposes to the union; the nitrate of ammonia so formed slowly decomposes with the chalk into nitrate of lime and carbonate of ammonia, which latter salt, having a basic reaction, determines, as before, the formation of nitric acid; and so on, *ad infinitum*. Such is the plausible theory of Kuhlmann; and no better can be found.

But this theory leaves unexplained the occurrence of nitric acid in the atmosphere; ammonia there is truly, but neither chalk nor any other porous or gas-condensing material.

The action of electric discharges has been shown to be productive of no appreciable effect whatever. All that appears thus far is, that ammonia must be pre-existent; all nitrogenous compounds (nitric acid included) can, by simple means, be converted into ammonia; therefore, nitrogen must already be in some previous state of combination, otherwise nitric acid will not be naturally

formed. It is necessary to say "naturally," because we can form ammonia, and, therefore, nitric acid, from pure nitrogen, by the use of proper materials, aided by a very high temperature, under conditions, that is, which do not occur in nature.

But a very great step in advance has now been made. Nitric acid has been obtained at ordinary temperatures without electricity, by the direct union of nitrogen with oxygen. On November 26, 1855, M. Cloëtz communicated to the Academy of Sciences of Paris a Memoir, containing the results of a series of experiments, of which the following is an abstract:—Atmospheric air, after having been perfectly purified from every trace both of ammoniacal and acid vapours, by passing through tubes containing alkaline carbonates and sulphuric acid, was slowly drawn through a series of flasks containing various substances of an alkaline nature, and more or less porous. The experiment lasted about four months, in the autumn and early in spring, the mean temperature being probably below the average of the year, or 51°F ., so that it was little favoured by a high temperature.

The first four flasks contained fragments of recently-calced brick; in the first, impregnated with solution of pure carbonate of potassa; in the second, with precipitated carbonate of lime; in the third, with precipitated carbonate of magnesia; in the fourth, with both precipitated carbonate of lime and carbonate of potassa. "The next four flasks contained, instead of brick, fragments of pumice, first calcined with sulphuric acid, afterwards washed with distilled water, then again heated to redness without the addition of acid. Each of these flasks contained, like the first four, and in the same order, alkaline or earthy carbonates, alone or mixed."

The ninth flask was filled with broken, porous bones, calcined at a white heat, and impregnated with a solution of pure carbonate of potassa. The tenth flask contained calcareous marl.

After now passing through a column of pumice impregnated with sulphuric acid, in order to remove any ammoniacal vapours which the air might have acquired from the last flask, the current passed successively into four flasks filled with uncalcined pumice, mixed with the same matters as had been added to the brick and calcined pumice of the first flasks.

The next four flasks contained,—the first chalk, slightly moistened; the second, calcareous marl, with alkaline carbonate; the third, an intimate mixture, in the form of bullets, of decanted kaolin and precipitated carbonate of lime; and the fourth, argillaceous earth, taken from one metre below the surface. Of these eighteen flasks, nitrates were found after the experiment in considerable quantity in all those containing the brick, calcined pumice, and ordinary pumice. In the chalk, the pure marl and the marl containing alkaline carbonate, and in the mixture of kaolin and carbonate of lime, traces only of nitrates were found; in the calcined bones, and in the argillaceous earth, none whatever.

This experiment conclusively shows that, without the presence of any acid or ammonia, pure oxygen and nitrogen may combine directly to form nitric acid by contact with porous substances, containing alkaline or earthy matters.

(To be continued.)

On a New Method for the Production of Chlorine, by M. LAURENS.

THE process proposed by M. Laurens for the preparation of chlorine consists in decomposing chloride of copper by heat. Here is a *résumé* of the operation:—First, prepare

by the following series, among others : we cite those not containing carbon :—

H_2SO_4	(wanting)
$H_2S_2O_4$	Dithionic acid
$H_2S_3O_6$	Trithionic acid
$H_2S_4O_{10}$	Tetrathionic acid
$H_2S_5O_{14}$	Pentathionic acid
HCl	Hydrochloric acid
HClO	Hypochlorous acid
HClO ₂	Chlorous acid
HClO ₃	Chloric acid
HClO ₄	Perchloric acid.

Professor Odling found that it is possible to pass from the first to the second member of this series by a process of direct oxidation.

NOTICES OF PATENTS.

486. *Distillation of Bituminous Substances.* J. YOUNG, Limefield, Edinburgh. Dated February 25, 1861.

THIS invention has reference to improvements in apparatus for distilling the Boghead minerals, bituminous coals, and other bituminous substances, and consists in the arrangement of a series of vessels constructed for this purpose, of stoneware, brick, or other suitable material, communicating with each other, and placed side by side, or in a circle, as may be most convenient. These vessels having been filled with the bituminous substance to be treated, are connected with a steam-boiler, and a jet of steam passed through the whole in succession, at a sufficiently high pressure and temperature for ensuring the proper distillation of the coal. The oils generated during the process are drawn off at intervals from the stoneware vessels, and when they have accumulated in sufficient amount and the charge in the first vessel is exhausted, this is disconnected and the jet of steam brought to bear directly on the second, while another newly charged with coal is made the last in the series. The process is, therefore, a continuous one, and the gaseous products of the distillation are allowed to pass off from that vessel which at the time happens to furnish the last outlet.

The improved arrangement of apparatus here described is calculated to facilitate the manufacture of mineral oils of the paraffin class, for which Mr. Young has long been celebrated.

488. *Oils.* C. CORROY-REGNAULT, Margaret Street, Cavendish Square, London. Dated February 25, 1861. (Not proceeded with.)

FOR the purification and refining of such oils as are usually manufactured by processes of distillation, the inventor treats the crude material with one of the following substances, employed in the state of powder,—viz., charcoal, brick-earth, clay, talc, steatite, chalk, slate, or coal-ashes. After being intimately incorporated with any of these, the impure oil or resinous substance is submitted to distillation, when the glutinous, gummy, or otherwise prejudicial matters are retained in the retort, and a partially-refined product passes over. It is proposed further to subject this to a second process of distillation from powdered quick lime, caustic soda, and generally small proportions also of chloride of lime, and one of the class of materials employed in the first process. A third treatment, similar in nature to the second, may sometimes be resorted to with advantage. The process is applicable likewise to the clarifying, decolorisation, and disinfection of oils and fats already manufactured.

The system of treatment here defined is undoubtedly capable of effecting great improvement in the quality of many crude oily products, as, for instance, those derived

from Irish peat and the inferior descriptions of mineral oil. It must be borne in mind, however, that processes involving the employment of large quantities of bulky, porous materials, and repetition of treatment, must necessitate great loss in the successive stages of the process of purification.

513. *Glue or Composition suitable for Covering the Caulking of Ships, &c.* W. J. HAY, Southsea, Hampshire. Dated February 28, 1861.

THE present is an improvement upon a former invention, for which letters patent were granted to Mr. Hay on February 1, 1858. The object now accomplished is the preparation of a material more elastic than heretofore, and devoid of the unpleasant odour usually attaching to these compositions. Instead of crude naphtha and rough creosote as solvents for the asphalt and caoutchouc, the inventor prefers to employ resin oil, and he makes the new composition of the following ingredients melted together :—The several varieties of natural bitumen or asphalt, vegetable pitch or tar, and resin oil are used with or without caoutchouc. The patentee claims, also, the employment of oil of turpentine, wood naphtha, or any spirit or essential oil.

The material here referred to is evidently a description of "marine glue," the manufacture of which, according to the original specification of Mr. Jeffery, must be seriously affected by the high price which shellac has commanded for some time past in the drug market.

514. *Improvements in the Treatment of Certain Ores containing Metals, and in Obtaining Products Therefrom.* R. LAING, Ince, near Wigan. Dated February 28, 1861.

THIS invention relates to the economical use and application of the gases which are allowed to escape from the leaden chambers in the ordinary process of manufacturing sulphuric acid. It is proposed to employ these waste gases for the purpose of rendering soluble the copper and other metals contained in the roasted pyrites left as a residual product after the extraction and conversion of its sulphur into sulphuric acid in the chambers. By washing with water, the soluble metallic compounds may then be recovered.

This proceeding is likely to meet with advantageous adoption in those vitriol works where iron pyrites is employed as the source of sulphur.

537. *An Ointment for the Cure of Sores.* C. STEVENS, Charing Cross, London. A communication. Dated March 2, 1861.

THIS ointment is composed of the following ingredients :—Litharge, one lb.; strong vinegar and olive oil, of each about a pint. These materials are first well incorporated, and then boiled till they form a thickish paste. It is recommended to apply the ointment by spreading on a surface of bibulous paper.

The patent remedy thus procured would be an intimate mixture of Goulard-water, glycerine, and common lead-plaster.

529. *Improvements in Distilling and Rectifying, and in Apparatus employed therein.* M. HENRY, Fleet Street, London. A communication. Dated March 1, 1861.

ACCORDING to this invention, the liquids to be distilled are fed continuously into a vessel or chamber in which they undergo distillation, and whence they run out as soon as they have parted with their volatile portions, the requisite degree of heat being obtained by an arrangement of steam pipes coiled within the still, and an outer steam jacket or casing. The inventor prefers the use of shallow vessels exposing large surfaces of liquid, by which the rapidity of distillation is promoted. A number of vessels contrived as above may be arranged at different

levels, and the liquid and steam caused to circulate from one to another, so that distillation may be carried on at various temperatures, and products of different degrees of volatility obtained. A thermometer is appended to each vessel in the series to indicate the temperature.

The arrangement of distilling apparatus here described is extremely ingenious. A further improvement would consist in passing the exit steam pipe through a supplementary tank or vessel wherein the liquid might be slightly warmed before actually entering the still.

554. *Glass.* T. PETITJEAN, Brydges Street, Covent Garden, London. Dated March 5, 1861.

AMONGST other claims for improvements of a mechanical nature in the apparatus employed for glass-blowing, the inventor enumerates the use of charcoal as a fuel especially adapted to these purposes, and, moreover, generally applicable to the manufacture of glass.

The bulky nature and quick-burning properties of ordinary wood charcoal are difficult to reconcile with the statement of advantages referred to in this specification.

Grants of Provisional Protection for Six Months.

1927. George Fowler Jones and James Jones, York, "Improvements in apparatus for protecting and arranging water-pipes, and withdrawing water therefrom, and preventing injury thereto by frost."

1935. William Edward Newton, Chancery Lane, London, "An improved process for producing colouring matters or pigments from manganese."—A communication from Paul Morin, Rue St. Sebastien, Paris.—Petitions recorded August 3, 1861.

2146. James Duncan, Greenock, Renfrewshire, N.B., "Improvements in the manufacture of sugar, and in the apparatus employed therein, also in the apparatus employed in re-burning animal charcoal."—Petition recorded August 28, 1861.

2176. Edward Joseph Hughes, Chancery Lane, London, "An improved apparatus for collecting the gases which escape from furnaces."—A communication from Mr. Emil Langen, Sieburg, Prussia.

2178. William Armand Gilbee, South Street, Finsbury, London, "Improvements in the process and apparatus for the manufacture of steel."—A communication from Jules Paulis, Brussels.

2233. Edward Harrison and Thomas Shuttleworth Yates, Oldham, Lancashire, "A certain compound or certain compounds to be used as a substitute for gunpowder."

2238. Nicholas Doran Proby Maillard, Dublin, "Improvements in the material and preparation of the material and apparatus for making potash, pearlash, and caustic potash of commerce."

2377. Joseph Jacob, Golden Square, London, "Improvements in the mode of and apparatus for obtaining and treating hydrogen gas, and the application thereof to various purposes, parts of which improvements are applicable to the manufacture of iron and steel."—A communication from Carl Preisenhammer and Carl Weniger, Zöptau, Austria.—Petition recorded September 23, 1861.

2441. Pierre Alexis Francis Bobœuf, Paris, "The preparation and application of certain new hemostatic and antiseptic agents."—Petitions recorded September 30, 1861.

2477. Charles Husson, Nantes, France, "An improved process for silvering looking-glass in piles of several sheets superposed without interruption, which process consists in the use of a chemical powder."

Notices to Proceed.

1457. Henry Du Mont, Rue du Rivoli, France, "A

photographic apparatus having for object to reproduce the successive phases and shiftings of a motion."

1580. John Fisher Williams, Queen Square, St. George-the-Martyr, London, "Improvements in compounds of india-rubber and gutta-percha with other substances."—Petitions recorded June 19, 1861.

1638. Samuel Alexander Bell, Epping Villas, Stratford, Essex, "Improved machinery or apparatus for arranging and securing splints, tapers, or matches in frames preparatory to dipping."—Petition recorded June 26, 1861.

2100. Louis Marino Casella, Hatton Garden, London, "Improvements in mercurial thermometers."—Petition recorded August 22, 1861.

CORRESPONDENCE.

Deodorisation of Sewage.

To the Editor of the CHEMICAL NEWS.

SIR,—I have read with great interest the information which from time to time you have given to your readers on the highly important subject of the deodorisation and utilisation of sewage. In a late number you report the successful application of carbolic acid as a disinfectant.

Up to the present time I believe that lime is the substance that has been most extensively used for the disinfection of sewage, but I presume it is found of little use except in very great excess.

I wish to inform you of a fact which has come repeatedly under my observation, which seems to prove to me that lime is not suitable for the disinfection of sewage. One part of my business has to do with the melting down of large quantities of animal fats, which is done by means of steam and sulphuric acid.

Necessarily there is a large quantity of refuse left behind when the fatty matter is separated. Now, to utilise this refuse and also to make it into a solid artificial manure by drying, I found it necessary to neutralise all the free SO_4H , which I did by adding a small quantity of lime. From the fat being melted with the least possible quantity of SO_4H there is very little free acid. Now, whenever the whole of the acid is neutralised and lime is in excess, then the mass begins to stink, and gas begins to come off. I should say that this occurs in about twenty-four hours, when the mass has been slightly heated by the steam-boiler flue passing round the iron pan or boiler in which it is placed.—I am, &c.

Manchester.

TECHNOLOGIST.

On the Estimation of Alcohol in Wine.

To the Editor of the CHEMICAL NEWS.

SIR,—In reference to a letter which appeared in the last Number of the CHEMICAL NEWS, I trust I have quite as keen a sense of honour and propriety as "Suum Cuique" in acknowledging the rights and privileges of others, and beg to assure that gentleman that he is greatly mistaken in supposing that I took the initiative for those experiments from another person, never having seen Mulder's work by Dr. Bence Jones, nor had I any recollection of the extract and discussion appearing two years ago in the CHEMICAL NEWS: therefore, if my experiments tally with Mulder's, it was the result of circumstances, and from close personal observation, that I derived that knowledge, having been requested by a wine-merchant, who complained of the discrepancies and irregularities of the Excise charges for duty, to institute a series of experiments, in order that he may be the better prepared in future to seek redress from the proper authorities—the same quality of wine being differently charged at two different ports. I commenced, of course, by distillation; and with the view of knowing how far to carry that process, so as to ensure

the entire elimination of the alcohol, I attached a graduated scale to the retort, and noted the precise moment when oiliness ceased to form in the tube as stated. Observing that it was just at half the bulk of the wine used, I had no difficulty in getting at the specific gravity and strength of the alcohol either as gain or loss, since, if the one was ascertained, the other, i. e. the extractive matter, was also known, and *vice versa*; and, to shorten the process, preference was given to its estimation as loss.—I am, &c.

JOHN HORLEY.

Nitrates in Water.

To the Editor of the CHEMICAL NEWS.

SIR,—I see that in your last Number you published the results of my analysis of a water from Tewkesbury, which contained a very extraordinary proportion of the nitrates of lime and potassa. I was so astonished at meeting with such a large quantity of those salts, particularly the latter, that I felt desirous of learning whether it was really a natural water. I learned that the well was situated on the site of a demolished inn, and was very near where the stables had formerly existed, that the water had been of fair quality for a long time, but all at once had changed its character; it is therefore most probable that some accidental waterfall had caused the oxidized products of the former stable to overflow into the well.

I am, &c.

WILLIAM HERAPATH, F.C.S., &c.,
Professor of Chemistry, Bristol, Old Park.

Bleaching Wood.

To the Editor of the CHEMICAL NEWS.

SIR,—I have some wood which, naturally nearly white, has, through neglect in drying, become brown and otherwise stained. The sap not having been evaporated, has filled the pores of the wood with coloured matter. I have tried various chemical agents to remove this colouring matter, but in vain, some of them make the matter worse.

Can any of your readers suggest any substance that may possibly bleach this wood so as to make it nearly white, or if not, tell me of any good work on bleaching which would be likely to give me the desired information?—I am, &c.

W. W. C.

Relations between the Elementary Equivalents.

To the Editor of the CHEMICAL NEWS.

SIR,—On comparing the equivalents of Cs, Rb and K, it will be found that they form one of those remarkable proportional triads of which K, Na and Li, I. Br and Cl. are familiar instances.

$$\frac{\text{eq. Cs} + \text{eq. K}}{2} = \text{eq. Rb nearly.}$$

$$\frac{128.4 + 39.3}{2} = 83.85.$$

It is an interesting fact that there should exist two of these proportional triads amongst five elements whose properties so much resemble one another.—I am, &c.

CHARLES W. QUIN.

Derivation of Names of Elements.

To the Editor of the CHEMICAL NEWS.

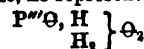
SIR,—I cannot trace the origin or meaning of the words "iron," "ferrum," or "zinc;" can any of your readers help me? Is "manganese" a mere transposition and repetition of the word "magnesia," with the compounds of which those of manganese were confounded till 1740? The signification of the rest of the names of the common elements seems tolerably evident.—I am, &c.

A TUTOR.

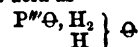
Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

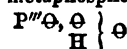
Constitution of the Acids of Phosphorus.—M. Lieben speculates (*Annales de Chimie et de Physique*, t. lxxiii., p. 92) that, setting pyro-phosphoric acid out of the question for the present, all the other acids of phosphorus may be considered as derivatives of a triatomic radical phosphoryle PΘ. Thus, he represents phosphorous acid as



and hypophosphorous acid as



These formulæ, he says, account for the fact that both of the above acids contain 3H, two of which in phosphorus and one in hypophosphorous acid are replaceable by metals. The theory of Berzelius, which represented the former as 3HO, PO₂, and the latter as 3HÖPO, did not satisfactorily explain this fact. In metaphosphoric acid, the author considers that the 2H which in hypophosphorous acid are combined with PΘ are replaced by Θ. Thus, the formula for metaphosphoric acid will be



M. Lieben believes that pyrophosphoric acid is tetrabasic, and thinks that until experiment has revealed more of the nature of this acid, it will be useless to propose a rational formula.

Another Chrome-Green.—Bichromate of potash heated with phosphate of ammonia forms, according to M. Guignet (*Bulletin de la Société Chimique de Paris*, No. 4, p. 76), an insoluble metaphosphate of chromium, which is not affected by a red-heat. A slight excess of the phosphate of ammonia must be employed. The same phosphate is obtained when the bichromate of potash is calcined with acid phosphate of lime. M. Guignet doubts its usefulness as a pigment, the colour not being sufficiently intense.

II. ORGANIC CHEMISTRY.

Synthetic Formation of a Saccharine Substance.

—M. Boutlerow found (*Bulletin de la Société*, No. v. p. 84) that when dioxymethylene is boiled with an excess of lime or baryta water the mixture, which at first is colourless, quickly becomes yellow, and soon passes to an intense brownish-yellow. The odour of dioxymethylene disappears at the same time, and a smell like that of burnt sugar is produced. No gas is evolved during the reaction. When the lime water is added slowly, the mixture being kept near the boiling point, and stopping when the colouration takes place, a neutral solution is obtained, in which carbonic acid causes no precipitate. Evaporated at first on a water bath, and finishing under an air-pump receiver, a thick, yellowish, syrupy substance is obtained mixed with some crystals. Treated with absolute alcohol, the uncrystallizable substance is removed, and a white crystalline powder is left. This the author found to be formate of lime. The alcoholic solution evaporated under the air-pump left an extract smelling like syrup prepared with burnt sugar, and tasting like liquorice juice, but acid to litmus. When heated on platinum foil it behaved like saccharine substances, but left a calcareous ash. It reduced the cupro-potassic tartrate even in the cold, and instantly when heated. Like the sugar of mannite, it had no rotatory power. The solution did not appear to ferment with yeast, but as the experiment was made with a very minute quantity of the substance, the author does not consider it decisive. Boutlerow made two analyses of the substance, but the results did not agree well. He considers, however, that in its composition the new substance approaches the bodies which mannite and some of its congeners furnish in losing one molecule of water, and

therefore he names it provisionally *methylenitane*. As the first example of the formation of a substance having many of the characteristics of a true sugar, by means of the more simple compounds of organic chemistry, the production of methylenitane, says the author, is a remarkable fact. Further researches will no doubt soon enlighten us more on the subject, so we leave for the present the author's speculations on the formation of the body, and the analogies with other reactions.

MISCELLANEOUS.

SCIENTIFIC NOMENCLATURE.

The following is extracted from a letter addressed by "C. W. M." to the *Athenæum*:—

Had Milton lived in our days, and studied the "ologies," I think it would have been to them, and not to the clannish names of the North, that he would have gone for illustrations in his Sonnet on the book called "Tetrachordon." Surely, that hard names are the curse of modern science, must be admitted alike by the ignorant and the learned.

Science is not for the idler. No man who has the industry and ambition to become a scholar will grudge reasonable pains bestowed on the language and classification of his subject. Much force, however, lies in the word "reasonable." In many branches of study, but especially in the sciences of observation, the demands made upon the student exceed, as I think, all reason. Mineralogy, organic chemistry, zoology, and physiology are all overdone with technical words, while botany (the ladies' science) is the very abyss of hard names. This might be tolerated if the names were final. But no systematic nomenclature, which is to conform to theory, ever can be final; for the theory itself must change with advancing knowledge. It seems to me that this common character of all theories has been much lost sight of by the inventors of names. Hence it is that to make ourselves acquainted with the results obtained by previous observers, we have to master in each science, not one set of names, but many systems. In botany, it would appear that no one thought he had done useful work unless he had added something to the already overwhelming mass.

In chemistry, the researches of previous years have led to the substitution of name for name, and symbol for symbol, one after another, so many deep that one does not know the "right" appellation of wood-naphtha, or how to ask for a liquid which used to be sold as "spirits of salts," and was *sometime* (as the epitaphs say) muriatic acid, hydrochloric acid, chlorhydric acid, and lately, according to a chemical reformer, hydra-chlora. Sulphates are called thionates by the organic chemists, who run up the Greek numerals until they get to *pentathionates*, and I certainly thought that the acme of descriptive nomenclature had been reached in "methylethylamylophenylammonium," until I found a recent author proposing to substitute for it a word three syllables longer by way of simplification: I forget exactly how he spelt it.

Is systematic nomenclature a necessity of systematic investigation? I think not. But as the consent of authorities seems to be, in practice, at least, against me, I can only venture to propound the question for discussion, and to state reasons for my own opinion. The present opportunity is a particularly favourable juncture for this discussion, because most of the sciences of observation show symptoms of a change in fundamental ideas.

The applications of mathematical physics to chemistry—of chemistry to the functions of animal and vegetable life—the use everywhere of improved optical methods, among which I may mention spectral analysis and examination by coloured or polarised light,—and, more than all the increasing tendency to view Nature as

one whole, and the increasing power to perceive universality in her laws—are threatening a complete and immediate disruption of all our existing theories.

And what is a theory? Our lexicon tells us it is a way of seeing—a mode of viewing. No mathematician, no accurate reasoner, ever thinks of a theory as being true, even potentially. It is a mode of viewing the truth, changeable with our point of view, and falling short of the truth by just so much as our limited reasoning falls short of the infinite variety and complexity of the universe, which we can no more grasp in its material than in its abstract form. The finite cannot comprehend the infinite.

These observations are trite; but it is just for the want of noting them that we are encumbered with the flood of names. These have been invented, and grouped, and changed, and re-grouped, and systematized, by each new theorist, as if his theory not only embraced the truth, but were the truth. Here and there some great man, like Linnæus, saw the real meaning of his doctrine, as a guide to generalization; and of his system, as an index to his facts. But he wrote only for the learned, and those who explained his system popularly inflicted his nomenclature on the student, but failed to render his idea.

It is a good thing for an objector to be able to point out a remedy. A good thing, I say, but not a necessary thing. A cook who has spoilt the dinner cannot expect his master to tell him how he should dress it; and an instructor, whose teaching is obviously bad, cannot expect a student to show him his business. Improvements can only be carried out by those who are thoroughly conversant with the details. An outsider can only indicate general principles, and give general advice. I shall content myself with suggesting the following negative rules:—Never invent a name for a thing which already possesses an exclusive one. Never alter an individual name to accord with any theory. Even if a name be not exclusive, do not distinguish, until absolute necessity arise. Look upon individual names as subjects for certainty and fixity; but upon systems as provisional modes of grouping them. Never treat as fact what is merely the consequence of a theory.

I do but ask of the botanists and chemists to follow an example already set them by mathematicians on the one hand, and by men of business on the other. What is an old treatise on botany or chemistry worth now? But the old mathematicians are still profitably read by rising men—indeed, a man can do but little unless he does read them. One has certainly heard of libraries so neatly packed that, like a new plant in a botanical genus, a new book revolutionized the whole arrangement.

Soapstone of Mylos, an important article of commerce in Turkey and Russia, where it is used as soap, has been analysed by M. Landerer, the result being—silice, 63; alumina, 23; water, 12; and sesquioxide of iron, 1.25. This mineral is of a greyish colour, and has schistose fracture. It can be cut into shavings, and adheres a little to the tongue; softens in water, dissolving gradually in it; and afterwards becomes white and greasy to the touch. It becomes grey again after desiccation.

ANSWERS TO CORRESPONDENTS.

H. Mulcaster.—The second lecture is not yet delivered. Indeed, we do not think it was intended to give more than one.

A. O. Z.—Consult Knapp's Technology, published by Baillière.

An Idea.—There is no reward offered by Government. Inventors must take care of their own interests. The duties of the committee are simply to get all the information possible and to make the best use of it.

American Gas Light Journal.—Received. The exchange shall be sent.

W. Stevenson.—Received.

THE CHEMICAL NEWS.

VOL. IV. No. 102.—November 16, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Analysis of Feldspar, Kaolin, and Clay,
by M. H. SAINTE-CLAIRE DEVILLE.

1. **Treatment with Soda.**—The experiments which I have already performed on this subject suggested to me the idea of attacking the powder of Orthosen feldspar, clay, and kaolin by a concentrated lye of caustic soda the prolonged digestion effects a kind of solution of these substances in the form of a thick turbid syrup. When this is poured into water, there is produced a true decomposition, similar to that of bismuth salts. The syrupy matter, may, in fact, be looked upon as a hydrated and strongly alkaline aluminous glass, which is split up into two parts by the action of water: the soluble portion is a silicate of soda without a trace of alumina; the insoluble part is a double silicate of alumina and soda of a constant composition, which gave me the following results upon analysis:—

Silica	44.6	6
Alumina	26.4	3
Lime	0.4	
Soda	14.9	1
Potash	1.0	
Water, by difference	12.7	3
	100.0	

The water is rather low, and its proportion only corresponds to 2.3, instead of to 3; but the reason is that these substances cannot dry in the air without losing their combined water in the manner of zeolites.

It is remarkable that the same result is always obtained when we treat by digestion in soda either a silico aluminous material with an excess of alumina as bauxite, or a substance with an excess of silica as feldspar. In the first case there is dissolved an aluminate of soda without a trace of silica; in the second case there is dissolved a silicate of soda with a trace of alumina, the residue being the same in both cases. The same product is obtained, perhaps with more certainty, by evaporating the concentrated alkaline solution to dryness, and slightly calcining the residue. In any case a tolerably large excess of alkali must be employed. The silicate insoluble in water is, it must be understood, soluble in acids. Clays and kaolin behave in the same manner when treated with the caustic lye.

2. **Treatment with Hydrofluosilicic Acid.**—The solvent power which hydrofluosilicic acid $[2\text{Si Fl}_3, 3\text{H Fl}]$ has upon alumina is considerable, and exerts itself instantaneously. I did hope that I should be enabled by its means to remove from clay all the

alumina, and investigate the condition of the residuary silica in respect to its solubility in weak alkalies, which would be of interest in relation to the constitution of the clay contained in hydraulic lime nodules.

It is seen, indeed, from the above observations, that the alumina prevents the solution of nearly double its weight of silica (in the proportion of $\frac{44.6}{26.4}$) when the mixture of the two substances in the hydrated state is placed in contact with an alkali which would dissolve each of them separately. But an unexpected reaction takes place when weak hydrofluosilicic acid, such as is always used, is placed in contact with ignited alumina. There is at first formed a neutral fluosilicate of alumina $—2\text{Si Fl}_3, \text{Al}_2 \text{ Fl}_3$. Then, when an excess of alumina is added, and the digestion prolonged, the fluoride of silicium is decomposed, silica is deposited, and there is formed hydrated and soluble fluoride of aluminium: $\text{Si Fl}_3 + \text{Al}_2 \text{ O}_3 = \text{Si O}_2 + \text{Al}_2 \text{ Fl}_3$.

The presence of fluosilicate of alumina is shown by filtering the acid liquid which has stood over the alumina (I have always employed alumina obtained from the ignition of ammonia-alum), and by pouring into it chloride of sodium, the almost insoluble fluosilicate of soda is precipitated, and hydrochlorate of alumina is formed, whence the alumina can be precipitated by ammonia. Upon washing and re-dissolving this, no residue of silica will be perceived when the hydrofluosilicic acid and the alumina have been mixed in the proper proportions. The same reaction takes place between kaolin and hydrofluosilicic acid. It is, therefore, perfectly useless to employ pure alumina for the experiments which I have just described: kaolin answers the purpose quite as well, and, when it is free from iron, products of great purity are obtained. However, as kaolins always contain a little feldspar, and as this is readily attacked by hydrofluosilicic acid, a little potash gets into solution.

Warm hydrofluosilicic acid has a slight acid odour which immediately disappears when kaolin in excess is added to it; the saturation is complete when the odour of clay, which is somewhat alkaline, is apparent in the liquid. There then remains no more hydrofluosilicic acid in solution, but only hydrofluat of alumina, or hydrated fluoride of aluminium.

But before this transformation takes place, and when the liquid is still strongly acid, if strong alcohol be added to it an oily matter precipitates, which soon solidifies by crystallisation, and which is a kind of hydrofluoaluminic acid. It is composed of

		Calculated.
Fluoride of aluminium	65.4	$3\text{Al}_2 \text{ Fl}_3$ 66.2
Hydrofluoric acid	10.5	2H Fl 10.4
Water	24.1	10HO 23.4
	100.0	100.0

If the acid liquid is evaporated, instead of being treated with alcohol, acid is evolved, and a crystalline substance remains, which, after being washed in boiling water and dried in the air has the following composition:

¹ *Annales de Chim. et de Phys.*, lxi. p. 326.

² It would be better to employ the formula of M. de Marignac, $\text{Si Fl}_2, \text{H Fl}$; but as chemists are not yet accustomed to it I will continue provisionally to make use of the old formula, which, however, should be abandoned, as the new one is simpler and more rational.

			Calculated.
Fluoride of aluminium	. 59.0	$2\text{Al}_2\text{F}_6$	60.8
Hydrofluoric acid	. 8.8	Fl H	7.1
Water	. 32.2	10HO	32.1
	100.0		100.0

The slight difference which exists between the calculated hydrofluoric acid and that found by experiment is manifestly due to this product being contaminated with the preceding compound, which is a little richer in hydrofluoric acid.

Finally, if dilute hydrofluosilicic acid is completely saturated with excess of alumina or kaolin, a liquid is obtained which, during evaporation, does not liberate acid, and has a faint alkaline odour; it deposits in proportion to the degree of concentration a crystalline powder, which, when dried by artificial heat, loses a little water, and possesses the following composition:—

			Calculated.
Fluoride of aluminium	. 59.7	Al_2F_3	57.3
Water	. 40.3	7HO	42.7
	100.0		100.0

It is a hydrate, the composition of which, varying slightly with the degree of desiccation, should be represented by the formula $\text{Al}_2\text{F}_3 \cdot 7\text{HO}$, having regard to the ease with which it loses water upon the slightest application of heat.

All these compounds are free from silica, and resolve themselves at a red heat into fluoride of aluminium, which can be volatilised in a platinum crucible at a red white heat. This operation, which cannot be effected without the intervention of atmospheric oxygen, is never quite complete, in that there is always obtained in the platinum crucible a slight residue of alumina often crystalline.

It is remarkable that fluoride of aluminium, so insoluble in water, so perfectly unattackable by acids, presents itself under these circumstances in the form of a hydrate dissolved in a perfectly neutral liquid. We may imagine, to explain this fact, that there is a hydrofluo-aluminic acid of the composition $\text{Al}_2\text{F}_3 \cdot 3\text{HFl}$, analogous, consequently, to hydrofluosilicic acid, and of which cryolite, $\text{Al}_2\text{F}_3 \cdot 3\text{NaFl}$, would be a neutral salt, as well as the potash cryolite $\text{Al}_2\text{F}_3 \cdot 3\text{KFl}$, which may be prepared by pouring into a potash salt a solution of alumina in hydrofluoric acid.

In this case the soluble fluoride of aluminium would be simply a neutral fluoaluminate of alumina (making use of the old nomenclature used for the fluosilicates), $\text{Al}_2\text{F}_3 \cdot \text{Al}_2\text{F}_3$, or the hydrofluo-aluminic acid, $\text{Al}_2\text{F}_3 \cdot 3\text{HFl}$, in which each equivalent of hydrogen is replaced according to the ordinary law for the formation of neutral alumina salts, by $\frac{3}{2}$ equivalent of aluminium.

As to the hydrofluo-aluminic acid, it may be prepared by dividing a certain quantity of hydrofluoric acid into two equal portions, saturating one of them with alumina and mixing.

The preceding analyses have been performed in the following manner:—Place the weighed substance in a weighed platinum-boat, and introduce this into a small tube of green glass, drawn out at one end and closed at the other with a cork, through which a glass tube passes. Pass through it a current of dry hydrogen and heat the boat and the tube to a temperature of 400° or 500° C. with a spirit or gas flame. Water and hydrofluoric acid are evolved, which strongly attack the glass. Upon weighing the boat after the operation, the loss gives the water and hydrofluoric acid, the weight of the residue

giving the fluoride of aluminium. This should be almost entirely volatile at a white heat.

In the same boat weigh a fresh quantity of substance which is mixed with seven or eight times its weight of pure litharge fused and powdered. Weigh the whole and replace the boat in the glass tube, through which a current of dry nitrogen must be passed (for the apparatus to produce nitrogen in analyses, see *Annales de Chimie et de Physique*, third series, vol. xxxv., p. 442); heat the mixture without fusing the litharge, and water only is evolved. The glass tube, if new, should not be attacked. Weigh after the operation. The loss of weight gives the water of crystallisation in addition to the water formed at the expense of the oxide of lead, which is transformed into fluoride, and of the hydrofluoric acid, the fluorine of which combines with the lead. With these two estimations of loss, and by means of two equations of the first degree, the composition of the substance analysed can be obtained. Let a be the loss in hydrogen, b the loss in nitrogen, and in contact with the oxide of lead we have,—

$$\begin{aligned} x + y &= a \\ \frac{9x}{20} + y &= b \end{aligned}$$

in which x represents the hydrofluoric acid, and y the water. It is evident that the quantities of hydrofluoric acid will be proportional to $a - b$, and that when $a = b$ it is nothing. This happens whenever the neutral fluoride of aluminium is analysed.

I have obtained in these analyses very fine specimens of fluoride of aluminium. It is, indeed, the most economical and quickest method of obtaining this beautiful substance.

It may also be prepared very easily in the amorphous state by fusing a mixture of equal equivalents of cryolite and dry sulphate of alumina. In this manner are obtained sulphate of soda and fluoride of aluminium, which may be separated by water. The fluoride is then distilled in a tube of gas carbon in a current of hydrogen. In this way Captain Caron and I have procured the fluoride of aluminium, with which we have reproduced in the dry way some artificial minerals. M. Damour and I have likewise obtained it by heating wavellite to red-whiteness.

I have, in particular, succeeded in obtaining it in transparent crystals, which have enabled me to observe their influence on polarised light. I was much surprised to see these crystals, which I took to be cubic, sensibly rotate the light between two Nicol's prisms. I re-measured the angles, and found that they were not right angles, but equal to 88.20° or 88.30° . This was rather difficult to determine exactly, on account of the weak lustre of the crystalline facets of fluorine compounds, and of this one in particular.

Fluoride of aluminium, therefore, crystallises in rhombohedra of about 88.30° , and not in cubes, as I hitherto supposed. (For the properties of this substance, see *Annales de Chimie et de Physique*, third series, vol. xlix., p. 79.) If it is wished to prepare cryolite artificially, the best plan is to treat non-ferruginous kaolin with hydrofluosilicic acid, which gives fluoride of aluminium. Upon causing marine salt to react upon the same acid slightly concentrated, and warm gaseous hydrochloric acid and fluosilicate of soda are obtained. In the cold there is obtained a solution of hydrochloric acid, and an almost insoluble precipitate of fluosilicate of soda. When this is calcined in a muffle, it is transformed into fluoride of sodium, which, when mixed and fused with the fluoride of aluminium, gives cryolite.

Cryolite may be also obtained by treating fluoride of sodium with hydrated fluoride of aluminium; it is precipitated in the pulverulent state.

The hydrofluosilicic acid I have prepared in all my experiments by passing steam, or better by allowing water to fall, drop by drop, on a coarse mixture of sand and pounded fluoride of calcium heated to redness in a tubulated stone retort, and condensing the vapours of hydrofluosilicic acid, which will be a sufficiently strong liquid to mark 17° of the hydrometer, and which may be brought by concentration up to 29° or 30° . Not a trace of silica is deposited in this operation. Hydrofluosilicic acid prepared by means of fluoride of silicon and water only marks 4° or 5° when the water has become solid with the gelatinous silica.

At 7° Baumé, hydrofluosilicic acid in solution contains 66 grammes per litre of anhydrous acid ($2\text{SiF}_6 \cdot 3\text{H}_2\text{O}$).

At 29° it contains 325 grammes per litre, and possesses its maximum concentration.

I have performed these determinations by taking 10 cubic centimetres of the acid under trial, and treating it with an alcoholic solution of acetate of potash, and washing with alcohol the fluosilicate of potash, which appears gummy, but which is readily permeated by liquids. Dry the filter, and weigh the fluosilicate after having heated it. The fluosilicates of soda are crystalline and tolerably dense salts, which only appear gelatinous and transparent because their index of refraction is scarcely superior to that of water itself. Hydrofluosilicic acid at 29° is a very powerful acid, which drives off by the aid of heat almost all other acids, sulphuric excepted. It cannot be preserved in glass, but it neither attacks wood nor organic matters, and it may be preserved in small casks. It changes glass into fluosilicate of soda with great rapidity. I am persuaded that it will become of great industrial importance when its economic manufacture is understood.

3. Iron and Pyrites.—Clay can be used instead of kaolin to prepare pure aluminous products. But it must not be imagined that the iron contained in the clay can be removed from it by treatment with muriatic acid. That which is in a state of oxide is dissolved, but there remains iron in the form of pyrites. To completely purify clays, they must be digested with a solution of hypochlorous or hypochloric acid, which may be very easily prepared by a method given by MM. Calvert and Davies (by heating in a water-bath a mixture of equal parts of oxalic acid and chlorate of potash, and collecting the gas in water). This is the analytical process which I have for some years taught my pupils at the Ecole Normale to dissolve metallic sulphides, and to estimate the pyrites in clays and hydraulic limestone. The pyrite is transformed into sulphate of iron upon contact with the hypochlorous acid, which does not attack the sesquioxide and carbonate of iron.

Clays thus treated become white and at once suitable for the manufacture of pure aluminous products by the methods just described.

4. Vanadium.—Whilst conducting my researches on the mineral from Baux, M. Terrell informed me that M. Beauvallet thought that he had found small quantities of vanadium in clay from the neighbourhood of Paris. I mentioned in the following terms this interesting discovery, hitherto unpublished, in the memoir of my own experiments, presented to the Académie des Sciences of Paris:—"I have heard in connection with researches which are not my own that vanadium is likely to become

more common than could be imagined. If the experiments to which I allude are confirmed, they will impart a still greater interest to a body which is at present extremely rare, but with which the labours of Sefstrom, Berzelius, Wöhler, and H. Rose have made us thoroughly acquainted."

And, indeed, M. Beauvallet shortly after published his own researches. This young chemist has shown me some very finely-crystallised vanadate of ammonia, which he had obtained from the clays of Gentilly. M. Terrell has since confirmed them by extracting small quantities of vanadium from the refractory clays of Forges-les-Eaux. It, therefore, seems to be well established that the presence of vanadium may be discovered in all aluminous, and often in ferruginous, minerals. Captain Caron has found it in the emerald of Naxos, in a certain number of natural aluminates, and lastly, in a very abundant iron ore found in the department of Cher.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 245.)

Determination of the Relation of the Atomic Weights of Silver and Chloride of Potassium, Chloride of Sodium, and Chloride of Ammonium.

—The determination of the relations between the atomic weights of these four bodies can be made with such exactness that if we can arrive at the conviction or the certainty that the bodies experimented upon are pure, the result obtained should be considered as almost absolutely exact. I have, therefore, in these determinations, an infallible means of submitting Prout's law to a decisive irrevocable proof. I have, therefore, taken all imaginable care to procure these chlorides pure. To ensure this, I have had recourse to physical and chemical forces to separate the bodies which may be combined or mixed in a more or less stable manner. In order to make the results control each other, I have repeated the determinations so great a number of times that I much doubt whether there exists in the annals of chemical science an example of a greater endeavour to arrive at the discovery of the truth. I have devoted a whole year to these experiments, which are apparently so simple.

Determination of the Relation which exists between the Atomic Weights of Silver and Chloride of Potassium.—I will detail, as briefly as possible, all the means which I have employed to procure the chloride of potassium. This account may appear tedious, but it seems to me quite indispensable; for I attach great importance to the determination of the relation existing between these two bodies.

1. Chloride of Potassium from Chlorate of Potash.—The purest chlorate of potash which can be obtained by means of repeated crystallisation always contains iron, manganese, silica, and sometimes alumina and copper. M. de Marignac has already proved the difficulty of separating iron from both chlorate and perchlorate. To effect the elimination of these impurities, I add to a filtered and boiling solution of chlorate, rendered alkaline by pure potash, a few drops of sulphide of potassium solution. After boiling for a quarter of an hour, I filter the boiling liquid and add to it, still keeping it in ebullition, a fresh quantity of sulphide of potassium. If, after the first addition of the alkaline sulphide, the liquid has been kept long enough in ebul-

lition, the subsequent addition does not occasion the least discoloration. When this is the case, I quickly cool the solution, so as to obtain the salt in the form of a crystalline powder. The magma is introduced into a displacement apparatus, the bottom of which is closed by a linen plug, and washed at first with alkaline and afterwards with acid water. This apparatus is connected, by means of a caoutchouc tube, with a reservoir which is in communication with an air-pump. A few strokes of the pump immediately remove the mother-liquor and the salt appears dry. I then moisten it with pure water, a few drops only at a time, constantly working the pump until the liquid which escapes below has no alkaline¹ reaction, and neither precipitates nor colours nitrate of silver. When the purification is effected in this manner, very little washing water is required. Two kilogrammes of commercial chlorate yielded at one operation from seven to eight hundred grammes of salt, perfectly free from iron, copper, and chlorine, but still retaining silica, and sometimes alumina. I then dissolve it three times in succession in pure water, taking the precaution each time to cool the solution rapidly and eliminate the mother-liquor in the displacement apparatus, with the assistance of the air-pump.

I then decompose the chlorate thus purified and dried in a platinum retort by a careful application of heat. Traces of chlorine are evolved and a chloride is produced which has not the least alkaline reaction. Upon employing chlorate obtained by means of crystallisation only, and which consequently retains at least manganese, iron, and silica, a decided disengagement of chlorine takes place and an alkaline chloride is produced. The purest chloride, when it is decomposed without a judicious management of the heat produces, however, a very notable disengagement of chlorine.

Since the chloride thus procured still contains traces of silica, and sometimes of alumina, it must be purified from them. The following are the various methods to which I have had recourse to obtain this result:—

I fuse the chloride in a platinum crucible enclosed in a second crucible. I then introduce into the liquid a bent platinum wire, to the curve of which I attach a pellet of fine platinum wire. I thus collect together all the suspended matter. When there are no more brilliant points—for it is under this form that silica and alumina present themselves in fused chlorides—I allow the crucibles to cool slowly. When part of the chloride is solidified I decant it, and cause the portion still liquid to pass through the solid mass. I thus effect a veritable filtration of the fused chloride through crystals of the same substance.

The chloride, after it has been received into a platinum dish and become solidified, appears under the form of either a vitreous, colourless, and perfectly homogeneous substance, or as a crystalline mass, of a cubical structure, according as the cooling has been rapid or slow. In these two conditions it possesses a sensibly different specific gravity.

I have only made use of this chloride for my determinations when a portion dissolved in water produces a liquid which is neutral to test-paper and absolutely limpid, which is, I confess, very seldom.

The results given under numbers I. and II. (Second Series) were derived from two different specimens of chloride obtained in this manner.

The chloride thus purified was dissolved in pure water, contained in a platinum dish. After standing for

some time, the perfectly clear solution has been decanted into a platinum retort and mixed with chloride of ammonium, obtained by the direct combination of ammonia with hydrochloric acid, both pure and in the gaseous state. The mixture is evaporated to dryness. The residue, well dried, was introduced, by small portions at a time, into a platinum crucible, contained in a second one, and fused. After cooling, the still liquid portion of the chloride was filtered through the crystalline mass.

Chloride of potassium thus prepared has always been perfectly colourless and homogeneous. When dissolved in water it forms a solution absolutely limpid and perfectly neutral to test-paper.

Numbers III., IV., and IX., the weighings of which have been performed *in vacuo*, were effected with two different specimens of this chloride.

Although these chlorides have furnished such concordant results, they may, nevertheless, contain other chlorides. I have, therefore, attempted to satisfy myself on this point by two distinct methods,—firstly, by dividing one portion of these chlorides into four successive parts by crystallisation from a pure aqueous solution; and, secondly, by causing a chemical force to act upon the chloride,—that is to say, by uniting it into a perfectly definite compound susceptible of being well purified.

To this end I have saturated about 200 cubic centimetres of pure boiling water, contained in a platinum dish, with chloride of potassium, from determination No. IX. (Second Series), and I have rapidly effected its crystallisation. The mother-liquor, decanted and mixed with the washing waters, was evaporated until a pellicle was formed on its surface; this furnished a second mother-liquor, which was evaporated after the addition of the washing waters until a pellicle formed and yielded a third crop of chloride. The mother-liquor from this was evaporated to dryness. The four specimens of chloride which were obtained in decreasing quantities were mixed with a tenth part of their weight of pure chloride of ammonium and thus submitted to fusion.

All these chlorides were colourless and dissolved in water, producing a neutral and perfectly clear liquid. The results given under the numbers IX. a, IX. b, IX. c, IX. d, are obtained from determinations made with these four specimens of chloride.

3. Chloride of Potassium from Chloroplatinate of Potassium.—I have united chloride of potassium from the chlorate with bichloride of platinum, in order to separate the chlorides which it might contain, and which crystallisation would not remove. For this object I procured a sufficient quantity of pure platinum. To get this I followed the method described by Berzelius, which is, however, very expensive, inasmuch as it necessitates the complete destruction of a large platinum crucible in which the operation is performed. I have repeated the purification three times instead of twice, as recommended by the illustrious Swedish chemist. Platinum thus purified is much whiter than the metal met with in commerce, and when fused is excessively ductile and malleable, as M. H. St. Claire Deville has recently stated.

I then dissolved this platinum in weak aqua regia, employing for this purpose a Bohemian glass flask. The chloride, evaporated to dryness, was dissolved in alcohol. I then added an aqueous solution of chloride of potassium from the chlorate, taking care to leave the platinum in excess. The chloroplatinate of potassium was washed first with dilute alcohol, containing chloride of platinum in solution, and then with weak pure alcohol. The

¹ Acid?

chloroplatinate of potassium was mixed with a fourth of its weight of perfectly pure chloride of ammonium and calcined at a bright red heat in a platinum crucible. The residue was dissolved in cold, pure water, and the solution was filtered through pure, spongy platinum, which had been strongly heated and placed in a drawn-out funnel. I took the platinum sponge as a means of filtration because I found that a solution of alkaline chlorides passed through paper always took from it traces of silica.

In order to get rid of the small metallic particles which might have been carried through, I allowed the solution to remain undisturbed for at least twenty-four hours in a platinum dish. At the end of this time the liquid was carefully decanted, mixed with pure sal-ammoniac, and evaporated to dryness in a platinum dish; it was finally fused in a double platinum crucible. The fused chloride never presented any traces of brilliant spangles. It may be decanted to the last drop without any foreign body whatever being perceived. When solid it is perfectly colourless, and when dissolved in water produces an absolutely pure and neutral solution.

Numbers v. and vi. (Second Series) of the Table refer to chloride of potassium purified from chloroplatinate of potash; and Number vii. to the same chloride, which was twice combined and twice separated by the means just indicated.

3. Chloride of Potassium from Nitre.—Two different specimens of nitre, weighing about 250 grammes each, which had been crystallised at least ten times each, and were completely freed from iron, manganese, copper, &c., were mixed with one and a-half times their weight of pure chloride of ammonium. Each of these mixtures was projected, little by little, into a large platinum capsule heated to full redness. The first portion of the mixture attacked the platinum pretty strongly, but when once the bottom was covered with alkaline chloride the additions of the liquid could be continued without sensibly altering the metal. At each addition a deflagration was produced. The resulting chloride of potassium was pulverised and mixed with a tenth of its weight of pure sal-ammoniac and of chloroplatinate of ammonia. The whole was fused in a double crucible of platinum. The platinum derived from the decomposition of the chloroplatinate of ammonium carries down with it to the bottom of the crucible the silica which the purest nitre always contains. After standing a certain time, the liquid is decanted into a platinum vessel. The chloride of potassium thus obtained has a very decided violet tint, due to the presence of very finely-divided platinum.

To separate this platinum, I dissolve the chloride in cold, pure water, and filter the solution through strongly-ignited platinum sponge. The filtered liquid, after standing for twenty-four hours, was treated in the way described for chloride of potassium extracted from the chloroplatinate of potassium.

The two specimens of chloride thus produced were each perfectly colourless, neutral to test-paper, and yielded with cold water an absolutely limpid liquid.

The determinations of Numbers viii. and x. (Second Series) refer to these two specimens.

At least fifty grammes of the chloride of Number x. were dissolved in pure water and divided by crystallisation into four successively diminishing portions, as explained in the case of the chloride prepared from the chlorate. I have determined the proportional relation of the four specimens thus obtained; the result is given in Numbers x. a, x. b, x. c, x. d (Second Series).

4. Chloride of Potassium prepared from Tartar.—Cream of tartar, crystallised ten times, was carbonised in a silver crucible and the carbon exhausted with pure water. The carbonate of potash was mixed with an excess of pure hydrochloric acid and the liquid evaporated to dryness. The residue was heated to dull redness and taken up after cooling with cold water. The solution was allowed to rest until it had become perfectly limpid by the deposition of silica; then decanted and supersaturated with ammonia it allowed traces of peroxide of iron and alumina to be deposited. After the liquid had again become clear I evaporated it in a platinum vessel and mixed the residue with a tenth part of its weight of pure sal-ammoniac and of chloroplatinate of ammonium. The mixture was fused, and the resulting chloride, as I have explained in the case of the chloride of potassium, prepared from nitre.

This chloride was absolutely colourless, neutral, and dissolved in water, forming a perfectly limpid liquid.

The results given under Number xi. refer to the chloride prepared in this manner.

(To be continued.)

Notes on Nitrification, by R. HAINES, M.B., Professor of Materia Medica, Grant Medical College, Bombay.

(Continued from page 246.)

A further step has been taken by M. de Luca, who has shown that porous substances were not essential to this combination. Air was ozonised by passing over phosphorus, then purified by passing through carded cotton, and over potassa and sulphuric acid; the pure ozonised air then passed over potassium and pure potassa. In three months the potassium became converted to potassa and deliquesced, and from both alkaline solutions nitrate of potassa could then be separated by crystallisation.¹

Oxygen, then, in the form of ozone, required but the concomitant predisposing affinity of an alkali to unite at once with nitrogen as nitric acid. Ozone, as Schönbein has shown us, is produced whenever an electric spark traverses atmospheric air; hence, in Cavendish's experiments it is ozone which is the medium of the combination, the water of the air and the gas jar playing the part of base, and not a feeble one, to the newly-formed acid.

But ozone, besides being produced artificially during the slow combustion of phosphorus and by electric sparks, is a natural and normal constituent of atmospheric air: it is believed to be chiefly formed during the evolution of oxygen by the leaves of plants. It is comparatively abundant in the air of rural districts, but disappears almost completely in the air of large towns or in crowded dwelling-houses. It remained, then, with regard to the nitrifying powers of oxygen and uncombined nitrogen, merely to put to the crucial test of more natural conditions the phenomena thus artificially evoked.

This also has been effected by M. de Luca. He prepared three sets of apparatus similar to those last described, but without the ozonising portion, by which atmospheric air was slowly drawn first through carded cotton, then over sulphuric acid, then over potassium, and, lastly, over hydrate of potassa. Two such apparatus were set to work in the court of the laboratory of the College of France, and one in a hot-house of the Botanic Gardens at the Luxembourg. The apparatus worked only during the day for six months; at the end of that

¹ *Comptes-Rendus*, December 31, 1855.

time both the potassium and the hydrate of potassa had run down into liquid potassa in all. In this liquid, in the two apparatus in the open air at the College of France, not a trace of nitric acid could be found; in that in the hot-house there was abundance of nitric acid, so that nitrate of potassa had begun to crystallise out.

There are, therefore, two distinct methods in which nitric acid is formed in nature, which are alike only in this, that both are processes of oxidation, and that in both the presence of some powerful basic material is essential to determine the combination. In the one, ordinary oxygen and ammonia are, by the agency of porous bodies, condensed in their pores, and in the presence of alkaline bases made to combine and form nitrates of such bases; in the other ozonised oxygen, whose affinities are vastly more powerful, unites directly with nitrogen in the presence of strong bases. Both these processes, there is every probability, go on incessantly; the nitric acid of the air being chiefly formed by the latter method; that of the soil chiefly, but not exclusively, by the former; not exclusively, because if ozone can oxidise nitrogen without the intervention of porous bodies, still more readily will it do so when it meets with them along with ordinary oxygen.

The question, in reference to the researches of De Luca, is of interest in another way, as it concerns the balance of organic nature. Hitherto it seems to have been the received doctrine that the quantity of combined nitrogen in the globe was constant, not susceptible at least, of increase. Philosophers have, I know, been chary of broadly stating this in so many words, but it is tacitly assumed whenever the subject is touched upon in the works of Liebig, Dumas, and others of the organo-chemical school which they have established. It is concluded, without dispute, that the nitrogenous proximate principles of animal bodies are directly derived from the nitrogenous principles of the vegetables which form their food. Animals appear to be totally destitute of the power of assimilating nitrogen, as such; neither can they convert such simple compounds as ammonia or nitric acid into the complex principles of animal life. On the contrary, if the careful and concordant experiments of Regnault and Reiset, of Valentin and Brünner, and of Boussingault, are correct, a portion at least of such nitrogenous principles suffers entire destruction, and the nitrogen is evolved uncombined, for expired air always contains an excess of nitrogen, which can only be derived from the decomposition of nitrogenous compounds.

During the natural decay of animal structures the whole of the nitrogen is evolved in the form of ammonia, or of some of its modifications, and by this ammonia vegetables are nourished, and by further combinations the plastic materials which are to form the food of animals are built up. In this circulation of the nutrient materials of the animal and vegetable worlds, ammonia forms the zero point to which once in every round the nitrogen compounds unceasingly return,—not, however, as we have seen, without diminution.

Again, vegetables have as little power as animals to assimilate uncombined nitrogen, but their capacity to destroy such already-formed combinations far exceeds that of animals; the oxygen exhaled by the leaves is invariably mixed with pure nitrogen, which in some cases equals, or even exceeds, the oxygen in volume. This nitrogen can only be produced from the decomposition of the absorbed ammonia, or its equivalent nitric acid.

Moreover, the nitric acid which is formed so abundantly from ammonia is liable to total decomposition.

Whenever gunpowder or similar mixtures are burnt, the nitrogen of the nitrate is evolved in a free state, no longer capable of taking its place in the series of transformations. The cycle is to some extent a double one,—nitric acid forming the starting point of a loop line,—in every stage of which except one the combined nitrogen suffers diminution, thus:—

- | | | |
|------------|-----------------------|--------------------------|
| 1. Ammonia | { 2. Ammonia | 3. Vegetable* Principles |
| | 2. Nitric acid* | 3. Vegetable* Principles |
| | 4. Animal* Principles | 5. Ammonia. |
| | 4. Ammonia | |

The points of the series at which this diminution occurs are marked by an asterisk.

Of the original formation of ammonia we know little. The numerous experiments by which it has been attempted to show its production by the union of free nitrogen with nascent hydrogen, are all inconclusive. That it exists, however, in the interior of the earth, is shown by the fact of its almost constant occurrence in the vapours emitted from volcanoes, in the Solfonni of Tuscany (with boracic acid), and in solid masses of rock-salt. In Liebig's words, it "has not been produced by the animal organism: it existed before the creation of human beings; it is a part, a primary constituent, of the globe itself." But the amount given off in the atmosphere cannot be very great; hardly comparable, one would think, with the amount destroyed in the functions of animal and vegetable life. It is not, therefore, without some reason that the fears expressed in the following extract should have occurred to a thoughtful observer:—

"With regard to gunpowder, the question may well be raised, whether its use can be said to harmonise with the economy of Nature. I do not mean by this the consideration that men and animals are killed by means of firearms, but rather the fact that by the combustion of gunpowder a portion of nitrogen which formerly belonged to Nature's capital stock of ammonia, is turned off into the atmosphere as free nitrogen, no longer capable of taking a part in the molecular transformations of the vital organism. The result, therefore, cannot but be, that the capital stock of ammonia in Nature will, in course of time, be diminished; for we are unacquainted with any natural phenomenon whatever capable of augmenting it. If this be the case, the vegetable and animal kingdoms must, after some thousands of years, suffer diminution."

Our knowledge of the natural formation of nitric acid through the medium of ozonised oxygen, if worthy of interest on no other grounds, will at least satisfy our conceptions as to the universal harmony of natural operations, and will tend to relieve our fears for the well-being of a distant, and, we may now hope, flourishing posterity.

TECHNICAL CHEMISTRY.

The City Pumps,⁴ by H. LETHBRIDGE, M.B., M.A., Ph.D.,
&c., Medical Officer of Health for the City of London.

THE water from twelve of the City pumps has been analysed during the quarter, and the results, as in former

¹ "Agricultural Chemistry," First Edition, p. 109.

² Mohr's "Commentar zur Preussischen Pharmacopoe," II. 34.

⁴ From the "Quarterly Report on the Sanitary Condition of the City of London."

cases, show an enormous amount of saline and organic impurity. Up to the present time there have been thirty-four of the City pumps examined, and in every case the water has been fouled with surface drainage. Not one of them derives its supply from the deep strata of the London basin; and, excepting the pump in Glovers' Hall Court, and that in Guildhall Buildings, none of them furnish water that is fit for domestic purposes; indeed, it is difficult to understand what motive or inclination can lead persons to the continued use of them. If such water were supplied by the public companies of London, there would be an universal condemnation of it; for at the best of times, some of these pumps yield water which contains from 100 to 130 grains of saline and organic matter in the gallon; and all of them are excessively hard from the altered products of decomposition. The fact of the presence of ammonia, saltpetre, sulphate of lime, common salt, and organic matter in large proportions is sufficient to indicate the source of the supply; and the knowledge of the composition of the soil through which the water passes should warn us of the danger that may at any moment arise from its use. It may be that the water has often been drunk with apparent impunity, and that it has rarely shown any immediate manifestation of its morbid action; but it cannot be that the products of corruption can be constantly admitted into the human body without danger of insidious mischief,—and there is the still greater danger of the impurities of the soil passing unchanged into the water, and being a source of quick and certain injury. This is indicated by the circumstance that the water from the City wells is constantly changing,—that it is hardly the same from hour to hour; for the soil through which it passes is pierced in every direction with drains and sewers, and is charged with every species of corrupting refuse, which passes in variable proportions into the porous strata from which the water is pumped. This ought to tell us, not merely of the disgusting nature of the supply, but also of the dangers which accompany it. Experience has shown that wells like these are liable at any moment to receive the leakings from a cesspool or a sewer, and thus to be the immediate cause of fatal disease. In the autumn of 1854 there was a sudden and serious outbreak of cholera in the parish of St. James, Westminster. The course of the disease was confined to a small area in the neighbourhood of a favourite pump in Broad Street; and soon it was remarked, that of seventy-three persons who died during the first days of the visitation, sixty-one had been drinking the water of the pump. It was also remarked, that among persons who were living in the same street, and occasionally in the same houses, those only were attacked who drank the favourite water of the pump; in fact, in a number of cases which were particularly investigated, it was ascertained that persons who lived at a distance from the parish, and who had the water sent to them because of its supposed goodness, were seized with cholera and died. A full inquiry into all the circumstances of the matter proved that the well had become charged with cesspool drainage, and had thus acquired its poisonous action. The pollution had perhaps been going on for years, and yet the water had not betrayed it, and had been drunk with comparative impunity: indeed, its cool and sparkling qualities, the fatal fascinations of corruption, had gained for it such a high repute in the neighbourhood, that it was a favourite water, and was generally drunk. Another such a case occurred at West Ham, in Surrey, in the autumn of 1857. Suddenly at that place there was a visitation of

cholera in a row of sixteen cottages that were apparently isolated from epidemic influences. It showed itself along one side of the street, where, in a few days, thirteen persons were attacked with the disease, seven of whom died. Dr. Elliott, the Health Officer for the district, suspected from his inquiries that the common pump on that side of the street had been concerned in the mischief; its water was, therefore, examined, and it was found to be polluted with the soakage from an adjoining sewer. At once the use of it was interdicted, and, to make the matter certain, the handle of the pump was taken away, and from that moment the further spread of the disease was arrested.

Again: in the cholera visitations of 1848-9 and 1853-4, there were two striking examples of the influence of such water in the propagation of disease. The southern districts of London, comprising nearly a fifth of the population of the metropolis, were visited most severely with cholera at both of those outbreaks, and the persons who suffered most on each occasion, were those who drank the worst quality of water. The inhabitants of those districts are supplied by two rival companies, who obtained their water from the Thames at different parts of its course. In one case the water was charged with a larger amount of organic matter than in the other; and, although the conditions of the population were in every other respect the same, yet this had the effect of augmenting the mortality to a frightful extent. In the second visitation of the disease, the circumstances of the supply were changed: the water of the old company, which was once the worst, was then the best, and the severity of the disease was changed likewise; for those who partook of the still bad supply suffered as before, and their mortality was three and a-half times greater than their neighbours. Here was an experiment on a large scale, such as "no sceptical philosopher would have dared to propose," in which a large number of persons were placed under circumstances exactly alike, except in one particular, namely, in the use of a water charged to a comparatively small extent with organic pollution, and this determined the result—a mortality of 130 in the 10,000 instead of 37. A similar tale may be told of the cholera visitation of other places in this country. The town of Newcastle-upon-Tyne was supplied with comparatively pure water in the year 1849, and it then suffered but little from cholera; whereas, in the visitation of 1853, when there was so calamitous a loss of life from this disease, the water supply was made impure by the drainage from the sewers. "The same effect," says Dr. Farr, "was the result of the same cause in Hull, in 1849. And other examples may be cited, in which the converse happened, as at Exeter, where the inhabitants, after having suffered severely from cholera in 1852, obtained pure water, and escaped its ravages."

Nor are the percolations from the graveyards of a city less injurious; experience has demonstrated that this also is a prolific source of disease. Sir James Magregor relates, that when the British army was in Spain, about 20,000 soldiers were buried in a rather small space of ground; this was going on for two or three months, and soon the troops who drank the water from the wells of the neighbourhood, were attacked with dysentery and malignant fevers. The cause of the mischief was clearly traced to the hardly recognisable impurity in the water from the shallow wells. Here, however, in the churchyards of this City, there are the remains of ten times such a buried army undergoing decay; and in the whole of this metropolis, in a space of not more than 218 acres of soil, there were buried not long since as many as

50,000 dead in the year. In a generation of thirty years, this would give us a million and a-half of decomposing bodies in the surface soil of London; and through these the water percolates, to find its way into the porous stratum which supplies the shallow wells. At best, the change of this corruption is but imperfect, and the presence of ammonia and saltpetre tells of the process of decay, and indicates the danger which accompanies it.

All these considerations have forced upon us the conclusion that water from rivers and surface wells, contaminated with the refuse of drains, and the soakings from graveyards, is unfit for public use; and, with the knowledge of this fact, I should be wanting in my duty, as your sanitary officer, if I failed to inform you of it.

On Aniline Blue, by M. CH. LAUTH.¹

A COMPARISON of the formula of aniline violet, $C_{20}H_{17}N_3O_2$, with that of aniline red, $C_{20}H_{20}N_4O_4$, led me to think that by treating the latter with reducing agents, it might be transformed into the violet. I therefore tried all the ordinary reducing agents, but none produced a satisfactory result. When treating, however, a solution of aniline red in commercial alcohol with a salt of tin, I noticed that the colour of the mixture, which at first was yellow, slowly became brownish, then green, and finally blue. The red matter had disappeared, and a new product had formed—*aniline blue*. I soon convinced myself that the active agent in this transformation was aldehyde, commercial alcohol always containing a certain quantity of this body. I now prepared some chemically pure aldehyde by means of the aldehyde of ammonia. I also prepared pure mononitrated trianiline by the following process:—Rough aniline red was mixed with four times its weight of sand, and then treated several times with boiling water. The sand was used to retain the resinous matters which always accompany rough aniline. The aqueous solutions were now precipitated by means of common salt, and the precipitated colour was again treated as the rough product had been. This operation was repeated three times; the last solutions in boiling water were treated with a boiling solution of common salt, and on cooling the liquid deposited brilliant crystals of mononitrated trianiline.

These crystals are dissolved in sulphuric acid diluted with four times its volume of water, and to that solution the pure aldehyde is added. The reaction commences in a few minutes, and after a time, longer or shorter, according to the quantity of aldehyde and acid employed, the red colour completely disappears, and gives place to a beautiful blue. As soon as the change takes place, the liquid must be saturated with caustic soda, which causes an abundant precipitate of the new colouring matter. The liquor contains acetate of ammonia.

In the above reaction aldehyde may be replaced by most of the hydrides, the essential oil of bitter almonds, the hydride of valeryl, and the oils of rue and aniseed. Any acid or acid salt may also be substituted for sulphuric acid. It is most probable that the transformation of the red to a blue colour, is due to some reducing action. M. Willm, however, is occupied with the analysis of the products, and will shortly publish his results.

The new colour has the following properties:—It is soluble in alcohol, ether, glycerine, &c., and dissolves in acids and alkalis. It is precipitated from the latter solutions when they are saturated. It is completely pre-

cipitated from its solutions by tannin, which forms with it a compound totally insoluble in water, only slightly soluble in alcohol, but more so in acetic acid.

The blue compound possesses most of the chemical properties of the other coloured derivatives of aniline, but not their stability. At 200° it is completely destroyed. Light quickly acts on it. It dyes wool, silk, and albumenized cotton, and gives shades of colour of great purity. When combined with aniline red, it produces a violet incomparably more beautiful than the ordinary violet.

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 29, 1861.

J. P. JOULE, LL.D., *President, in the Chair.*

Professor A. D. Bache, Superintendent of the United States Coast Survey, and Commander M. F. Maury, of the United States Navy, were elected Corresponding Members of the Society.

John Edward Morgan, Esq., M.B., was elected an Ordinary Member.

Mr. SPENCE brought before the meeting part of a mass of iron and copper pyrites containing abundance of large and well-defined crystals of pure arsenious acid.

Not only are these crystals a novelty as a natural product, but as an instance of rapid mineralisation they are interesting. The lump was found among a cargo of small disintegrated ore imported from Huelva, in Spain, and containing no solid masses of large size, and the lump was merely an aggregation of small pieces firmly agglutinated together and full of hollow cavities, studded over with the crystals of arsenious acid; and from the history of the cargo of ore it is not probable that more than twelve months has been required for their development. Heat does not seem to have been the cause of the metamorphosis, as there is no evidence of heat at all approaching to ignition having occurred; discoloration of the ore would at once have been the result of this.

The ore is chiefly a sulphide of iron and copper; but as an arsenide of one or both of these metals exist in it, decomposition of this latter must have taken place; the arsenic has become oxidised, and crystals of arsenious acid are the result.

The piece of ore will be deposited in the Society's mineral collection.

The discovery of these crystals being pure arsenious acid was made by Mr. Bottomley, Mr. Spence's assistant in his laboratory.

An interesting conversation followed, in which the fact was stated that arsenic is a constituent of nearly all the artificial manures which have superphosphate of lime as their basis; and in connection with this a report was named of arsenic having been found in some of the crops grown with such manures.

Professor Roscoe exhibited the beautiful lithographic map representing the dark lines in a portion of the solar spectrum, lately published by Professor Kirchhoff. The lines are printed in ink of six different shades, and are of six different degrees of thickness, so that the leading features of the spectrum can be at once recognised. The position of the bright lines produced by the incandescent vapour of certain metals is also given on the map, and the coincidence of many of these with the dark Fraunhofer's lines rendered evident.

Professor Roscoe stated that the length of the drawings when complete would amount to some twenty feet, and that to give an idea of the scale on which the map was

¹ *Bulletin de la Société Chimique de Paris*, No. iv. p. 78.

made, he might remark that the distance between the two double lines "D" is upwards of four millimètres. These maps are as yet only printed together with the memoir by Professor Kirchhoff, upon the Solar Spectrum and the Spectra of the Chemical Elements in the Transactions of the Berlin Academy; an English edition will, however, shortly be published, giving a translation of the original memoir, and containing the same drawings of the spectrum as those exhibited, which accompany the German text.

Dr. R. AUGUS SMITH read the first part of "An Examination into the Products of the Putrefaction of Blood." He found that 54° Fahr., or nearly so, was a marked temperature; that below it there was little putrefaction, but above it a large amount, which increased as the temperature increased as far as he tried, viz., to 72° F. The amount of gas given in twenty-four hours, from three quarts of diluted blood, was one hundred cubic centimètres, at 57° F. (16° Cels.), but on raising the temperature to 72° (22° Cels.) the amount of gas in the same period rose to 397 C.C., or four times the amount of putrefaction by fifteen degrees rise of temperature. When the temperature was below 54°, no gas could be collected for many days. A very short rise above 54° caused sufficient pressure to allow gas to be collected. These facts were used to illustrate climate and sudden spread of disease.

During the early period of putrefaction, the Author found the sulphuretted hydrogen and organic matter to increase rapidly. One hundred cubic centimètres destroyed 29 of a solution of permanganate; the amount then rose rapidly to 36, 40.6, 41, 60.2.

Nearly ninety-eight per cent. of the gas that escaped was found to be carbonic acid and gases with a similar action, i.e., absorbed readily by caustic alkalies.

The gases were found to be:—

Carbonic Acid.	Sulphuretted Hydrogen.	Residue.
82	—	17.32
85.78	—	14.22
89.72	—	10.28
95.40	—	4.60
96.20	—	3.80
96.07	1.58	2.35
96.43	2.78	0.79
97.62	0.06	2.31
97.09	1.93	1.98

The residual gas was found to consist of:—

Carbonic Oxide	4.8 per cent.
Carburetted Hydrogen	2.5 "
Hydrogen	6.2 "
Nitrogen	86.5 "

100.

Where the sulphuretted hydrogen is 1.5, the sulphur is to the carbon as 1 to 24.8

Both the oxygen and carbon of the carbonic acid are derived from the blood, which is therefore carried rapidly away.

The point of greatest importance was said, by the author, to be the separation of these substances, which do not seem to be pure gases, and which are entirely absorbed by alkalies, and partly by acids and metallic salts. The portion absorbed by acids salts was found to contain carbon and nitrogen, in the relation of 140 to 54, or as 100 to 38.5; but part of this was evidently as ammonia. In albumen it exists in the relation of 100 to 28.9. The whole of the putrefactive matters were not removed by acids or by acid salts. When the carbonic acid and sulphuretted hydrogen are removed, the putrefactive matters still remain. These gases, therefore, are not the only substances to be feared.

The condition in which these putrefactive bodies exist was then discussed. The author believed that one of the conditions in which solid substances were taken into the air was in solution, the solution itself being taken up as a

vesicle; and he instanced analogous cases, such as sulphuric acid and zinc when hydrogen is forming. The liquid evaporates and a concrete globule forms, leaving at last a portion of solid matter in various states. This condition can be supposed to occur readily in many cases, but it does not appear to be the probable result in all cases, as he cannot readily imagine vesicles coming through the close pores of bodies such as are penetrated by these vapours. Between the actually mechanical method of taking solid matter into the air, such as when waves are agitated by the wind, and the purely chemical method, such as when a liquid is transformed into a vapour by heat, there must be many intermediate stages. These stages are required, apparently, as we can scarcely imagine pure gases undergoing transformation similar to bodies in a putrefactive state; and we are not prepared with a theory by which diseases will be communicated without the agency of bodies in such a state of change—one of the oldest of theories and one promising to live long. Besides, the fact of a substance being found capable of being absorbed by metallic salts, and containing carbon with nitrogen in such a large amount, leads us to believe that bodies not very far removed from the substances decomposed are found in the vapours, and, if not far removed, capable of undergoing transformations so as to become farther removed, and by such transformations exercising their special influence.

MICROSCOPICAL SECTION.

October 21, 1861.

Professor WILLIAMSON in the Chair.

The following gentlemen were elected members of the Section:—Mr. Murray Gladstone, Mr. John Whalley, Mr. William Henry Heys, Dr. William Roberts, and Dr. Thomas Alcock.

The SECRETARY presented sixty specimens of soundings received since the last Session, from the commanders of various steamers and sailing vessels, amongst which were a number from the south coast of Ireland, banks of Newfoundland, coast of Nantucket, United States, north coast of Brazil, &c. The Secretary was requested to write a letter of thanks from the Section to each contributor.

The CHAIRMAN remarked that these specimens deserved the best attention of the Section, not only on account of their intrinsic interest, but to show the contributors that their kindness in preserving the soundings for the Section was fully appreciated.

Mr. DALE offered, with the assistance of the Secretary, to prepare the material, by separation from the tallow, &c., and Mr. Nevill, Mr. Heys, and several other gentlemen offered their assistance in mounting, examination, and reporting to the Section.

The CHAIRMAN observed that the method he employed in the preliminary examination of similar specimens, when freed from tallow and dried, was to stir the mass in a vessel of water, when most of the organic forms rose to the surface, in consequence of containing small quantities of air; the creamings off the top of the liquid would be found to contain sufficient indications whether the specimens deserved further attention.

The SECRETARY read a letter from Mr. Joseph Sidebotham, accompanying a specimen and a drawing of the Aulacodiscus formosus, showing the four projecting knobs or handles visible upon that diatom. Mr. Sidebotham states that in all drawings hitherto published, these protuberances appear like simple elevations or bosses, nor could they be seen otherwise until the binocular microscope revealed their true shape. Mr. Dancer first called his attention to this peculiarity.

Mr. CROMPTON exhibited, and presented to the members, specimens or capillary tubes used by him to collect and preserve fluids for microscopical examinations for medical purposes. Mr. Crompton has used such tubes for more

than a year, and has preserved specimens of blood, urine, &c., which by any other method would have spoiled. The main feature consists in hermetically sealing the tubes after the introduction of the fluid, by holding their ends alternately in the flame of a candle or lamp until the glass melts, and the orifice closes; the tubes may be about three-fourths filled by capillary attraction or immersion, and care must be taken not to allow the fluid to approach the hot end of the tube whilst being sealed. The Edinburgh vaccine tubes answer the purpose well; they may be about three inches long, and a number of them may be carried in a small pocket case at all times ready to be filled. When required for examination, the tube is broken, and the enclosed fluid placed under the microscope.

The SECRETARY exhibited a specimen of the compound salt of magnesia and copper, prepared by Mr. Thomas Davies, of Warrington. Doubts have been expressed if it were a true compound salt, or a mere mechanical mixture, Mr. Dale, who at the Secretary's request had prepared some of the salt, undertook to report further upon it. Some forms of crystal were exhibited, producing novel effects by polarised light.

Mr. LYNDEN exhibited a fine specimen of copper ore, with jasper, from Cornwall, the colour of which, when illuminated with the Lieberkühn, was very gorgeous.

Mr. LATHAM exhibited the ovary of a flea, and portions of the skin and teeth of the dogfish.

Mr. HEYS exhibited specimens, mounted specially for the Lieberkühn, of various seeds, hairs, and glands of plants; the tessellated spines on the *Symphytum asperillum*, or rough comfrey, and the ruby coloured oil glands of the *origanum onites*, glowing, like precious stones, with the reflected light from the above object, was very much admired. For certain classes of objects no illumination can compare with that of the Lieberkühn in the use of the binocular microscope, and side lights may be obtained, to some extent all around the object, by manipulation with the mirror.

ROYAL INSTITUTION OF GREAT BRITAIN.

General Monthly Meeting, Monday, November 4, 1861.

WILLIAM POLE, Esq., M.A., F.R.S., Treasurer and Vice-President, in the Chair.

The Rev. W. Wentworth H. A. Bowyer, M.A., was elected a Member of the Royal Institution. Alexander J. Ellis, Esq., was admitted a Member of the Royal Institution.

CHEMICAL SOCIETY.

Thursday, November 7, 1861.

Dr. A. W. HOFMANN, F.R.S., President, in the Chair.

MR. HARCOURT, of Oxford, read a paper *On the Action of Certain Gases on the Peroxides of Potassium and Sodium*, the method of preparing these bodies having already been published in the *CHEMICAL NEWS*. His original experiment was to act on potassium and sodium with air and oxygen gas, the metals having been previously filtered through linen, and cast in a glass tube. Under these circumstances sodium combined with two equivalents of oxygen and potassium with as many as four equivalents. The action of water on these oxides had been investigated. Some difficulties arose in conducting the experiments, on account of the powerful action of the oxides on the material of which the containing vessels were composed; glass, platinum, and silver being rapidly attacked at a high temperature, a compound of oxide of silver with oxide of the alkaline metal being produced when a silver vessel was employed; in the case of

tetroxide of potassium oxygen was evolved at the same time. These results were obtained by heating the peroxides in a silver boat, and passing over them a current of nitrogen gas, the boat being placed in a glass tube. The action of vapour of sulphur on the peroxides of these two metals was next investigated, when a rather unexpected result was obtained; instead of a sulphate being formed, as might have been anticipated, a sulphide was produced, together with sulphurous acid gas. By the action of iodine on the binoxide of sodium a substance containing sodium, oxygen, and iodine was produced, which was probably a mixture of iodide and iodate of the metal. Carbonic oxide passed over the binoxide of sodium gave rise to the formation of carbonate of soda; with tetroxide of potassium oxygen was evolved and carbonate formed. Carbonic acid also produced carbonate of the metal, oxygen being set free both from the peroxide of sodium and of potassium. With protoxide of nitrogen and binoxide of sodium a nitrite was formed, nitrogen being evolved; this gas, however, seemed to have no action on the tetroxide of potassium. The action of binoxide of nitrogen gave rise to the formation of nitrite of soda; but with the tetroxide of potassium red fumes were produced at the same time from the combination of the oxygen set free from the peroxide with the excess of binoxide of nitrogen; the apparatus was filled with nitrogen before commencing the experiment. The product in this case was a mixture of nitrite and nitrate of potash.

The PRESIDENT inquired whether the substances were heated in the last experiments?

Mr. HARCOURT replied that, although heat was employed in most cases, the experiments with the oxides of nitrogen were performed at the ordinary temperature.

The PRESIDENT then remarked on the difference between the deportment of the two metals, potassium and sodium, and asked whether there was any appearance of a binoxide of potassium being formed previously to the higher oxide?

Mr. HARCOURT concluded, from the colour of the compound at an intermediate stage, that a lower oxide was first formed: he endeavoured to verify this by determining the increase of weight, but could not obtain a constant result: he also thought there was a possibility of a tetroxide of sodium existing, but the temperature necessary for its production being higher than that required for the preparation of the potassium compound, the containing vessel was destroyed before this point of oxidation could be reached.

A Paper, by Mr. F. A. ABEL and Mr. F. FIELD, *On some of the Results of the Analysis of Commercial Copper*, was then read by Mr. ABEL. After referring to the importance of determining accurately the amounts of arsenic, antimony, bismuth, and lead present in different varieties of copper, he stated that the usual method recommended in books on analysis for the separation of arsenic and antimony from copper, namely, by digesting the mixed sulphides of the metals in sulphide of ammonium, was imperfect, for that as much as one per cent. of arsenic, and even twenty or thirty per cent. of tin, might be overlooked, on account of the power possessed by sulphide of copper of holding back the sulphides of these metals, and preventing them entering into solution, as had been pointed out by Mr. Bloxam; and that a better mode of proceeding was to add ammonia to the nitric acid solution of the copper, and then pass sulphuretted hydrogen into the liquid, for that in this case the sulphide of copper was precipitated, whilst the sulphides of arsenic and antimony were retained in solution by the sulphide of ammonium formed at the same time. The following process was recommended for the analysis of commercial copper:—Two hundred grains of the copper were to be dissolved in nitric acid and filtered from slag, &c., a small quantity of a solution of nitrate of lead, and excess of ammonia, and a little carbonate of ammonia

were then to be added, the liquid filtered, and the precipitate washed and digested in oxalic acid solution, filtered from oxalate of lead and of bismuth; a stream of sulphuretted hydrogen passed through the filtrate after the addition of excess of ammonia, and after filtration from a little sulphide of lead, a slight excess of hydrochloric acid added, and the whole to be warmed for a few hours. The presence or absence of antimony could be ascertained with certainty from the colour of the precipitate obtained. For the detection of bismuth and lead, another portion of two hundred grains was to be dissolved in nitric acid, and a little phosphate of soda and excess of ammonia to be added, and the precipitated phosphates of bismuth and lead collected, washed, and dissolved in dilute nitric acid, tartaric acid and excess of ammonia added, and sulphuretted hydrogen passed through the liquid, the precipitated sulphides collected and dissolved in nitric acid, and the solution digested with hydrated oxide of copper, when oxide of bismuth would be precipitated, the oxide of lead remaining in solution; the liquid having been filtered, chromate of potash was to be added to the filtrate when chromate of lead would be precipitated. Iron if present would be found with the bismuth, and could easily be separated. The accuracy of these processes was tested in the following manner:—A hundred grains of electrotype copper were dissolved and treated as directed, and no yellow precipitate was obtained from the ultimate solution on addition of hydrochloric acid. To another portion of a hundred grains one-tenth of a grain of arsenic was added, and this could be easily detected, and even one-hundredth of a grain of arsenic in a hundred grains of copper could be discovered by the process. Antimony in equally small quantities could be separated in the same manner. The process was also applicable for the examination of commercial hydrochloric acid for arsenic. The presence of very minute quantities of arsenic could also be discovered in copper by the magnesia test; the copper to be dissolved in nitric acid, excess of ammonia added and then sulphate of magnesia, and the whole allowed to stand. By this process two hundredths of a grain of arsenic in a hundred grains of copper were detected. The method recommended by Mr. Field, namely, to separate the main bulk of the copper from the solution of the sample in nitric acid by adding oxalic acid; and to examine the filtrate for the other metals, was also efficacious, although the plan before given was preferable. Mr. Abel then alluded to the method recommended by Dr. Odling for examining copper for small quantities of arsenic, which was to act on the finely-divided copper, with cold hydrochloric acid exposing the solution to the air, and after the solution of the copper, to distil the liquid, when chloride of arsenic would be found in the distillate if arsenic were present in the copper. Mr. Abel pointed out that if heat were applied before all the copper was dissolved, arseniatted hydrogen was evolved, but that mere traces of arsenic could be detected in copper by placing a piece of the metal in hydrochloric acid and adding a small quantity of sesquichloride of iron or chlorate of potash, and leaving it to stand for some time, when a grey coating would be formed on the surface of the copper if it contained arsenic; also that a very small quantity of bismuth might be detected by adding to the solution of bismuth a little lead-salt and then iodide of potassium, when, if bismuth be present, a brown or crimson-coloured precipitate would be obtained instead of the bright yellow iodide of lead; in this manner one twenty-five-hundred-thousandth of a grain of bismuth could be detected. To apply this test to a specimen of copper, nitrate of lead is added to the nitric acid solution of the metal, and then ammonia and carbonate of ammonia, the precipitate dissolved in acetic acid and iodide of potassium added. Mr. Abel then said that bismuth had been discovered in a great number of copper samples, and that arsenic and silver were almost always present, but that antimony was very rarely found,

and lead never, excepting in manufactured copper, such as copper bolt and sheet, and that in these cases it was intentionally added.

Dr. ODLING said that he had observed that hydrogen was evolved during the solution of copper in hydrochloric acid, and that he preferred the use of chlorate of potash, to avoid the delay of exposing the liquid to the air.

Mr. RILEY remarked that he had found phosphorus and antimony in copper.

The PRESIDENT inquired whether the amount of bismuth in the new bronze coinage had been determined.

Mr. ABEL replied that the quantity was very minute, and had not, therefore, been determined.

Mr. DE LA RUE alluded to some analyses of commercial copper which Dr. Hugo Muller had performed; it appeared that Dr. Muller had found both sulphur and gold.

Mr. ABEL said that he had succeeded in detecting gold occasionally.

The PRESIDENT stated that the Duc de Leuchtenberg had examined the refuse obtained from copper in the electrotype factories, and had found a great number of metallic impurities.

Dr. MULLER had also examined these residues, but did not find so many constituents; the residue contained a considerable quantity of nickel.

Mr. DE LA RUE remarked that in the electrotype process the bulk of the copper was separated in a very satisfactory manner from the impurities, so that the residue might be easily examined.

Mr. F. FIELD then read a short paper *On the Occurrence of Bismuth in Various Descriptions of Copper Ore obtained from Different Parts of the World*; a table of analyses of various samples was exhibited, from which it appeared that, as a general rule, bismuth was only found in the sulphuretted ores of copper.

Mr. NEWLANDS then exhibited a tabular arrangement of organic bodies including substances of every variety of composition, the advantage of which arrangement was that the relations between the composition of different substances was more easily discovered.

It was announced that at the next Meeting Dr. Benoe Jones would read a paper *On the Occurrence of Phosphate of Lime in the Human Urine*.

NOTICES OF BOOKS.

Report of the Committee on the Decay of the Stone of the New Palace at Westminster.

CONCLUDING NOTICE.

IN February of the present year, the St. Stephen's Porch of the Westminster Palace was placed under the experimental treatment of Mr. Charles Hibble, of Grafton Street, Fitzroy Square. The process employed by that gentleman was one which had received application to the private dwelling-house, No. 49, Bedford Square, early in the year 1854; and which, according to a certificate from the owner of the house had completely remedied the symptoms of decay at that time apparent in the Bath and Portland stones employed in its construction. The materials entering into the composition of Mr. Hibble's preparation are turpentine, linseed oil, Dorking lime ground, silicate of lead, and burnt coppers; the two last named being used as driers. It is preferred to apply two coats, the second containing rather more linseed oil. Another ingredient is alluded to by Mr. Hibble as having been employed upon St. Stephen's Porch; this consists of a final coating of silver sand sifted through a dredger.

The general character of the process is described and admitted to be not very different from that presented by an ordinary coating of paint. This consideration, taken together with the complex nature of the preservative

agent, has not tended towards the establishment of a very favourable opinion in regard to this process.

Another mode of applying linseed oil is communicated by Mr. P. C. Hardwick, Architect of the Bank of England. This consists in the employment of an oleaginous preparation made as follows:—Linseed oil, one gallon; soft soap, one pound to a pound and a-half; alum, half a pound; and litharge, quarter of a pound. The oil, soap, and alum, without the addition of any water, are all to be heated together in an iron pot; when melted, the litharge, wrapped in a canvas bag, is introduced, and the whole kept constantly stirred for a period of three hours over a slow fire. After cooling the mixture will have the consistency of thin paint, and may be preserved in air-tight tin canisters for a considerable time. There is a tendency, it is said, for the alum to subside to the bottom of the vessels on longer keeping; this deposit would, however, with a greater degree of probability, consist of the heavy sulphate of lead produced as the result of the action of litharge upon the sulphates in the alum.

The circumstance which induced Mr. Hardwick to advocate a trial of this composition in connexion with the Houses of Parliament, was the observation that very satisfactory results had followed the application of the same material, some twenty years since, by the foreman of works, to brick walls and buildings, including stone window-sills, on a farm in Sussex, which by this means were rendered far less absorbent of moisture, and had resisted, in a remarkable manner, the disintegrating effects of the atmosphere.

A process somewhat similar to the last is described by Mr. G. R. Burnell, O.E., a member of the Committee, as having been practised more than twenty years ago upon the dome of the Pantheon and other stone buildings in Paris. Three parts of olive oil are mixed with one of beeswax and a little litharge, generally in the proportion of about one-tenth by weight. These ingredients having been thoroughly incorporated, with the assistance of heat, are, while still hot, laid on to the surface of the stone, which should previously have been, if possible, both dried and warmed by the employment of fire-baskets or braziers. Such treatment necessarily facilitates the absorption of the oily fluid into the pores of the stone, which is thus rendered much more repellant of moisture. The instance of its application abroad, already cited, is reported to have been undoubtedly successful, so far as regards the protection of the stone; but it must be remembered that the introduction of the oxide of lead as an ingredient in this and the preceding compositions must always be attended with the disadvantages arising from the gradual darkening, by sulphuretted hydrogen in the air, of the stone surfaces so coated.

Of the processes upon which the Committee have taken evidence, the last remaining to be described is the mode of treatment patented by Mr. William Hutchinson, of St. John Street Road, for the induration of stone by immersing the blocks in melted coal-tar, pitch, or common resin, using the first or second of these substances accordingly as the colour is desired to be black or white. Some very satisfactory and interesting particulars are furnished in regard to the preparation of paving slabs, tomb-stones, window-sills, fonts, and other small architectural ornaments. These, although they may have been cut from very inferior stone, will, after their induration, have become completely impervious to water; the frost cannot affect them, and their external surfaces, originally soft, (as with the Tunbridge Wells stone, for instance,) may now have a high polish imparted to them.

In considering the applicability of such a process to stones in position, obstacles are presented which appear almost insurmountable. The high temperature required for the fusion of these substances, and the necessity for drying and even warming the stone before being brought into contact with the melted material, are difficulties which

render it imperative to introduce some modification into the working of the process before it can be deemed applicable to the treatment of the Houses of Parliament.

With the present notice we conclude our critical review of the several processes which have been submitted and received the consideration of the Decay of Stone Committee; and in the ten pages of our journal devoted to the discussion of these proposals it has been our object to institute comparisons in regard to their probable efficacy as determined by an examination of the scientific principle which lies at the basis of each invention. Many of these suggestions appear worthy of extended investigation, and we entirely concur in the recommendation conveyed in the report, to the effect that it would be desirable to proceed further to a practical trial of at least one process in each class, and thus endeavour to secure without delay the opportunity of watching closely the relative capabilities of the several methods proposed, that some really valuable information may at length be acquired as to the best means of guarding our great national edifice from the ultimate and more serious consequences of confirmed decay. An important condition of such competitive trial must consist in requiring each candidate to give an explanatory statement in reference to the materials themselves and the mode in which they are about to be employed; for not only would such a course furnish a guarantee that no permanent injury to the face of the building would be likely to result from the treatment proposed, but such data must always be available for the information of those who may hereafter be entrusted with the supervision of the experimental results, and when, at the expiration of a sufficiently long interval of time, the comparative efficacy of these applications shall have been prominently developed.

Elementary Treatise on Physics, Experimental and Applied.

By Professor A. GANOT. Translated and Edited from the Ninth Edition, by E. ATKINSON, Ph.D., F.C.S. Part I. London: H. Baillière, 219, Regent Street.

This promises to be one of the best works on physics in the English language. The arrangement is methodical, the definitions clear and concisely expressed, and the illustrations profuse and to the purpose. The work is to be issued in monthly parts. The first part contains:—Book I. On Matter, Force, and Motion. Book II. On Gravitation and Molecular Attraction. Book III. On Liquids. It will be completed in about twelve parts and if the succeeding issues are equal to the one now before us, it will form a most valuable volume for the student.

NOTICES OF PATENTS.

564. *Cementation.* W. B. NEWTON, Chancery Lane, London. A communication. Dated March 5, 1861.

THE Patentee claims for the cementation of iron or steel the employment of alkaline carbonates, and, particularly, also the carbonates of baryta and of strontia, mixed with a certain quantity of powdered charcoal. This process is applicable whether the operation of the furnace be continuous or intermittent, and whether the treatment be conducted in reverberatory or in ordinary furnaces, or in steel melting crucibles. Under the head of carbonates, above-mentioned, the Patentee urges a distinct claim for the exclusive use of any other salts, which, under the influence of heat and the conjoint action of carbon and the nitrogen of the atmosphere, are capable of producing cyanides, which are in reality the agents operating in the conversion of iron into steel, and constitute the special basis of this process of cementation.

The theoretical views advanced in support of the patent claim are strictly in accordance with recent investigations

upon this subject; and the important assistance rendered in the process of cementation by the employment of cyanides has long been recognised.

576. *Preserving Animal or Vegetable Matters.* A. G. BRADÉ, Paris. A communication. Dated March 7, 1861. (Not proceeded with.)

THE specification describes a new method of preserving meat, fish, vegetables, &c., by virtue of the principle of endosmosis and exosmosis, which is carried out with the further object of abstracting a portion of the water naturally contained in these substances, and of keeping the said matters, thus partially dehydrated, sufficiently protected from actual contact with the atmospheric air. The mode of accomplishing this consists in immersing the articles of food to be preserved, for a moderate length of time only, in saline solutions or other suitable materials; they are then withdrawn and allowed to become surface-dry.

It is well known that methods of preservation have been based upon the exclusion of air by coatings of albumen and gelatin, and that if the meats have previously been soaked in brine their protection is rendered more certain; but there is, on the other hand, much evidence to show that beyond a comparatively short interval of time no reliance can be placed upon stores of provisions so treated.

577. *Preserving the Aroma and other Properties of Coffee and Cocoa from the Effects of the Atmosphere.* W. PRIDDING, Borough Road, Southwark. Dated March 7, 1861. (Not proceeded with.)

THE inventor first forms a packet of roasted and ground coffee or cocoa, or compound of either, by placing it in a mould and subjecting it to great pressure, so as to cause its particles to cohere. And after extracting such packet from the mould, he immerses it in a solution of gelatin, to which a small quantity of alcohol or methylated spirits of wine has been previously added. He then allows the prepared solution of gelatine to evaporate to dryness, and if needful dips the block of coffee or cocoa a second time, to increase the thickness of the coating, which should not be less than about one-thirty-second part of an inch. It is further recommended to surround the prepared coffee with an envelope of thin sheet gelatin, and to close the overlapping seams of this covering by moistening them with the solution of gelatin previously employed, and allowing them to dry. The objects secured by this proceeding are, firstly, the hermetically sealing the packet of coffee or cocoa as a means of preserving the aroma, and guarding against the deteriorating influence of the atmosphere; and, secondly, the gelatin will itself serve a useful purpose by refining the decoction in the process of making the coffee.

This invention is in principle very similar to that last described, and the remarks made in reference to the preceding specification are equally applicable to that now before us. It is questionable whether any external protective covering would be completely effectual in preventing decomposition or deterioration of ready-ground coffee on keeping for any length of time. The safer course, when coffee must be preserved, would surely be to roast and grind the berries as required.

583. *Improvements in the Mode of, and Apparatus used in the Process of Fermentation.* G. HOLLANDS, Rochester. Dated March 7, 1861. (Not proceeded with.)

THIS invention relates to certain arrangements whereby the beer in the fermenting vats or tanks is made always to stand at the same level, which is such that the yeast continually forming in the process of fermentation may conveniently deliver itself into the yeast vat placed below, and so avoid the necessity for frequent skimming and consequently exposing the surface of the beer.

The plan of making the yeast overflow into a separate vessel placed contiguous to the fermenting tun is one frequently adopted in breweries.

593. *Apparatus for Obtaining Gas.* J. JACOB, Golden Square, London. A communication. Dated March 9, 1861.

THIS invention refers to the production and employment of hydrogen gas. The specific claims of the patentee are as follows:—Firstly, the process of manufacturing hydrogen gas, on a scale suited for commercial purposes, by passing steam over red-hot iron filings or borings, or iron crushed, ground, or pulverised, as described. Secondly, the heating of the retorts or generators with the hydrogen gas produced in the process, combined with air and under pressure. Thirdly, the employment of the hydrogen gas and air, combined and under pressure, for heating and lighting purposes, and for all metallurgical operations.

The preparation of hydrogen according to a plan which is found described in all Manuals of Chemistry, and which has so frequently been put into operation on a moderate scale, can scarcely, we think, constitute a valid claim for a patent right; neither does the application to heating, lighting, and metallurgical purposes possess much originality; as proof to the contrary may be mentioned—Deville's gas furnace, the oxy-hydrogen blowpipe, the lime light, and the gas soldering apparatus frequently employed in uniting the plates of sheet lead used in the construction of sulphuric acid chambers.

Grants of Provisional Protection for Six Months.

1773. Thomas Cobley, Meerholz, Hesse, Germany, "An improved process for preserving and indurating timber, wood, and other vegetable matters, and for rendering the same unflammable."—Petition recorded July 15, 1861.

2312. Frederic Maitland Ransome and Ernest Leslie Ransome, Ipswich, Suffolk, "Improvements in treating stone, bricks, and other surfaces, and in the manufacture of filters."—Petition recorded September 16, 1861.

2335. James Cane Coombe and James Wright, Bridge Street, Blackfriars, London, "Improvements in the manufacture of stones, bricks, tiles, slabs, statuary, and such like materials, and in the method of and means for cleansing and indurating the same when employed in buildings or for building purposes, and for preserving them from atmospheric and other influences."

2348. Theophilus Redwood, Montague Street, Russell Square, London, "An improvement in the manufacture of paper."—Petition recorded September 19, 1861.

2489. Ebenezer Partridge, Patent Axle Works, Smethwick, Staffordshire, "Improvements in hardening iron and steel, and a composition or substance to be employed therein."

2525. Thomas Tidmarsh, Dorking, Surrey, "An improved artificial manure."

2526. John Schwartz, Osborne Street, Whitechapel, London, "An improvement in the manufacture of sugar."—Petition recorded October 9, 1861.

2551. Edward Thomas Hughes, Chancery Lane, London, "An improved compound to prevent the incrustation and sediments of calcareous matters in boilers."—A communication from Ernest Louis Pichio, Paris.

Invention Protected for Six Months by the Deposit of Complete Specification.

2580. William Smith, Salisbury Street, Adelphi, London, "Improvements in the apparatus for and method of increasing the illuminating power of gas."—A communication from Hugo Carstanjen, Cologne, Prussia.—Deposited and recorded October 17, 1861.

Notices to Proceed.

1543. Thomas Gray, Hill's Cottages, Union Road, Wandsworth, Surrey, "An improved method of bleaching coloured rags and vegetable fibres."

1547. Thomas Mellodew, Oldham, Charles William Kesselmeyer, Manchester, and John Mayo Worrall, Salford, Lancashire, "Improvements in dyeing and printing velvets, velveteens, and other fabrics with floated threads."—Petitions recorded June 17, 1861.

2526. John Schwartz, Osborne Street, Whitechapel, London, "An improvement in the manufacture of sugar."—Petition recorded October 9, 1861.

CORRESPONDENCE.

Specific Gravity and Solid Contents.

To the Editor of the CHEMICAL NEWS.

SIR,—The following samples of milk having come under my notice last week, I quote them as furnishing a very pertinent answer in the negative to your correspondent "Specific Gravity," in the CHEMICAL NEWS for August 10, 1861, who asks if it be possible in quantitative analysis of water, having obtained the specific gravity, to calculate from thence the total amount of solid contents in any volume, and *vice versa*, knowing these could you not calculate the specific gravity. However easily this may be done in the case of wine as shown, it is far otherwise with water, for instance, there being no rule of proportion to guide us. No. 1 sample of milk was of sp. gr. 1.031, and yielded per thousand grains, of dried curd 78 grains, of serum 82 grains; total, 160. No. 2 sample of milk was of sp. gr. 1.033! and yielded per thousand grains, of dried curd 50½ grains, of serum 49 grains; total 99½.—I am, &c.

JOHN HORSLEY.

Estimation of Alcohol in Wine.

To the Editor of the CHEMICAL NEWS.

SIR,—I accept, of course, without reserve, as absolute truth, the statement of Mr. Horsley in your last Number; and therefore can only point out as remarkable coincidences, that on the page (36 of Vol. I.) which contains the extract from Mulder, there is also a communication from Mr. Horsley; and further, that on page 108, and immediately following the first letter on the subject, there is again a communication from Mr. Horsley.

I am, &c. SUUM CUIQUE.

November 12, 1861.

MISCELLANEOUS.

Chemical Society.—The next meeting of this Society will take place on Thursday the 21st inst., at Burlington House. Chair taken at 8 p.m. A paper will be read by Dr. Bence Jones "On the Occurrence of Phosphate of Lime in the Human Urine."

Phosphorus in Oysters.—Mr. James Bruce writes to the *Mechanics' Magazine* as follows:—"Some little time ago, in the opening of an oyster, I was much surprised at observing a very bright flame about the centre, which, when examined a little closer, I found to proceed actually from a small quantity of real phosphorus; the quantity I could not ascertain, but there might be half a grain. I took it from off the oyster and immersed it in water, till I had an opportunity of observing it more minutely; which, when I did, I found it to be in every respect the same as the phosphorus obtained from bones, urine, &c. The oyster itself was perfectly alive and fresh, so that it could not happen from any decomposition of the constituent parts of the shell by the aid of putrefaction; it must have proceeded from some other source. As I have never read of this substance being furnished perfectly ready-formed in Nature, any elucidation of the subject from some of your numerous readers will be very acceptable. The circumstances, moreover, may serve to explain the accidental

poisoning from eating particular kinds of shell-fish; and again, those *cas rares* of 'spontaneous combustion' in the human body, as phosphorus, when heated, requires no more air than what is contained in the stomach to produce combustion which would perforate that organ."

Aureolin.—Under this name Messrs. Winsor and Newton are about to introduce to the public a magnificent yellow pigment, which supplies a desideratum hitherto in vain sought for by artists. It is the nearest approach to a perfect yellow in existence, and more closely resembles the purity of the prismatic spectrum than any other artificial colour. It is transparent, has great brilliancy and richness, both pure and in combination, and is very permanent, being entirely unaffected by exposure to sulphuretted hydrogen and other atmospheric impurities, or to the direct rays of the sun during an entire summer. Aureolin, with ultramarine and madder red, completes the triad of brilliant, permanent, and transparent primitive colours. We hope shortly to be able to lay before our readers a full description of the chemical as well as the artistic properties of this beautiful colour.

Relative Bulks of Snow and Ice.—In Sir Henry James' "Tables for the Reduction of Meteorological Observations," 1860, p. 30, are given the results of some experiments upon the conversion of ice and snow into water, conducted at Kingston, Canada West, February 28, 1854, in the presence of Lieutenant-Colonel Gordon, Lieutenant Farrell, Lieutenant the Hon. J. Bury, and Lieutenant Cox, Royal Engineers. In one experiment, one foot cube of ice was taken so pure and transparent that manuscript was perfectly legible through it. The average temperature of the block was zero, and its weight was 57 lbs. The lump was allowed to melt at a temperature of 52° Fahr., when it yielded ½ths of a cubic foot of water, or 1512 cubic inches, weighing 54 lbs. A corresponding experiment was then performed with virgin snow. One foot cube (1728 inches) was taken up soon after falling, and compressed into a cubic vessel; its average temperature was 19.5° Fahr., and its weight 63 lbs. 14 ozs. When melted at a temperature of 52°, it yielded 1728½ cubic inches of water, weighing 1021 ozs., or 63 lbs. 13 ozs. These remarkable results require verification in this country.

Conglomerate Copper.—The *Mining Gazette* (Lake Superior) states that in the Portage Lake District there is a belt of conglomerate which is remarkably rich in copper. It says:—"It is a singular fact, that nearly every belt of conglomerate within the trap range in this district, and we believe in the country, has more or less copper. This is a very marked and singular mineralogical feature, and offers a field for investigation for the geologist and mineralogist, of very great interest. The conglomerate has not been, as yet, in any country, put down as a metalliferous-bearing rock, and many questions arose on the discovery of the masses in the conglomerate belt at the Minnesota mine. It is still a question whether it may yet be more than the receptacle of mineral deposited from the real metalliferous rocks adjacent, which is a point of vast importance to the true knowledge of our veins, and is of great value in determining the value of a conglomerate lode, by a close examination of the overlying belts of trap, since the lodes are always over the conglomerate and are never under them."

ANSWERS TO CORRESPONDENTS.

Beta.—The fullest account of the various combinations of phosphoric acid and lime is to be met with in Gmelin's Chemistry, Vol. 3.
Hair Wash.—G. B.—The following has been recommended for this purpose:—Tinct. Cantharid 4 drms.; sp. ammon. co., 3 drms.; glycerin, 2 drms.; ol. rosmari, 20 minims, dissolved in sp. rectif. 1 drim; rose water or distilled water 7 ounces.

THE CHEMICAL NEWS.

VOL. IV. No. 103.—November 23, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Preparation of Urea from Ferrocyanide of Potassium, by M. CAREY LEA.

In the preparation of urea by Wöhler's method the amount obtained always falls short of that which should theoretically be produced. Having remarked that even when the operation is carefully performed with Liebig's proportions, the lixiviate always contains undecomposed cyanide of potassium, it occurred to me to try the effect of a more thorough oxidation, and the result proved very favourable. The following was the course adopted:—

Of roasted ferrocyanide of potassium 850 grammes are mixed with 318 grammes of thoroughly dried carbonate of potash in grains, and fused in an iron vessel. When decomposition is complete, the vessel is a little cooled, and 1900 grammes of red lead are to be added, not all at once, but 300 to 400 grammes at a time, with intervals of ten minutes, stirring and keeping up heat enough to retain the whole in a state of fusion.

After the last addition of red lead the mixture is suffered to remain half an hour on the fire to complete the reaction. Heat is applied in all for about four hours. In this way the cyanide of potassium becomes thoroughly oxidised. The process is concluded in the usual manner, and 500 grammes of urea are obtained. No particular precaution is necessary during the fusion, or even the lixiviation with cold water, but in evaporating the solutions the greatest care must be taken to carry off the vapours.—*Am. Journ. of Science*, No. xc.

On the Estimation of Tellurium, by M. H. ROSE.

Precipitation of Tellurium by Sulphurous and Phosphorous Acids.—Sulphurous acid is the best re-agent to precipitate tellurium from solutions of tellurous acid. For the precipitation to be rapid and complete, it is necessary to strongly acidulate the liquid with hydrochloric acid. Phosphorous acid, or rather phosphatic acid obtained by the slow oxidation of phosphorus in the air, can replace sulphurous acid in the precipitation of solutions of tellurous acid; the precipitation, however, is slower, even in the presence of an excess of hydrochloric acid, and the solution must be evaporated down several times successively in a water-bath to a syrupy consistency, and re-dissolved in water, to be sure of precipitating all the tellurium.

Reduction of Oxygen Compounds of Tellurium by Cyanide of Potassium.—M. Oppenheim, has already pointed out the employment of cyanide of potassium in fusion (*Journal für Praktische Chemie*,

vol. lxxi., p. 279), to reduce tellurium, and to separate this body from selenium. This process does not yield the theoretical quantity of tellurium, there is always more or less loss.¹ The most correct results are obtained by fusing the oxygen compound of tellurium at a gentle heat with cyanide of potassium in a long-necked flask, through which is passed a current of hydrogen. After fusing for about ten minutes, let it cool in the current of hydrogen, then fill up the flask with water, and turn it upside down in a beaker full of the same liquid. The telluride of potassium dissolves, colouring the water red; it decomposes in the air, depositing tellurium; this decomposition is hastened by passing a current of air through the solution. The deposited tellurium is collected on a filter, and the liquid, after addition of hydrochloric acid, is treated with sulphurous acid to reduce a small quantity of tellurous acid which may have been formed.

A boiling solution of cyanide of potassium only dissolves very little metallic tellurium. Telluro-cyanide of potassium is formed in this case. From this solution tellurium is precipitated by hydrochloric acid, but not by atmospheric air.

Reduction of Oxygen Compounds of Tellurium by Fusion with Alkaline Carbonates.—Instead of cyanide of potassium, alkaline carbonates can be employed, and especially a mixture of equal equivalents of carbonate of potassa and soda mixed with an equal weight of chloride of potassium or sodium in powder. The fusion cannot be performed in a flask, on account of the high temperature which is necessary; a porcelain crucible must be used, through which a current of hydrogen is passed.

This process gives less accurate results than the one in which cyanide of potassium is used.

Estimation of Tellurium in the State of Sulphide.—Tellurous acid is completely precipitated by sulphuretted hydrogen from its hydrochloric, or even nitric solution in the state of sulphide of tellurium; this may be dried at 100° and weighed: the nitric solution must contain no nitrous acid. If there is doubt respecting the purity of the sulphide of tellurium, it must be dissolved by chlorine in the presence of a small quantity of water, taking care to pass the chlorine which has traversed the sulphide of tellurium through a second vessel of water to avoid the loss of a small quantity of tellurium. When the solution is complete, dilute the liquid and heat it, to drive off chlorine; add hydrochloric acid to make sure that there is no fresh disengagement of chlorine, for all the telluric acid which has been formed must be completely transformed into tellurous acid before the tellurium is precipitated by sulphurous acid.

Estimation of Tellurous Acid by Simple Evaporation.—When tellurous acid is dissolved in other acids, especially nitric, it can be estimated by evaporating it on the water-bath and heating the dry mass in a platinum crucible to about 200° or 270°.

¹ M. Oppenheim has since modified his process by employing a boiling solution of cyanide of potassium, and using it simply to separate tellurium, selenium, and sulphur.—See CHEMICAL NEWS, Vol. III. p. 34.

If there is hydrochloric acid in solution, nitric acid must be added. Sulphuric acid may be completely driven off; but it is necessary to keep the mixture for some instants at the temperature of zinc.

Separation of Tellurium from Other Metals.—Tellurium cannot be separated by means of sulphide of ammonium from the metals whose sulphides are insoluble in this re-agent; tellurium always remains in the precipitated sulphides. It is necessary to employ in these cases the precipitation of tellurium from its solutions by sulphuric acid or the volatility of chloride of tellurium.

Separation of Tellurium from Sulphur.—Fuse the mixture of sulphur and tellurium with cyanide of potassium in an atmosphere of hydrogen; dissolve in water: tellurium soon separates from this solution in a metallic state. To estimate the sulphur, add potash to the liquid, and pass a current of chlorine through it, which transforms all the sulphur into sulphuric acid.

When the weight of the tellurium only in a mixture is wanted, oxidise with hydrochloric acid and chlorate of potash, heat the solution to drive off the chlorine, transform the telluric into tellurous acid by hydrochloric acid, and decompose the tellurous acid by sulphurous acid.—*Poggendorff's Annalen*, vol. cxii. p. 307.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 259.)

Method of determining the Proportional Relation between Silver and the Chlorides.—I will now describe how I proceed with the determination of the proportional relation between silver and the chlorides. For this purpose I followed Gay-Lussac's process for the assay of silver by the wet way. This method has been already employed by M. de Marignac and by M. Pelouze. Almost all the last memoir of M. Dumas on the equivalents rests upon the method by the wet way.

In performing this I weighed either in air or in *vacuo* a certain quantity of chloride, and calculated and weighed the quantity of silver necessary, according to Prout's law, to precipitate the chlorine contained in the chloride. I took for silver 108; for chloride of potassium $39 + 35.5 = 74.5$; for chloride of sodium, $23 + 35.5 = 58.5$; and for chloride of ammonium, $4 + 14 + 35.5 = 53.5$. The excess of silver in the liquid after double decomposition, and the amount of this excess would yield me the relation between the proportional numbers of silver and the chlorides.

This is how I proceeded with the operation:—The silver, after having been heated to redness in a crucible of the same metal and properly cooled in the air, is weighed and introduced into a white glass flask, with the stopper well ground in with emery, and having very thick sides to enable it to withstand an internal pressure of at least ten atmospheres. I then pour upon the metal ten times its weight of pure nitric acid at 25° Baumé. I put in the stopper and fix it solidly in its place by the aid of strong cord. I then surround the flask with wire gauze, and place it in a bath where its temperature is raised to 45° or 50° C. At the end of twenty-four or thirty-six hours all the silver is dissolved without any trace of gas developing itself, and consequently without anything escaping from the flask. Indeed, the binoxide of nitrogen, as fast as it is produced, reduces the nitric

acid to the state of nitrous or hyponitric acids, which at this temperature remain perfectly dissolved in the large excess of nitric acid employed. If the temperature of the bath does not exceed 50° , there is really nothing to fear. I have made, under these conditions, and without any accident taking place, upwards of a hundred solutions of silver in a closed flask, employing from three to fifty grammes of silver at a time. Twice only, the temperature of the bath rising much too high, two flasks which were immersed in it yielded to the internal pressure and produced a rather violent explosion. I have had recourse to the solution of silver in a close vessel, because I have come to the conclusion that the method of solution in open vessels such as is adopted in the assay rooms of the Mint causes a slight loss of silver. I have seen assayers of proved skill obtain in three assays of the same silver results presenting greater differences than the errors which are due to the admirable method of Gay-Lussac. The same fact presented itself to me when I wished to familiarise myself with this process to prepare myself for the determinations. Moreover, the constant presence of silver in the washing-waters of the gas, arising from the solution of the metal during my syntheses of the nitrate and chloride of silver, sufficiently point out the necessity of effecting this solution either in a close vessel or in apparatus in which the escaping gases may be washed. I ought to add, however, that the loss experienced upon a gramme of silver never affects the accuracy of the assay of silver in the Mint, owing to the much more considerable and unavoidable error of experiment.

The solution of the metal having been effected, and the flask well cooled, I introduce such an amount of pure water that, with the acid already added, the total weight of the liquid becomes at the minimum thirty-five times, and at the maximum fifty times the weight of the silver employed. I now carry the flask into a dark room lighted with gas. After having conveniently inclined it, I introduce into it a tube, sealed up at one end, fixed to a rod of platinum, and containing the chloride weighed with the greatest accuracy which my balances will admit of. I then drop the chloride into the solution of silver, and wash the tube out several times with water, so as not to lose the traces of chloride which may remain adhering to it. After having tightly closed the flasks, and enveloped it in caoutchouc, I shake it until the liquid, at first turbid, becomes perfectly clear. I then proceed to the assay of the silver remaining unprecipitated. For this purpose, I have prepared with the greatest care the decimal solutions of salt and silver, such as are employed in the laboratories of the Mint.

On the other hand, I have myself manufactured pipettes, tubes which, when empty in a vertical position, would furnish me ten, five, four, three, two, one, or half a cubic centimètre of decimal solution. I also constructed bourettes, which, when placed in a vertical position, would deliver drops exactly equal to the twentieth of a cubic centimètre. The bourette itself and the tubes of half, one, and two centimètres, are subdivided into twentieths of a cubic centimètre. The small bourette consists of a graduated tube of four or five millimètres, internal diameter, drawn out to a point so as to expose an opening of about one millimètre in diameter, and melted to a larger open tube, the aperture of which is covered with a piece of vulcanised caoutchouc folded over the side of the tube, and more or less strongly bound to it, according as it is wished to deliver the drops more or less rapidly: my bourettes would only give five or six per minute.

If there is hydrochloric acid in solution, nitric acid must be added. Sulphuric acid may be completely driven off; but it is necessary to keep the mixture for some instants at the temperature of zinc.

Separation of Tellurium from Other Metals.

—Tellurium cannot be separated by means of sulphide of ammonium from the metals whose sulphides are insoluble in this re-agent; tellurium always remains in the precipitated sulphides. It is necessary to employ in these cases the precipitation of tellurium from its solutions by sulphuric acid or the volatility of chloride of tellurium.

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When the weight of the tellurium only in a mixture is wanted, oxidise with hydrochloric acid and chlorate of potash, heat the solution to drive off the chlorine, transform the telluric into tellurous acid by hydrochloric acid, and decompose the tellurous acid by sulphurous acid.—*Poggendorff's Annalen*, vol. cxii. p. 307.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 259.)

Method of determining the Proportional Relation between Silver and the Chlorides.—I will now describe how I proceed with the determination of the proportional relation between silver and the chlorides. For this purpose I followed Gay-Lussac's process for the assay of silver by the wet way. This method has been already employed by M. de Marignac and by M. Pelouze. Almost all the last memoir of M. Dumas on the equivalents rests upon the method by the wet way.

In performing this I weighed either in air or *in vacuo* a certain quantity of chloride, and calculated and weighed the quantity of silver necessary, according to Prout's law, to precipitate the chlorine contained in the chloride. I took for silver 108; for chloride of potassium $39 + 35.5 = 74.5$; for chloride of sodium, $23 + 35.5 = 58.5$; and for chloride of ammonium, $4 + 14 + 35.5 = 53.5$. The excess of silver in the liquid after double decomposition, and the amount of this excess would yield me the relation between the proportional numbers of silver and the chlorides.

This is how I proceeded with the operation:—The silver, after having been heated to redness in a crucible of the same metal and properly cooled in the air, is weighed and introduced into a white glass flask, with the stopper well ground in with emery, and having very thick sides to enable it to withstand an internal pressure of at least ten atmospheres. I then pour upon the metal ten times its weight of pure nitric acid at 25° Baumé. I put in the stopper and fix it solidly in its place by the aid of strong cord. I then surround the flask with wire gauze, and place it in a bath where its temperature is raised to 45° or 50° C. At the end of twenty-four or thirty-six hours all the silver is dissolved without any trace of gas developing itself, and consequently without anything escaping from the flask. Indeed, the binoxide of nitrogen, as fast as it is produced, reduces the nitric

acid to the state of nitrous or hyponitric acids, which at this temperature remain perfectly dissolved in the large excess of nitric acid employed. If the temperature of the bath does not exceed 50° , there is really nothing to fear. I have made, under these conditions, and without any accident taking place, upwards of a hundred solutions of silver in a closed flask, employing from three to fifty grammes of silver at a time. Twice only, the temperature of the bath rising much too high, two flasks which were immersed in it yielded to the internal pressure and produced a rather violent explosion. I have had recourse to the solution of silver in a close vessel, because I have come to the conclusion that the method of solution in open vessels such as is adopted in the assay rooms of the Mint causes a slight loss of silver. I have seen assayers of proved skill obtain in three assays of the same silver results presenting greater differences than the errors which are due to the admirable method of Gay-Lussac. The same fact presented itself to me when I wished to familiarise myself with this process to prepare myself for the determinations. Moreover, the constant presence of silver in the washing-waters of the gas, arising from the solution of the metal during my syntheses of the nitrate and chloride of silver, sufficiently point out the necessity of effecting this solution either in a close vessel or in apparatus in which the escaping gases may be washed. I ought to add, however, that the loss experienced upon a gramme of silver never affects the accuracy of the assay of silver in the Mint, owing to the much more considerable and unavoidable error of experiment.

The solution of the metal having been effected, and the flask well cooled, I introduce such an amount of pure water that, with the acid already added, the total weight of the liquid becomes at the minimum thirty-five times, and at the maximum fifty times the weight of the silver employed. I now carry the flask into a dark room lighted with gas. After having conveniently inclined it, I introduce into it a tube, sealed up at one end, fixed to a rod of platinum, and containing the chloride weighed with the greatest accuracy which my balances will admit of. I then drop the chloride into the solution of silver, and wash the tube out several times with water, so as not to lose the traces of chloride which may remain adhering to it. After having tightly closed the flasks, and enveloped it in caoutchouc, I shake it until the liquid, at first turbid, becomes perfectly clear. I then proceed to the assay of the silver remaining unprecipitated. For this purpose, I have prepared with the greatest care the decimal solutions of salt and silver, such as are employed in the laboratories of the Mint.

On the other hand, I have myself manufactured pipettes, tubes which, when empty in a vertical position, would furnish me ten, five, four, three, two, one, or half a cubic centimetre of decimal solution. I also constructed bourettes, which, when placed in a vertical position, would deliver drops exactly equal to the twentieth of a cubic centimetre. The bourette itself and the tubes of half, one, and two centimetres, are subdivided into twentieths of a cubic centimetre. The small bourette consists of a graduated tube of four or five millimetres, internal diameter, drawn out to a point so as to expose an opening of about one millimetre in diameter, and melted to a larger open tube, the aperture of which is covered with a piece of vulcanised caoutchouc folded over the side of the tube, and more or less strongly bound to it, according as it is wished to deliver the drops more or less rapidly: my bourettes would only give five or six per minute.

It is absolutely necessary to hold the bourettes in a vertical position, for the same instrument which furnishes with the greatest accuracy twenty drops per cubic centimetre only delivers seventeen or eighteen when it is inclined 45° ; in a position of 10° or 15° no more than sixteen or seventeen drops are required to make a centimetre.

To perform the assay, I had besides the following arrangement:—In a long narrow box, the anterior portion of which was furnished with yellow glass, and the posterior portion lighted by a gas lamp, I arranged a perfectly spherical glass globe containing a saturated solution of the double chromate of potash and soda, so as to concentrate the rays and obtain a cone of yellow light. I then place the flask containing the assay in such a position that the surface of the liquid may be traversed by the beam of yellow light. To make an observation, I place myself so as to make an angle of 60° with the luminous ray traversing the flask. When this artifice is employed, a liquid containing two milligrammes of silver per litre produces a yellow opaque and dull precipitate of chloride of silver, when half a centimetre of decimal solution is allowed to fall carefully upon its surface; when the quantity of silver is reduced to one milligramme, the precipitate of chloride is yellow, opaque, and brilliant; when the liquid is diminished in strength, so that it contains no more than the twentieth of a milligramme of silver per litre, there is still produced an appreciable cloud upon the addition of a corresponding quantity of decimal solution. It is only necessary to wait sufficiently long, without touching the flask, to be certain of it. Nevertheless, in these assays I have only worked to tenths of a milligramme.

In all my experiments I have continued adding decimal salt solution as long as I have seen a cloud produced on the surface of the liquid after standing fifteen minutes. When I have arrived at the extreme limit, and have just passed it, I add five centimètres of decimal solution of silver. After well shaking, I neutralise three-quarters of the excess of silver, so as to obtain immediately upon agitation a clear liquid approaching very nearly the extreme limit. When there was a difference between the results of two assays, a difference that has never exceeded two or three tenths of a milligramme, I always took the minimum result.

Those who have made many assays by the wet way will have noticed that the interior sides of a flask in which they have shaken for some time chloride of silver

produced by successive precipitation, becomes covered with a kind of varnish of chloride, appears greasy, and so loses its transparency. To obviate this inconvenience when it occurs, I remove, by means of a pipette, a portion of the liquid after it has been agitated and become limpid by standing sufficiently, and transfer it to a bottle having parallel sides of crystal, and ascertain in this second bottle the presence of silver or salt in excess. The liquid in this bottle was always added to the first whenever it was found necessary to continue the operation.

The assay presents another difficulty which may lead one greatly in error when not forewarned of it.

A liquid from which there has been precipitated by a saline solution almost all the silver, but which still contains between one and two milligrammes of silver per litre, is precipitated equally upon the addition of a decimal solution of silver or of salt. In this case, however, there is a very evident difference in the resulting turbidity. The precipitate occasioned by the decimal salt solution to the assay containing one or two milligrammes of silver in the state of nitrate is always opaque, yellow, and brilliant; whilst the precipitate formed upon the addition of nitrate of silver to the same liquid is whiteish and translucent. I account for this anomaly by the slight solubility of chloride of silver in the alkaline nitrate in solution, and which precipitates in the presence of a silver solution richer than itself. I have, notwithstanding, always added decimal salt solution until precipitation ceases.

Such is the method of assay which I have adopted for all my determinations by means of double decomposition.

I now return to the chloride of potassium.

I have made two series of determinations: the first was performed nearly three years before the second. I separate the results because three of the five experiments composing the first series were performed with silver prepared by Gay-Lussac's process. The silver of Number I. underwent one treatment, that of Number II. two treatments, and that of Number III. three treatments. This silver is not pure, as I have already said; but, as I carefully ascertained the standard of that which I employed, by comparing it with metal obtained by electrolysis, or by Liebig's process, I was able to submit its weight to the necessary corrections. These three determinations show better than any argument how much impurity in the metal can affect the result. It is only with this object that I publish them. The nineteen

Proportional Relation between Silver and Chloride of Potassium.

First Series.

Numerical Order.	Weight of the Chloride of Potassium in air.	Weight of the Chloride of Potassium reduced to a vacuum.	Weight of the Silver in air.	Weight of the Silver reduced to a vacuum.	Weight of the Silver after allowing for impurities.	Weight of the excess of Silver after double decomposition.	Weight of Chloride of Potassium corresponding to the excess of Silver.	100,000 parts of Silver correspond to Chloride of Potassium:	
								A. Calculated from the weight of the crude metal.	B. Calculated from the actual weight of the metal.
I.	grammes. 7.4461	grammes. 7.450	grammes. 10.8004	grammes. 10.800	grammes. 10.7976	grammes. 0.0169	grammes. 0.0117	69.0898	69.105
II.	7.4461	7.450	10.8004	10.800	10.7983	0.0173	0.0120	69.093	69.104
III.	7.4461	7.450	10.8004	10.800	10.79914	0.0182	0.0126	69.098	69.103
IV.	7.4461	7.450	10.8004	10.800	10.800	0.0191	0.0132	69.104	69.104
V.	7.4461	7.450	10.8004	10.800	10.800	0.0189	0.0131	69.102	69.102
Mean.								69.1036	

Proportional Relation between Silver and Chloride of Potassium.

Second Series.

Numerical Order.	Weight of the Chloride of Potassium in air.	Weight of the Chloride of Potassium reduced to a vacuum.	Weight of the Silver in air.	Weight of the Silver reduced to a vacuum.	Weight of the excess of Silver after double decomposition.	Weight of Chloride of Potassium corresponding to the excess of Silver.	100,000 parts of Silver corresponding to Chloride of Potassium.
I.	grammes. 2.09325	grammes. 2.09450	grammes. 3.03618	grammes. 3.03636	grammes. 0.0055	grammes. 0.0038	69.105
II.	1.98565	1.98685	2.88010	2.88028	0.0050	0.0034	69.099
III.	4.4759	4.4786	6.4920	6.49240	0.0115	0.0080	69.107
IV.	4.7013	4.7041	6.81890	6.81930	0.0121	0.0084	69.103
V.	7.08925	7.09352	10.28258	10.28320	0.0185	0.0128	69.103
VI.	8.8827	8.88805	12.8839	12.8847	0.0230	0.0159	69.105
VII.	9.65585	9.66160	14.0053	14.00615	0.0245	0.0168	69.104
VIII.	8.0961	8.10100	11.74300	11.74373 ⁽¹⁾	0.0202	0.0139	69.099
IX.	22.3365	22.3500	32.3980	32.4000	0.0572	0.0395	69.1034
IXa.	4.1246	4.12706	5.98248	5.98285	0.0106	0.0074	69.104
IXb.	3.2632	3.26516	4.73310	4.73338	0.0083	0.0057	69.103
IXc.	5.8849	5.88845	8.53575	8.53627	0.0149	0.0103	69.102
IXd.	5.1692	5.17232	7.49765	7.49812	0.0131	0.0091	69.104
X.	3.83185	3.83415	5.55790	5.55823	0.0097	0.0067	69.104
Xa.	3.8425	3.84461	5.5730	5.5734	0.0099	0.0069	69.105
Xb.	4.1910	4.19350	6.07880	6.0792	0.0108	0.0074	69.103
Xc.	5.17925	5.18237	7.51220	7.51267	0.0130	0.0090	69.101
Xd.	3.58975	3.59191	5.20670	5.20700	0.0092	0.0064	69.105
XI.	7.2525	7.25682	10.51930	10.51995	0.0194	0.0128	69.103
						Mean . . .	69.103

(1) Thus in the original paper: it is evidently a misprint for 11.74373.

experiments composing the second series are more than sufficient to establish the true proportional relation between silver and chloride of potassium.

The last two determinations of the first series were performed with silver obtained, in Number IV. with divided phosphorus and an excess of pure nitrate of silver, and in Number V. by the decomposition of acetate of silver crystallised a great many times, and, moreover, prepared with silver previously purified.

The chloride of potassium employed in the five determinations of the first series was the same, and was derived from the analyses of chlorate of potash, which I describe further on. The traces of silica and alumina were separated from it by the means already given. The solution was neutral, and yielded a liquid of absolute limpidity.

The silver employed for the determinations of the second series has been prepared either by electrolysis or by Liebig's process, or by the decomposition of pure chloride with carbonate of soda. All the silver was compared with the purest which I had been able to procure by the methods already indicated. The metal employed in experiment Number VII. was prepared by M. Liebig.

TECHNICAL CHEMISTRY.

On the Occurrence of Gold in Wales,
by T. A. READWIN, Esq.¹

I SHALL confine my observations in this paper to an area of about twenty square miles, situated north of the

¹ Read before the British Association.

turnpike road leading from Dolgelly to Barmouth. Professor Ramsay has so ably described the geology of this district, in a communication to the Geological Society of London in 1854, entitled "The Geology of the Gold-bearing Districts of Merionethshire," that I shall merely refer to its geology for the information of those present who have not perused the communication referred to. The Dolgelly district is bounded by the river Mawddach, the great Merioneth anticlinal range, and the little river Camlan. In this district are found the Cambrians, overlaid by the lower Silurian lingulas. The Cambrian rocks are coarse greenish-grey grits, and the lingula flags are arenaceous slaty beds, interstratified with courses of sandstone. Calcareous and greenstone dykes frequently penetrate both the Cambrian and the Silurian rocks. The metalliferous products are chiefly argentiferous galena, copper pyrites, blende, manganese, and mundic, associated frequently with gold. According to Sir R. Murchison, the most usual position of gold is in quartzose veinstones that traverse altered palaeozoic slates, frequently near their junction with eruptive rocks, whether of igneous or of aqueous origin. The stratified rocks of the highest antiquity, such as the oldest gneiss or quartz rocks, have seldom borne gold; but the sedimentary accumulations which followed, or the Silurian, Devonian, and carboniferous (particularly the first of these three), have been the deposits which, in the tracts where they have undergone a metamorphosis or change of structure, by the influence of igneous agency or other causes, have been the chief sources whence gold has been derived. After referring to the opinion of Professor Ramsay that gold in the Ural Mountains, Australia, &c., occurred in rocks of a similar age and character, Mr. Readwin continued: Sir R. Murchison's statement is

singularly corroborated by the position of the quartzose vein in the Clogau Mine, distinguished as the "gold lode," which traverses altered palaeozoic slates, near the junction of an eruptive bar of porphyritic greenstone; and the same law appears to obtain, also, with respect to all gold-bearing quartzose veins of the Dolgelly district, upon the ores of which I have made a very large number of experiments during the past eight years. There are in this district about twenty localities in which gold has been discovered visible, in quartz or associated, more or less with galena, blende, copper pyrites, telluric-bismuth, carbonate of lime, schist, baryta, iron pyrites, &c. By far the richest discoveries of gold have been made at the Dol-y-frwynog, Prince of Wales, Cambrian, and the Clogau mines. Gold has also been found in the "marine drift" by the Hon. F. Walpole, Sir Augustus Webster, myself, and others, a piece of which is now on the table. Mr. Arthur Dean, in a paper read before the British Association in 1844, stated that a complete system of auriferous veins exists throughout the whole of the Snowdonian or Lower Silurian formations of North Wales. Upon the faith of this, much money was spent at the Cwmheisian mines, and very little gold obtained by smelting operations, for reasons which are now not very difficult to understand. Much money was also spent about ten years afterwards at the same place, after setting the most eminent assayers to work to prove the truth of Mr. Dean's statement, in erecting machinery, which produced even less gold by amalgamation than the former method; although it was then held as an axiom, that gold always exists in a metallic state, that mercury has always an affinity for gold, and, therefore, wherever gold is present in minerals mercury will necessarily dissolve it. In this instance, however, this did not prove the case, and the result of operations upon 150 tons came at length to be considered as an enigmatical failure, as the following extracts from the experiments made at the time will show. [Here followed a detailed account of experiment No. 7, made on $4\frac{1}{2}$ tons of metalliferous minerals, which was triturated in 42 lbs. of mercury.] Ten pounds of this, on distillation, gave 70 grains residual metal, containing 184 grains of gold. A qualitative analysis of this residual metal gave gold, silver, lead, bismuth, zinc, arsenic, and also traces of copper and iron. The distilled mercury contained traces only of zinc and arsenic. It had been proved before the experiment, and also since, that the $4\frac{1}{2}$ tons of mineral contained several ounces of this "royal metal," but the quicksilver neglected it for associates of less dignity, though intrinsically of more real utility. This, however, was not the result expected. At the Dol-y-frwynog mine, about two years afterwards, Sir Charles Price operated similarly upon several tons of material, but with the same provoking failure as before. At present the Clogau mine is the most interesting and profitable. It stands at an elevation of 1000 feet above the level of the sea. The "Saint David's," or "Gold Lode," is the most remarkable feature. After giving some interesting descriptions of this mine, the author desired to notice especially that this lode is at the junction of the Cambrian sandstones and the lingula flags of the Lower Silurian rocks. A quantity of what was called "poor copper ore;" was raised from this lode and sold many years since; but in 1854 the refuse of this "poor copper ore" was examined, and indications of "native gold in considerable quantities" were found. Some of this refuse ore was put to the test, and in one instance, to his knowledge, 100 lbs. weight yielded 14½ ounces of gold. Many other experiments have been made by various persons with equal success, but owing

to the uncertainty of the operation of amalgamation, on the one hand, and the mines themselves being subject to two Chancery suits on the other, the general value of the lode, in bulk, has not until recently been determined. After some observations on the processes of assay,—which he did not think would give the approximate value of auriferous minerals, notwithstanding that the contrary had been asserted by many eminent men,—the author referred to a series of thirteen experiments, made by himself last autumn upon 112 lbs. of auriferous quartz from the Clogau Mine, duly prepared and sampled by Johnson and Son, of London. The whole quantity gave 25 oz. 16 dwts. 7 grains of amalgam, and of fine gold, 8 oz. 5 dwts. 19 grains. A gentleman of high standing in such matters had declared the value of the gold by assay to be 9½ per cwt., while he (Mr. Readwin) declared it to be 30½, so he (Mr. Readwin) happened to be right, as he had operated upon samples of 7½ lb. each, instead of the usual 400 grains. He would now state the result of actual working operations for gold at the Clogau Mine since the beginning of the year. This statement showed that 207 tons 8 cwt. of quartz gave 1314 ounces of fine gold; 3 tons of the best of this quartz gave no less than 9766 ounces of gold. If they added 56 ounces obtained from 5 tons in 1860, it showed a total quantity of 1370 ounces of gold from 212 tons of auriferous mineral, being at the rate of 6½ ounces per ton. This he believed was the first instance of a hundredweight of gold having been obtained from the Crown lands of this country, the value of which was 5300½. This "Royal mine" paid a royalty of $\frac{1}{16}$ th to the Crown. The cost of extraction had been very inconsiderable, and there was a probability of an equal yield of gold for some time to come. Mr. Readwin concluded by saying:—Seven years ago I determined, unwisely perhaps, never to leave this vexed question until I had mastered it. I have done so. Experiments are ended, works are the result of faith, and, after great wear of both mind and body, I have proved what many then doubted, and many more derided, that gold does really exist in the Dolgelly district, and that it can be extracted from the minerals in remunerative quantity. I am very glad to have had the opportunity of stating this to the British Association, under the presidency of the sagacious Director-General of the Geological Survey of Great Britain.

Mr. Jukes said he was sure they were all much obliged to Mr. Readwin for his very clear and lucid statement relative to the gold in North Wales. They had long known of its existence, but not that it existed in sufficient quantities to render its production remunerative.—Professor Smith, of Sydney, Australia, asked Mr. Readwin by what process he separated the gold from the quartz?—Mr. Readwin: Chiefly by amalgamation, taking care not to intrude any mineral that would have the effect of neutralising the mercury, as galena or copper pyrites, &c.—Professor Smith inquired whether some of the mercury was not lost in the process?—Mr. Readwin stated that some of the mercury was always lost; it became so finely divided that some of it was sure to be carried away.—Mr. Smith said he perceived that in old North Wales they were falling into the same difficulty as their friends in New South Wales. There were many things which would have a singular action upon mercury, and likely to injure its effect on the gold. The action they had was a chemical one, and, therefore, the gold-producers of New South Wales had depended, to a great extent, upon mechanical action exclusively, and generally they had been very successful. If anyone

could point out any means by which gold could be satisfactorily separated from the quartz, it would be a great boon. The mechanical means they employed in New South Wales would leave only half-an-ounce per ton, which was considered very fair. The production of gold in old North Wales, as detailed by Mr. Readwin, surprised him very much. The yield of gold in Victoria or New South Wales did not exceed one ounce per ton of quartz, and in many instances half-an-ounce was considered a fair yield. The digger of Australia would be extremely proud to get it in such quantities as six ounces per ton, as Mr. Readwin had stated the yield of the Dolgelly quartz to be. From three to four ounces per ton would be considered very good anywhere in the gold districts of Australia. In some small localities the yield had been as high as thirty ounces, forty ounces, or even more, per ton, but they were exceptional, and did not last long.—In answer to another question, Mr. Readwin stated that the actual cost of obtaining the 1300 ounces of gold was under 300*l*. He did not wish there to be any misunderstanding as to the yield of gold, for taking an average of the whole auriferous district of Dolgelly, he did not suppose the yield would be above half-an-ounce per ton.—A gentleman asked what was the cost per ton of crushing the quartz?—Mr. Readwin said that as they worked only by water power, the cost of crushing per ton would be less than half-a-crown.—Professor Smith expressed his surprise, as the cost per ton in Australia was calculated on an average at 1*l*. per ton. He referred to the fact that most of the firms there used the Cornish stampers for crushing the quartz, and that it answered its purpose well.—Mr. Readwin did not approve of the stampers, as he had found gold washed away after their use.

PHARMACY, TOXICOLOGY, &c.

On the Employment of Arseniuretted Alcohol for the Preservation of Objects of Natural History, especially Insects, by M. LEPRIEUR.

At the ordinary temperature pour 500 grammes of rectified alcohol, weighing at 94 per cent. on an excess of opaque arsenious acid. After eight hours of contact, during which time the liquid is frequently shaken, the alcohol is filtered. Upon evaporating 200 grammes of the solution in a platinum capsule over a water-bath, a residue is left weighing 0.03 grammes, or 0.15 grammes per kilogramme. On employing alcohol at 85 per cent. a residue is left of 0.102 grammes, or 0.51 grammes per kilogramme. If the alcohol at 85, and the arsenious acid, are kept in contact for four months, the residue weighs 1.38 grammes per kilogramme.

To be as energetic and complete as possible, the action of the arseniuretted alcohol should take place on tissues very recently deprived of life: it is, consequently indispensable in the case of insects, for example, that they should be immersed in it whilst still alive, or, better still, after having been asphyxiated with the vapour of ether, chloroform, or benzole. Some of them, indeed, must not be immersed living in even pure alcohol, for as soon as they touch the liquid they open their elytra and spread their wings as if they wished to fly away.

Almost all the coleoptera, the hemiptera, and even the orthoptera, can be plunged without danger into the arseniuretted alcohol, provided they are not left in longer

than ten or twelve hours; after this time they must be taken out of the alcohol and thrown upon blotting-paper, then they are to be pricked or cemented down until they have lost by evaporation the greater part of the liquid.

When the insects have been allowed to dry before being plunged into arseniuretted alcohol, the action of the latter is not so efficacious; when a collection is much attacked by larva, recourse must be had to an immersion for twelve hours.

Of all the methods which have been proposed to preserve collections, this is, according to M. Leprieur, the best. He has employed arseniuretted alcohol for twelve years, to the exclusion of all other means, and with uniformly good results.

It follows from M. Leprieur's experience that living insects, plunged into arseniuretted alcohol, and removed after remaining there for twelve hours only, increase in weight by about one-fourth; insects, therefore, preserve in their organs an amount of arsenious acid about equal to $\frac{1}{1000}$ of their own weight, a quantity quite sufficient to protect them from destruction by larvæ.

According to M. Leprieur, the employment of an alcoholic solution of arsenious acid possesses the advantage of preventing, in an almost absolute manner, the ravages of larvæ, whilst all other means, whatever be their efficiency, can have no other result than the actual destruction of the larvæ already present in a collection, without having any power to protect them from future ravages.—*Journal de Pharm. et de Chim.*, vol. xxxix. p. 435.

PHYSICAL SCIENCE.

On Phosphorescence, by M. DE REICHENBACH.

THE experiments of M. De Reichenbach tend to prove that phosphorescence is a usual consequence of all molecular phenomena, and not the result of combustion or oxidation. Mr. Phipson proved this last point some time ago, when he showed that dead fishes shine in the dark, even under water, and in the absence of oxygen.

According to M. De Reichenbach there is phosphorescence during fermentation or putrefaction, crystallisation, evaporation, condensation of vapours, the production of sound (vibrations therefore), and the fusion of ice; a considerable glow is remarked when a galvanic pile in activity, a block of ice in fusion, or a solution of sulphate of soda in the act of crystallising is observed in the dark.

The human body itself is not devoid of phosphorescence: in a healthy state it emits a yellow glow; when in ill-health the glow becomes red. The author considers that this observation may possibly be of use in diagnosis.

To perceive these phenomena the eye ought to have been previously rendered sensitive by remaining some hours in perfect darkness, and even then all eyes are not equally impressionable. But, if several persons unite in performing the experiment together, there will always be a certain number who are able to see the phenomena.

These facts of the production of light remind the author of observations published some time ago, by M. Wullner, according to which every molecular movement is accompanied by a disengagement of electricity.—*Poggendorff's Annalen*, vol. cxii., p. 459.

PROCEEDINGS OF SOCIETIES.

PHARMACEUTICAL SOCIETY.

Wednesday, November 6, 1861.

Mr. P. SQUIRE, President, in the Chair.

Professor BENTLEY exhibited a specimen of the Ordeal Root spoken of by M. du Chaillu in his book on Western Africa. It is there said to be in use among some African tribes as a test for witchcraft—an individual suspected of that crime being required to imbibe a strong infusion of the root. It is intensely poisonous, and if the individual dies he is supposed to have been guilty, but if, from any cause, he should survive the ordeal, he is considered innocent. The observed effects of the poison, and the character of the bark on the root, the Professor said, left no doubt on his mind that it was derived from a species of *strychnos*.

Mr. BROWN received the silver medal of the Society for a magnificent herbarium, consisting of 776 specimens of British plants. Several of the specimens were exhibited, and the preservation and mounting of the plants excited great admiration.

The PRESIDENT then read a Paper On Medicinal Extracts, taking Extract of Belladonna as an Example. The object of the Paper was to point out that the process for the preparation of the so-called green extracts, given in the London Pharmacopœia for 1851, was never followed by the manufacturers of those extracts on a large scale; and, further, that the plan which is adopted by the manufacturers is decidedly the better. Mr. Squire tested the different processes with some belladonna. The plant arrived at maturity in September this year, and was then cut, and immediately used. From this plant three extracts were made, one from the leaves only, as directed in the Pharmacopœia, another from all the soft parts (the plan adopted by the manufacturers), and a third from all the soft parts except the leaves. The first became mouldy in ten days. Both the others were good extracts, which kept well. To test the effects of the extracts, Mr. Squire placed specimens in the hands of some eminent ophthalmic surgeons—Mr. Wordsworth, Mr. White Cooper, and others—for trial, and they all reported that the extract made from the soft parts without the leaves acted most powerfully and rapidly in dilating the pupil of the eye. Mr. Squire concludes, therefore, that the most active and best keeping extract is made from the whole of the soft parts of the plant—a fact which applies to extract of henbane and other green extracts as well as belladonna. Another reason for adopting the manufacturers' plan was, that when the Pharmacopœia process was followed, there was a loss of one-third of the useful constituents of a plant.

Mr. DEANE said that *a priori* it was a serious mistake to reject the succulent stalks of plants like belladonna in making extracts, for the sap passed from the leaves to the stems, and there formed active substances. And this was the case, not only in succulent, but woody stems, and even roots, as had just been shown in the ordeal root. He had made extracts in all sorts of ways, evaporating *in vacuo*, by continuous boiling, and by evaporation at a low temperature; and he found that an extract evaporated without coagulating the albuminous matter kept without moulding if the plant was gathered in proper condition, but if the albuminous matter was coagulated, the extract became mouldy in a short time. An extract boiled down he also found to keep well and exhibit no tendency to mouldiness; and further, there was not the same disposition to ferment as was sometimes seen in extracts made at a low temperature. A great point in making a good extract was to make use of the plant at once before any heating took place. The effect of this heating was well

shown in the case of tea, the difference between black and green tea simply being, that in the former the leaves were allowed to heat before they were dried, and to make green tea the leaves were dried as soon as picked. Some change of the same sort took place in all plants, and the best and most elegant extracts were made from herbs gathered at the proper time and not allowed to heat. Plants in incipient inflorescence, or even one-third flowered, were unfit for extracts. The older they were, the less albuminous matter they contained.

Mr. HEATHFIELD said that the juice from stalks contained as much active matter as that from leaves. Fermentation, he said, took place in extracts where chlorophyll was absent, as in the extracts of broom, colchicum, and poppies; but when chlorophyll is present it never takes place. An extract from the stalks alone he found to liquefy soon. He agreed with Mr. Deane that the best extract was made from a plant of which the flowers were fully formed, and all the soft parts, leaves, flowers, seed-vessels, and branches were used. Such an extract would keep good an unlimited time. In 1831 he experimented with the juice of hemlock. He separated the albuminous matter and chlorophyll, and found that these in summer weather moulded very soon, while the juice kept well. In examining the juice to try and discover what had kept it, he found a considerable amount of chloride of sodium, which he believed was the cause. He had found no difference in the keeping properties of extracts made at different temperatures.

Mr. FRANCIS said he always used all the soft parts of the plants. The best extracts were made by evaporating in-doors to the consistence of treacle, and finishing out in the sun. In that manner an extract was obtained of the finest flavour, and which would not become mouldy. It was a mistake to make green extracts; they were brown when made in the sun.

Mr. HASELDEN said he had made extracts according to the Pharmacopœia process, which he found to keep very well, and have the true aroma, but which had not the consistence of extracts made from the whole plant. They absorbed moisture, and were unfit for making pills.

Mr. HOLLAND said that, as a large manufacturer, he had found it dangerous to allow the plants to ferment before making the extracts. He used all the soft parts that could be easily broken by the hand. He had noticed that the chlorophyll separated at one heat, and the albuminous matters at another.

Professor BENTLEY said that practical men, without an exception, never made extracts from the green leaves alone, but used all the parts of a plant in close contact with the leaves, which were essentially the same as leaves. The Pharmacopœia process could not be followed because an extract made from the leaves never kept, but became mouldy in a few weeks. Allowing the plants to heat was highly objectionable. The extract should be made immediately after the plants were gathered.

Mr. HILLS read some statistics. From about 1 cwt. of the belladonna plant, of which perhaps three-quarters were thrown away, he in the following years obtained the following amounts of extract:—1859, 1lb. 9½ozs.; 1860, 1lb. 10½ozs.; 1861, 2lbs. 4ozs.

The further discussion of this subject was adjourned until the next meeting.

Mr. FURLEY then exhibited his "Process for Rendering Pills Tasteless." He was provided with two saucers. Into one of these he put some powder composed of equal parts of tragacanth and sugar. The inside of the other he smeared with white of egg which had been well shaken and poured away from the froth. The pills were first placed in this latter saucer, which was then moved rapidly round and round, causing the pills to revolve within, by which means they in a second or two became covered all over with a film of albumen. They were then transferred to the other saucer and again made to revolve in the

powder, which now adhered to the albumen, and so the pills became coated with a mixture of tragacanth and sugar.¹

Mr. ATTFIELD afterwards exhibited, and explained the principle of, Casella's Minimum Thermometer, an account of which instrument has already appeared in the *CHEMICAL NEWS*.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, November 12, 1861.

Dr. R. ANGUS SMITH, *Vice-President, in the Chair.*

Mr. Thomas Coward was elected an ordinary member.

Mr. E. W. BINNEY said that some years since he read a paper before the Society, "On the Drift Deposits found near Blackpool," which was afterwards printed in Vol. X. (new series) of the *Memoirs*. In the gravel at Bispham, most probably the high level gravel of Mr. Prestwich, he found nineteen species of shells, all identical with those now found in the Irish Sea. He also stated that in the lowest bed of till there found, full of scored and striated rocks, he collected shells of the genera *turritella*, *buccinum*, *nassa*, *dentalium*, *nucula*, *cardium*, and *tellina*. Owing to such shells found in these deposits not having so arctic a character as those said to be found in other till beds, it has been supposed that they are of a more recent date than the glacial age of Forbes, or the pleistocene of Lyell. Professor King, of Queen's College, Galway, a geologist of high reputation, in his *Synoptical Table of British Aqueous Rock Groups*, &c., just published, classes the Blackpool fossils with post-pleistocene, as shell sands occurring just under deposits now forming around the shores of the British Islands, and immediately above the Devonshire raised beaches. The Blackpool gravels, near Bispham, are as good pleistocene as can be found in any of the high level gravels met with further inland, notwithstanding the fossils discovered in them, which are of the same kind, although not so numerous, as those met with in the gravels of Bowdon, Cheshire, and the sands of Haigh, Lancashire. He also said that the lowest bed of till seen at Blackpool, and containing the shells previously alluded to, had all the physical characters of the Scottish, Irish, and North of England iceberg and glacial drift, and had been subject to considerable elevations since its deposition.

Mr. R. D. DARBISHIRE said that he had lately found under undisturbed clay at a considerable elevation on the southerly slope of Great Orme's Head, a deposit of bones of different mammalia intermixed with shells of *mytilus*, *littorina* and *patella*. He hoped to lay the results of further observations before the Society on a future occasion. He supposed the deposit might be connected with the present or past existence of some "bone cave" in the limestone rock of the Head. He suggested, however, that possibly the bones and shells may have been the remnants of the cookery of former inhabitants of the district, and referred, in illustration, to the researches made amongst the *Kjokkenmøddings* on the coasts of Denmark.

Dr. JOULE, in reference to speculations on the thickness of the earth's crust, stated that he had some time ago received a letter from Professor Thomson, giving an account of the progress of investigations calculated to throw light on this interesting subject. Professor Thomson finds that

the equilibrium lunar tide in a solid glass globe (without mutual gravitation) of the same size as the earth is about five feet. Hence, from the phenomena of the actual tides of the ocean, it follows that the earth, as a whole, is more rigid than glass. The observations of Mallet, with experimental earthquakes, show that the earth's crust is many times less rigid than glass. Hence Professor Thomson infers that the earth, as a whole, is many times more rigid than the rocks and strata on its surface.

Dr. CRACE CALVERT stated that he wished to draw the attention of the manufacturing chemists of this district to a very simple and rapid method which had been devised by the eminent chemist M. Pelouze, Master of the Paris Mint, for determining the amount of sulphur existing in pyrites. He (Dr. Calvert) was induced to do so, believing that any process which would simplify the long and troublesome operations now followed to ascertain the value of this mineral would be useful to many members now present at this meeting. The process consists in mixing intimately together one part of pyrites, thoroughly pulverised in an agate mortar, with five parts of carbonate of soda, seven parts of chlorate of potash, and five parts of chloride of sodium, and placing the whole in an iron spoon, which is gradually carried to a dull red heat. The mass, when cold, is first washed with cold water and then with boiling water, until the whole of the soluble matter is removed; and this solution is tested with a standard solution of sulphuric acid. As 100 grains of carbonate of soda requires 92.45 of monohydrated sulphuric acid, or SO_3H_2 , it follows that the quantity of soda in the carbonate of soda employed will decrease in proportion to the quantity of sulphur from the pyrites converted into sulphuric acid, which will have neutralised a corresponding quantity of the soda in the carbonate.

This mode of assaying is so simple, that the Author states that he can determine, within one or one and a-half per cent., the value of a sample of pyrites, in the space of an hour's time.

M. Pelouze also states that by employing the following proportions of the same materials, the manufacturer can determine the amount of sulphur in burnt pyrites. Five parts of the latter substance are mixed intimately with five parts of pure carbonate of soda and five parts of chlorate of potash.

NOTICES OF PATENTS.

595. *Iron.* W. H. BUCKLAND, Barge Yard, Bucklersbury, London. Dated March 9, 1861.

This patent refers to the smelting or reduction of iron from the ore by the employment in the smelting furnace, crucible, or other receptacle, of a bath of melted cast-iron, into which the heated or semi-fused particles of ore may fall, and in consequence of being entirely surrounded by the fluid metal, become more quickly melted and reduced.

In the process of smelting iron in the ordinary blast furnace, a cistern is provided for the reception of the liquid iron, into which the metal runs freely, as produced, such arrangements having been devised as a means of maintaining a clear passage for the blast, and of permitting the ore not yet reduced to be surrounded by, and subject to, the uninterrupted action of the carbonic oxide gas generated in the hottest part of the furnace. According to the system now described, this potent action of the reducing gases cannot be exerted.

612. *Dyeing and Printing Textile Materials and Fabrics.* R. H. GRATRIX, Salford. Dated March 12, 1861. Not proceeded with.

This invention relates to the printing and dyeing of colours derived from aniline, nitro-benzol, naphthaline, cinchonine, and analogous substances. The inventor

[¹ Having given our readers the above information, we feel bound to add, by way of caution, that Her Majesty, "being willing to give encouragement to all arts and inventions which may be for the public good," has been graciously pleased to grant Royal Letters Patent to the inventor, his executors, administrators, and assigns, for the sole use, benefit, and advantage of the above invention. The Patentee, however, in consideration of a small sum, grants a licence for its use, and the apparatus required is ridiculously cheap.—*Ed. CHEMICAL NEWS.*]

produces a fast dye by passing cloth prepared with stannate of soda or other suitable mordant, through a thickened solution of tannin, and, after the ordinary dyeing, subjecting it to the fixing action of steam. After washing out the gummy matters, and drying, the material may then be dyed with any of the colours specified above. Or cloth or yarn, prepared as directed, may be passed through an acid or other solution of tannin, mixed with the colour intended to be used. Also, the mixed fabrics of cotton and worsted may be dyed by a more simple process than that commonly employed by passing the cloth through soap after the preparation of shumach or other such agent.

627. *Method of Fixing Colours in connexion with the Printing of Woven Fabrics and Yarns.* R. C. PATTISON, Daldorh House, near Catrine, Ayrshire; and A. M. PATTISON, Glasgow. Dated March 14, 1861. Not proceeded with.

For the purpose of fixing certain colours derived from tar products upon yarn and woven fabrics, the inventors employ tannin, in combination with an alkaline earth, in the process of printing.

The improvements suggested in this and the preceding specification are very similar. It is probable that the prior employment of materials containing tannin has operated in these instances adversely to the claim of novelty.

682. *Fixing Colouring Matters.* J. S. MILLER and T. P. MILLER, Springfield Works, Dalmarnock, North Britain. Dated March 18, 1861.

With the object of rendering more permanent such dyes as are procured from aniline and similar bases, the patentees entirely immerse the fabric in a solution of "the acetate of galls,"—i.e., tannin of nut galls dissolved with the addition of acetic acid. The material is then withdrawn and dried. In this condition it is ready for printing upon with a thickened colour of the following nature:—The aniline dye or other colouring matter obtained from similar sources is added to a solution of "tartro-acetate of stannate of soda or potash" in quantity sufficient to produce the desired shade of colour on the goods to be printed or dyed, and the mixture thickened by the addition either of gum, starch, dextrin, or other suitable farinaceous substances, employed in such proportion as will communicate to the colouring matter the requisite degree of consistence for printing on the fabric; or, in the case of dyeing by immersion, this last ingredient is altogether omitted. As the best means of fixing the colour, the printed goods are finally submitted to the action of steam in the ordinary way. For the preparation of the tin mordant specified above, the stannate of soda or potash is neutralised with tartaric acid and then slightly acidulated with acetic acid; or, instead of this combination, citric acid may take the place of acetic, or any other double salt of an alkaline stannate, with the potash or soda salt of other acids, may be substituted for those already described.

It is interesting to remark the close analogy, or even identity, between this specification and one of earlier date, No. 612, which was not proceeded with. In both processes stannate of soda and tannin are employed in combination with a free acid and a thickening agent.

693. *Producing Combinations of Certain Colours on Cotton Fabrics.* T. BROOKS, Sunnyside, near Rawtenstall. Dated March 20, 1861.

For the simultaneous production of aniline and madder colours in dyeing cotton goods, the inventor applies at once the mordants for these colours respectively, thereby rendering the fabric capable of taking up the various tints, or any combination of them, in the subsequent process of dyeing.

701. *Dyeing and Printing Textile Materials and Fabrics.* N. LLOYD and J. G. DALE, Church, near Accrington. Dated March 20, 1861.

As an improved method of fixing upon cotton and linen fabrics the several colours of the aniline class, described under the names of mauve, Magenta, roseine, &c., the patentees mix with any one of these tannin, and a thickening agent, such as gum, senegal, or other suitable mucilaginous material. The goods are printed with the above, then submitted to the steaming process, and finally passed through a boiling solution of tartarised antimony.

According to the terms of this specification the materials employed are the same as those just now described in Nos. 612 and 682, with the substitution, however, of antimony for tin in the mordant.

615. *Treating or Preparing Textile Materials and Fabrics.* A. PEEK, Manchester. Dated March 13, 1861.

In this invention it is the object of the patentee to increase the density of substance, and to improve the finished appearance of cotton and other vegetable textile fabrics and materials. In operating upon yarns or fabrics the first operation is preparatory, and consists in the removal of all gummy and greasy impurities, by submitting them to the ordinary process of "bucking," or washing. After such preliminary cleansing they are passed through a solution of silicate of soda or potash, employed alone, or in combination with a fatty or resinous soap. The yarns or fabrics thus prepared are dried by artificial heat or otherwise, and then aged or exposed to the atmosphere for as long a time as possible. In order now to fix the silica in the materials they are submitted to the action of any soluble salt, by surface padding or otherwise, which is capable of decomposing the alkaline silicate, and also the soap, when such has been employed, and by being rendered insoluble in this manner is precipitated within the substance of, or becomes incorporated with, the fibrous materials, yarns, or fabrics.

The impregnation of cotton and other fabrics, with the silicates of the alkalies and other compounds derived from the same, has frequently been practised for the purpose of rendering such materials less sensitive to the action of fire. These efforts have, however, been generally abandoned on account of the harshness and impoverished appearance of the fabric so treated, and with the exception of the method of preparation with soap and alum, applied to woollen material, no advantage beyond the single consideration to the manufacturer of rendering the goods more weighty seems likely to result from the mode of treatment here proposed.

619. *Silvering Glass, &c.* J. CUMBO, Great Portland Street, London. Dated March 13, 1861.

For the purpose of depositing a bright film of metallic silver upon glass and other services, the inventor employs ammonio-nitrate of silver, to which he adds a solution of Rochelle salt (tartrate of potash and soda). The silver is deposited by the action of this reducing agent at the ordinary temperature of the air, no external aid being required for the purpose of facilitating the production of the metallic film. The thin layer of silver so formed may be backed and strengthened by an electro-deposit of copper, or other cheap metal. This method of operating is also useful for the coating of small objects prior to electrotyping, which, being non-conductors of electricity, require to have a superficial conducting layer imparted to them. By commencing with a plate of glass having polished and dull surfaces, an ornamental device may be executed.

This process of depositing bright metallic silver upon glass was experimentally illustrated by Professor Faraday at one of his Friday evening lectures at the Royal Institution some years since. The brilliant lustre of the truly "silvered" surfaces was much admired.

Grants of Provisional Protection for Six Months.

2164. Hippolyte Lecaudey, Wellington Street, Strand, London, "Improved apparatus for succouring persons interred before life is extinct."—A communication from Raymond Malerille, Bordeaux, France.—Petition recorded August 30, 1861.

2323. George White, Pancras Lane, London, "Improvements in apparatus for filtering or purifying water or other liquids." A communication from Mr. Jean Antoine Victor Burg, Paris.—Petition recorded September 18, 1861.

2382. Thomas Davey, Tucking Mill, Camborne, Cornwall, "Improvements in the manufacture of safety fuses, parts of which are applicable to the manufacture of pipes and tubes of lead, and other soft metal, gutta-percha, caoutchouc, bitumen, and earthenware."—Petition recorded September 24, 1861.

2460. Edgar Breffit, King William Street, London, "Improvements in machinery employed in cutting hollow and solid corks."—Petitions recorded October 2, 1861.

2469. Robert John Ellershaw, Leeds, Yorkshire, "An improved machine for expressing oil."—Petition recorded October 3, 1861.

2482. Thomas Goulston Ghieslin, Hatton Garden, London, "Improvements in the treatment or preparation of certain foreign plants or vegetable substances, and of the application of the same to various useful purposes, for which horn, shell, whalebone, indurated leather, fish skin, ivory, bone, hard wood, and compounds of india-rubber or gutta-percha, have hitherto been employed."—Petition recorded October 4, 1861.

2515. Isham Baggs, Green Terrace, and James Thomas Parkes, Almorah Terrace, Middlesex, "Certain improvements in the manufacture and treatment of india-rubber and vulcanite as applied to various purposes."—Petitions recorded October 9, 1861.

2586. Charles de Groot, Brussels, Belgium, "An improved instrument for corking bottles and other vessels."

2597. Charles Denton Abel, Southampton Buildings, Chancery Lane, London, "Improvements in apparatus for the simultaneous manufacture of white lead and vinegar."—A communication from Robert Rowland, New York, U.S.

2607. James Webster, Birmingham, "Improvements in manufacturing oxygen gas and obtaining certain other products."

2699. Robert Mushet, Coleford, Gloucestershire, "A new or improved manufacture of titanite pig metal or alloy of titanium and iron."

2618. Frederick John Evans, Gasworks, Horseferry Road, Westminster, "An improved mode of, and apparatus for, carburetting gases for the purpose of illumination."

2620. Henry Lamplough, Holborn Hill, London, "Improvements in the means for igniting the wicks of candles and lampwicks."

2623. Josiah Timmis Smith, Barrow-in-Furness, Lancashire, "An improvement or improvements in collecting the inflammable gases evolved from blast furnaces."

2635. Herbert Frost, Manchester, "Improvements in apparatus for measuring fluids."

2637. Robert Mushet, Coleford, Gloucestershire, "An improvement or improvements in the manufacture of a certain metallic alloy."

2641. Richard Archibald Brooman, Fleet Street, London, "Improvements in reaping machines." A communication from Cyrus Hall McCormick.

Inventions Protected for Six Months by the Deposit of Complete Specifications.

2669. Enoch Chambers, Melbourne, Victoria, "The use of hydraulic power for the breaking, crushing, or pulverising of quartz, blue stone, or other stone or mineral of any description, and the use of a wrought iron lever or jaw in machines for crushing quartz or other mineral, and the use of steel teeth and steel shield pieces for the levers or

jaws in such machines."—Deposited and recorded October 25, 1861.

2683. Marc Antoine François Mennons, Rue de l'Ecliquier, Paris, "Improved apparatus for ascertaining the presence and degree or cessation of vitality in the human body and in animals of the higher class, applicable to the semeiosis of health, disease, and death."—A communication from Léon Victor Collongues, Grande Rue Passy Léz, Paris, France.—Deposited and recorded October 26, 1861.

Notices to Proceed.

1611. James Smith McArdle, Galway Ireland, "Improvements in treatment of sea-weeds for the purpose of obtaining therefrom certain valuable products."—Petitions recorded June 22, 1861.

1619. Jules Lafon, Bordeaux, Gironde, France, "Improvements in the production of chromo-lithographic impressions upon glass, porcelain, and other similar material."

1622. James Brown, Newport, Monmouthshire, "An improvement in the manufacture of iron."—Petitions recorded June 25, 1861.

1666. William Clark, Chancery Lane, London, "Improvements in the distillation of solid and liquid combustible matters."—A communication from M. Etienne Charles Zacharie Bouchard, Boulevard St. Martin, Paris.

1671. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in apparatus for manufacturing and bottling aerated liquids."—A communication from Messieurs Auguste Mondolot and Jean Alphonse Mondolot, Paris.

1741. Charles Cochrane, Ormesby Ironworks, Middlesborough-on-Tees, Yorkshire, "Improvements in treating gas in its passage from blast furnaces to the furnaces, stoves, boilers, or other heating apparatus where the gas is employed."

1783. Emile Georges Férmiér de la Provotais, Boulevard St. Martin, Paris, "Extracting the fibres from genista scoparia (broom), and their application to manufacturing paper and fabrics, and also treating the washing waters so as to obtain dyeing products therefrom."—Petition recorded July 15, 1861.

1905. Alfred Wood, Lewes, Sussex, "Improvements in refrigerating storehouses, and in other structures in which a low temperature is continually or periodically required."—Petitions recorded July 31, 1861.

2339. Edgar Breffit, King William Street, London, "An improved fuel."—Petition recorded September 19, 1861.

2460. Edgar Breffit, King William Street, London, "Improvements in machinery employed in cutting hollow and solid corks."

2669. Enoch Chambers, Melbourne, Victoria, "The use of hydraulic power for the breaking, crushing, or pulverising of quartz, blue stone, or other stone or mineral of any description, and the use of a wrought iron lever or jaw in machines for crushing quartz or any other mineral, and the use of steel teeth and steel shield-pieces for the levers or jaws in such machines."—Petition recorded October 25, 1861.

CORRESPONDENCE.*Economising the Waste Fumes from Vitriol Chambers.*

To the Editor of the CHEMICAL NEWS.

SIR,—In last week's Number there is a short account of a patent (514) for economising the waste fumes from vitriol chambers. A similar idea occurred to me three years ago. I found that although sulphuric acid does not dissolve metallic copper alone, it will do so if nitrous fumes are passed through it; I have made sulphate of copper in this way. The reason why the process was not largely carried out was, that the dilute acid only dissolves very little nitrous gas; strong acid dissolves large quantities;

but if the copper be placed in strong acid, and red fumes passed through the mixture, sulphate is formed, but in very small white-looking crystals, which are difficult to free from excess of acid economically.

The idea occurred to me then to treat pyrites containing both copper and iron, after burning, with vitriol and red fumes, and so to obtain a solution of sulphate of iron and of copper, by distilling gas liquor so as to pass the crude ammonia into the mixture; sulphate of ammonia and ammonio sulphate of copper remain, and the peroxide of iron precipitated may be re-dissolved in waste spirits of salts, to form a cheap persalt of iron for black, &c. dyeing. The mixed solutions I proposed to mix with copper scales (oxide) and evaporate to dryness, and heat in a retort to recover the ammonia, and leave pure sulphate of copper anhydrous. But the last operation has always failed from various causes already detailed; although ammonia is at first evolved, and sulphate of copper formed, afterwards other reactions take place. Although the idea of thus economising waste nitrous fumes failed with me, I am not aware that anyone has a priority on the score of originality. I may also add that I proposed an arrangement for the operation, which was perfect in its action. I arranged a series of Woulfe's receivers; in the first was a saturated solution of nitrous gas in strong vitriol, on adding water to which gradually the gas was evolved, and passed through the second; the gas, now reduced to nitric acid, was passed through the third, fourth, &c., with the addition of sufficient air to reconvert it into red nitrous gas. In the last receiver, strong vitriol absorbed the gas for another operation, by reversing the arrangement. Of course the receivers between the two extremes were charged with dilute acid and copper.—I am, &c.

JOHN S. BLOCKEY.

Sheepecar Chemical Works, Leeds, Nov. 14.

MISCELLANEOUS.

SCIENTIFIC NOMENCLATURE.

In reply to C. W. M., whose letter on this subject we reprinted from the *Athenæum* in our 101st Number, C. M. Q. writes as follows to the same paper:—

Although agreeing in the main with the remarks of your correspondent "C. W. M." on scientific nomenclature, I must take exception to that part of his letter in which he attacks the chemical philosophers. He sets out with the dictum, "Science is not for the idler." He must forgive me if I add a rider to it,—"Chemical nomenclature is not for the superficial." It is singular that he should find such fault with the only science amongst those which he aptly terms sciences of observation which possesses a perfectly intelligible and logical nomenclature. Chemical nomenclature, as founded by the immortal Lavoisier, is governed by a fixed principle, which ordains that every name shall express the composition of the substance for which it stands. An adept in chemical science knows at once the exact chemical composition of any body from the formation and termination of the name given to it. The word "methylethylsmylophenylammonium," which appears so unintelligible to "C. W. M.," is as easily remembered and understood by a chemist as "light blue, ruled, foolscap paper" would be by an ordinary individual. In this, chemical nomenclature stands in favourable contrast to that adopted by botanists. Such cacophonous epithets as "*Bogglesia Wigginsii*" simply afford the information that the plant so designated was discovered by the illustrious but mythical personages Boggles and Wiggins, without giving the smallest clue to its properties or character; but the term "Bichloride of rubidethyl," although just as imaginary, is the name of a substance, of certain composition, which may or may not be discovered some day. As to the synonyms possessed by a very few

chemical substances, it is the manufacturers and workmen who are the perpetrators of errors. Mere vendors cannot be posted up in the known composition of their wares. They are asked for spirits of salt, and they sell it. Ask them for hydrochloric acid, and they stare at you. As chemical science advances, names must necessarily change; but if "C. W. M." will examine the standard chemical works of the last twenty or thirty years, he will find but few alterations. The principal changes in this way are additions and not substitutions. When a new element or radical is discovered, a name is given to it descriptive either of its source or of some salient property. For instance, Bunsen has lately discovered two new metals, cesium and rubidium, by means of spectral analysis. Their names have been given to them from their forming blueish (*cesius*) and ruby lines in the spectrum. "C. W. M." is mistaken in saying that organic chemists call sulphates thionates. Sulphur compounds of the thionic series are *inorganic* substances, and are quite distinct in their composition from sulphates. Chlorhydric acid is simply the French form of hydrochloric acid; the difference in the two words being euphonic, and not theoretical, as in the long-abandoned name—muriatic acid. As to "hydrachlora," I fear it is only the invention of an enthusiast with a theory and nomenclature of his own. The proposed substitution for the ammonium compound must, I fancy, come into the same category; but, not having seen the article mentioned by "C. W. M.," it would be wrong to speak positively, "C. W. M." deserves the thanks of all scientific men for having stirred up the subject; and I trust it will be the means of calling forth many more remarks from abler pens than mine.

In conclusion, I beg to assure "C. W. M." that I make these explanations with perfect good humour, and only hope that he will be induced thereby to study chemical nomenclature a little more closely. He will soon see the beauty and intelligibility of such apparently absurd words as "ethylomethylpropylamine," and a host of others.

THE LIME LIGHT AT THE SOUTH FORELAND.

Five-and-thirty years ago Lieut. Drummond brought into notice the oxyhydrogen light, and applied it to a practical purpose. Having been appointed to conduct the Ordnance survey in Scotland and Ireland, he used this light in the focus of a parabolic reflector on lofty eminences, where the stations were usually placed, as it was of the greatest importance in those operations to have certain and determinate signals, which could be seen, under any circumstances as to weather, at great distances. Thus he succeeded in connecting the shores of England and Ireland, near Holyhead, a distance of 65 miles, and afterwards, in Scotland, the summit of Ben Lomond with that of Knock Laid, no less than 95 miles apart. It did not escape the comprehensive mind of Drummond to perceive that such a light, if capable of practical application, would be invaluable for lighthouse purposes. With the means he devised, however, he failed to obtain anything approaching practical command over the continuity of the light; and as a light that is liable to go out is inadmissible for lighthouse purposes, it is not surprising that it was condemned.

Since Drummond's time, until quite recently, the oxyhydrogen, or lime light, has been used only for the purposes of the microscope, or to produce scenic effects; not that the value of a light of such power and intensity has been lost sight of, but because all attempts to render it practically available in a commercial sense had failed.

The impossibility of turning it to a useful purpose seems to have so taken possession of the public mind, scientific as well as general, that although within the last two or three years, exhibition after exhibition, varying in duration from hours to months, have given the most incontestable proofs that with Bartable's apparatus the lime light can be burned as easily and certainly as a wax candle, yet, with a single but notable exception, not one eminent man of science has been found who has not scouted the idea of its practical utility. Upon almost every occasion of late, when our men of science have condescended to mention the lime light, it has been condemned by them as impracticable, and the idea of its applicability to any useful purposes contemptuously dismissed; whilst the assumption that it can be used for ordinary domestic purposes has met with most positive contradiction, interspersed, upon one or two occasions, with assertions as to the available sources and expense of oxygen gas, which, although valueless in themselves, are useful as indicating the amount and accuracy of information upon the subject possessed by some of those who have pronounced the severest condemnations of it. It has been the fashion in scientific circles to condemn the lime light, and there are few amongst those who have not compromised themselves more or less by decrying it; and experience will have taught us that from such we can expect but a tardy recognition of even a fact that is subversive of a long cherished dogma. This prejudice, which has seriously impeded the general introduction of the lime light, is traceable to its usual source—want of accurate information on the subject; for although the means were at hand, not one of those who ridiculed the idea that the lime light had been brought to a state of perfection, rendering its practical application to illuminating purposes easy and certain, took any trouble to make such an investigation as to the alleged fact as could justify the expression of any opinion at all. The public at large took the view of the *savans*, whose opinions were readily adopted and disseminated by those who are interested in the continuance of the present methods of producing artificial light; whilst the verdict of gas engineers, scientific advisers to Gas Companies, and other vested interests, upon the lime light was such as a notorious poacher might expect at the hands of a jury composed of cock-pheasants.

In the mean time, however, whilst the learned condemn it, and capital fought shy of it, the light went on burning steadily—carefully watched, examined, and, in course of time, appreciated by one, at whose hands truth never suffers, imposture is never spared, and whose opinions are ever formed for himself by patient and careful investigation, and not expressed until all doubt has been removed. Without having been made aware of the exact conclusions arrived at by this investigation, it must be supposed to have been favourable, as it led to a decision on the part of the Trinity Board to give the light a fair trial in a first order lighthouse, for which its peculiar qualities are pre-eminently fitted. To this end a contract was entered into with the Universal Lime Light Company by the Elder Brethren for the exhibition of the light in the South Foreland Lighthouse for three months, and upon its success will, in all probability, depend its extensive adoption for the purposes of coast lighting. The light was introduced on the 26th of August, and has continued to burn steadily and brilliantly every night since its substitution for the oil light. Indeed, after the report of Mr. Page, the engineer of Westminster bridge, upon the success of the lime light which for two months illuminated the finished part of that structure, no doubt can exist as to the facility with which it can be maintained, for no case of failure occurred there in maintaining regularly eighteen lights in nine different lamps, where the operation of making the gases had to be conducted upon temporary platforms, suspended between wind and water, and the whole arrangements necessarily of an incomplete and

temporary character, added very largely to the ordinary risk of a failure occurring. The lamp at the South Foreland is fitted with eight burners, to meet the requirements of the Fresnel apparatus, which is composed of eight panels: only six out of the eight burners, however, are required, as the two panels towards the land are darkened. The manipulation of the lamp is perfectly simple, not necessitating an amount of intelligence greater than is required in the case of an ordinary Argand lamp. When the time for lighting comes the lime wicks are inserted, the clock which moves them wound up, and the gases turned on, and no further attention is required until the hour arrives for putting out the light, when the gases are turned off, and the clock is stopped; the lime wicks are then removed, and nothing further remains to be done. The brilliancy of the light has not escaped the notice of our friends on the other side of the Channel, many of whom have been over to visit it. It is but fair to state that the present apparatus in which the lamp is exhibited is not calculated to give the maximum effect, having been especially adjusted for the usual Argand oil lamp, which differs from the lime light in the essential particular of focal distance, which is measured from the centre of the former, but from the surface of the latter; the lime light, therefore, although of the same diameter as the oil flame, is too near the lenses by half its diameter—in this case by $1\frac{1}{2}$ inches. It is to be hoped that this light will be tried both in a French apparatus, specially adjusted, and in the focus of a paraboloid, for it appears to possess every element, rendering it by far the best light for coast purposes ever introduced.

Amongst other attributes of the lime light is another of the very last importance. It is not affected by wind, even though unprotected by glass. At Liverpool, where the lime light was exhibited for two months upon the landing stage where the Birkenhead ferry boats ply, this property was most severely tested. One night a gale of wind came on, and increased in violence until the glasses of the lanterns were dashed in, and the light was exposed to its full power. No apparent effect was produced upon them, for they continued to burn as steadily and brightly as before. It has happened not so unfrequently as might be imagined, that the glass of our lighthouses has been broken, in violent gales, and the light blown out.

Professorship of Chemistry at Cambridge University.—This Professorship having become vacant by the death of the Rev. James Cumming, the election of a new Professor will take place on Friday, the 6th day of December next. The electors to the Professorship are the persons whose names are on the electoral roll of the University. The Vice-Chancellor and the proctors will receive the votes of the electors from eleven o'clock in the morning till one o'clock, when the Vice-Chancellor will declare the election.

ANSWERS TO CORRESPONDENTS.

* * All Editorial Communications are to be addressed to the EDITOR; and Advertisements and Business Communications to the PUBLISHER at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

* * In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

H. A. Simcol.—The minimum thermometer can be procured at Mr. Cassell's, Hatton Garden, London.

A would-be Member.—Write to the Secretary, Burlington House, Piccadilly, who will forward you the necessary information and blank forms to be filled up.

E. Ballard.—Received with thanks. In our next.

THE CHEMICAL NEWS.

VOL. IV. No. 104.—November 30, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Suggestions for a New System of Chemical Nomenclature, with reference more particularly to Organic Substances, by JOHN A. R. NEWLANDS, F.C.S.

CERTAIN letters, or syllables, are employed to denote the various quantities of each constituent in the substance named, and by uniting these the names are produced.

The accompanying list of numerals, which may be learned without much difficulty, is adapted for this purpose, though it, no doubt, admits of great improvement. Names constructed on these principles can be converted into formulæ, and formulæ into names, with the greatest ease, the one being an exact counterpart or reflex of the other. The names thus created may be either used *per se* or in conjunction with those at present existing. Thus, caprylic acid, $C_8H_{16}O_2$, may be either called meddatic acid, or medda-caprylic acid; and milk-sugar, $C_{12}H_{24}O_{12}$, may be named sepsatone, or sepsatic sugar, or sepsa sugar.

The numerals chosen for carbon are the consonants, simple and compound; thus, *b* signifies one of carbon; *d*, two of carbon; *f*, three; *g*, four; and so on in alphabetical order. The letter *c* is not employed, as its sound is always identical either with that of *k* or of *s*. *Q* and *x* are omitted for equally good reasons.

The numerals for hydrogen consist of a vowel followed by a consonant: thus, *ab* signifies one of hydrogen; *ad*, two; *af*, three; *ag*, four, &c.

Those for oxygen are simply the reverse of the preceding: thus, *ba* signifies one of oxygen; *da*, two; *fa*, three; *ga*, four, &c.

Lastly, the name is completed by a termination indicating its chemical properties: thus, acids may be made to end in *-ic*; bases, in *-ine*; neutral bodies, in *-one*; and hydrocarbons, in *-yl*.

The name of the group to which a substance belongs may also be frequently appended with advantage, such as *sugar*, *alcohol*, *aldehyde*, &c.

When two vowels come together, the letter *t* may be inserted to preserve euphony.

The following examples may serve to illustrate the method of nomenclature:—

1. Give the name of starch, $C_{12}H_{20}O_{10}$. Now, C_{12} is *s*; H_{20} , *ek*; and O_{10} , *pa*; the name is, therefore, *sekpato-ne*, or *sekpatic starch*.

2. Name, phenyl, C_6H_5 . C_6 is *k*; and H_5 , *ah*; the name is, therefore, *kahyl*, the base, phenylamine, $N(C_6H_5)_2$, being called *kahylamine*.

3. Name, ethylic-alcohol, C_2H_5O . C_2 is *d*; H_5 , *ah*; and *O*, *ba*. The name thus formed is *dakba*, or *dakbatic-alcohol*.

4. Give the formula of *mahban-one*. Here, *m* is C_8 ; *ah*, H_3 ; *ba*, *O*; and *n*, *N*. The formula, therefore, is, C_8H_3NO , or blue indigo.

5. What is the formula of *lakdatic acid*? *l* is C_7 ; *ah*, H_3 ; and *da*, O_2 . The formula, then, is, $C_7H_3O_2$, or benzoic acid.

6. Give the formula of *zehfan-ine*. *z* is C_{17} ; *eh*, H_{10} ; *fa*, O_3 ; and *N*, *n*; giving the formula, $C_{17}H_{10}NO_3$, or morphine.

Isomeric substances are distinguished by making a difference in their termination, thus:—

Allylic glycerin, $C_3H_5O_3$, "*fakfatic*" glycerin, or *fakfaterin*. Lactic acid, $C_3H_5O_3$, "*fakfatic*" acid.

The amount of nitrogen in a substance is indicated by the letter *n*, with or without a vowel, placed immediately before the end of the name: thus, *n* is *N*; *na*, N_2 ; *ne*, N_3 ; *ni*, N_4 , &c.

In like manner phosphorus may be represented by *p*, *pa*, *pe*, *pi*, &c.; arsenic by *r*, *ra*, *re*, *ri*, &c.; sulphur, by *s*, *sa*, *se*, *si*, &c. The same plan may be extended to other elements.

If one of these syllables be followed by a vowel, the letter *m* is inserted instead of *t*, in order to distinguish between substances containing *N* and those containing NO_2 .

Nomenclature of Substitution Products.

These may be named as at present, thus benzoic, nitrobenzoic, and binitrobenzoic acids may be termed *lakdatic*, *nitrolakdatic*, and *binitrolakdatic* acids, respectively; or a syllable may be introduced expressing the number of equivalents of hyponitric acid, chlorine, bromine, iodine, &c., which have replaced hydrogen in the normal substance: thus,—

Benzoic acid, $C_7H_5O_2$, *lakdatic acid*
Nitrobenzoic acid, $C_7(H_4NO_2)O_2$, *lakdanatic acid*
Binitrobenzoic acid, $C_7(H_2NO_2)_2O_2$, *lakdanetic acid*

Different quantities of NO_2 being represented by the syllables, *nat*, *net*, *nit*, *not*, and *nut*; of chlorine, by *cat*, *cel*, &c.; of bromine, by *bat*, *bet*, *bit*, &c.; and of iodine, by *dat*, *det*, *dit*, &c. Thus, in the case of acetic acid we have,—

Acetic acid, $C_2H_4O_2$, = *dagdatic acid*
Monochloroacetic acid, $C_2(H_3Cl)O_2$, = *dagdacatic acid*
Trichloroacetic acid, $C_2(HCl_3)O_2$, = *dagdacitic acid*
Monobromoacetic acid, $C_2(H_3Br)O_2$, = *dagdabatic acid*
Dibromoacetic acid, $C_2(H_2Br_2)O_2$, = *dagdabetic acid*
Iodoacetic acid, $C_2(H_3I)O_2$, = *dagdadatic acid*
Diiodoacetic acid, $C_2(H_2I_2)O_2$, = *dagdadetic acid*

As every change in composition is accompanied by a corresponding change in name, all the various modes of arranging bodies, such as homologous and isologous series, different degrees of oxidation, &c., have characteristic terms allotted to them.

Names deduced from empirical or rational formulæ may be distinguished as empirical or rational names. Thus, the rational name for aniline, $N(C_6H_5)_2$, is, as we have already seen, *kahylamine*; the empirical name, deduced from the formula C_8H_7N , is *kalnine*. Those of similar bodies, containing phosphorus or arsenic, in the place of nitrogen, would be,—

C_6H_7P , *kalpine*, and
 C_6H_7As , *kalrine*.

List of Numerals.

This list furnishes the names of organic compounds up to those containing 34 equivalents of C and 68 of H or O.

The numerals for O are merely the reverse of those for H; it is, therefore, only necessary to learn the latter.

	C.	H.		C.	H.		C.	H.
1	b	ab	18	bl	eg	35	il	52
2	d	ad	19	br	eh	36	im	53
3	f	af	20	ch	ek	37	in	54
4	g	ag	21	cl	el	38	ip	55
5	h	ah	22	cr	em	39	ir	56
6	k	ak	23	dr	en	40	is	57
7	l	al	24	fl	ep	41	it	58
8	m	am	25	fr	er	42	iv	59
9	n	an	26	gl	es	43	ob	60
10	p	ap	27	gr	el	44	od	61
11	r	ar	28	pl	eo	45	of	62
12	s	as	29	pr	ib	46	og	63
13	t	at	30	sl	id	47	oh	64
14	v	av	31	sp	if	48	ok	65
15	w	ew	32	st	ig	49	ol	66
16	y	ey	33	apl	ih	50	om	67
17	z	ez	34	str	tk	51	on	68

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 272.)

Determination of the Proportional Relation between Silver and Chloride of Sodium.—I have made ten determinations of the proportional relation between silver and chloride of sodium. To effect this I made use of chloride of sodium prepared by six different processes. I will describe each of them briefly.

I. Chloride of Sodium from Carbonate of Soda.—Commercial bicarbonate of soda was heated to dull redness in a silver dish, to render insoluble the iron which this body always contains. The residue, after cooling, was dissolved in pure cold water, and the solution evaporated to a pellicle in a platinum dish, and then allowed to crystallise. This was repeated ten successive times.

The carbonate was transformed into chloride in the same platinum vessel by passing a current of pure hydrochloric acid through the solution. The resulting salt was dried, heated to dull redness, and then taken up with cold water. The solution was decanted after standing twenty-four hours to separate the silica which was deposited, and then evaporated again. The chloride of sodium, which was perfectly white, was mixed with a tenth of its weight of pure chloride of ammonium containing chloroplatinate of ammonia, and then treated as I have explained above in the case of chloride of potassium obtained from nitrate of potash.

Chloride of sodium thus prepared is perfectly colourless and slightly hygrometric, a property which is not possessed by chloride of potassium. Its specific gravity differs according to whether it has been slowly or rapidly cooled after fusion. I have found it comprise between 2.125 and 2.150. When dissolved in cold water it produces a neutral and perfectly limpid solution.

Nos. I. and II. show the results furnished by two

specimens of chloride produced from two distinct samples of carbonate. The silver used in No. II. was prepared by M. Liebig.

II.—Chloride of Sodium from Colourless Rock Salt.—Some perfectly colourless rock salt free from iron, manganese, and sulphate, but still containing traces of magnesium and calcium, was submitted in a platinum dish to six crystallisations from a hot solution. As it was possible that the boiling mother liquid might contain potassium, this was always thrown away. The perfectly white salt was pulverised and exhausted with alcohol at 96 per cent., and then digested with alcohol at 65, containing bichloride of platinum. The clear alcoholic solution was separated and replaced by a fresh quantity of alcohol at 70, still containing bichloride of platinum, until about one-third of the salt was dissolved. These solutions were mixed together.

The residuary saline mass was treated with a fresh quantity of alcohol at 70, containing bichloride of platinum, until about one-third of the salt employed was left undissolved. The solutions obtained by this second treatment were then mixed together. The first and the second solution each represent, therefore, a third of the total weight of the salt experimented upon. The two solutions were separately precipitated by absolutely pure sal-ammoniac. After standing sufficiently long, they were decanted and evaporated to dryness in a platinum retort. The salt was mixed with pure sal-ammoniac and chloroplatinate of ammonia, and treated as I have explained in the case of chloride of potassium from nitre.

No. III. shows the result furnished by some of the first third of the salt, and Nos. IV. and V. the results obtained by some of the second third. The extraordinary concordance of these results proves the identity of the two chlorides, and demonstrates also to what accuracy it is possible to arrive by the wet way.

III.—Chloride of Sodium from Sulphate of Soda.—Sulphate of soda freed from all foreign metals by the ebullition of its solution with a slight excess of sulphide of sodium and carbonate of soda, was submitted to ten successive crystallisations. The salt thus obtained was dried in a platinum dish and mixed with twice its weight of chloride of ammonium prepared by the direct action of ammonia and pure hydrochloric acid. The mixture was heated to dull redness. The residue, mixed again with its own weight of pure sal-ammoniac, was heated to redness a second time. All the sulphate is thus converted into marine salt contaminated with traces of platinum and silica. It was fused with a small quantity of pure sal-ammoniac and chloroplatinate of ammonia, and treated as previously explained in the case of chloride of potassium from nitre.

No. VI. shows the result furnished by this salt.

IV. Chloride of Sodium from Tartrate of Soda.—Pure carbonate of soda was neutralised by tartaric acid prepared by decomposing tartrate of lead with hydrosulphuric acid.

The tartrate, crystallised a great many times, was dried and boiled several successive times with alcohol at 96, and then transformed into chloride by the means which I employed in the preparation of chloride of potassium from pure cream of tartar.

The result obtained is shown at No. VII.

V. Chloride of Sodium from Nitrate of Soda.—With this object I have prepared pure nitrate of soda from Chili saltpetre, from which I have eliminated the foreign metals by a mixture of sulphide of sodium and carbonate of soda, and which I have then crystallised at least ten successive times from pure water. This nitrate

was converted into pure chloride by the means employed to convert saltpetre into chloride of potassium.

No. VIII. shows the result obtained by this chloride.

VII. Chloride of Sodium from Chloro-platinate of Soda.—The chloride of sodium from rock salt, purified with bichloride of platinum and alcohol, and of which the results are given in Nos. III., IV., and V., was mixed with three times its weight of bichloride of platinum. The mixture was dissolved in pure water, and the solution evaporated to dryness. The residue taken up by a small quantity of water, at once completely dissolved in it. This solution, evaporated to a pellicle, was allowed to crystallise; the mother liquor was decanted. The crystals of a pure golden yellow colour were taken up by the smallest possible quantity of boiling water, and furnished, upon cooling, a fresh crystallisation of chloroplatinate of soda. I have in this manner effected the solution and crystallisation of chloroplatinate of soda six successive times. At this point I removed a certain quantity, and continued the solutions and crystallisations six times more.

The two chloroplatinates thus prepared were of a pure golden yellow. After having dissolved them in water, I decomposed them separately with pure chloride of ammonium, and evaporated the solution to dryness in a platinum dish. The resulting chlorides were then treated as I have above explained in the case of chloride of potassium from nitre.

In submitting the chloroplatinate of soda to so great a number of crystallisations, my object was to concentrate in the mother liquors the chloroplatinates more soluble than that of soda, and in the chloroplatinate of sodium the chloroplatinates less soluble than this; chloroplati-

nate of potassium, for instance, if by any means the salt employed in the preparation of the double salt could contain traces of a potassium compound.

I commenced the operation with about three hundred grammes of chloroplatinate of soda, and at the end of the operation I had separated above two-thirds of the compound with the mother liquors.

No. IX. shows the result obtained by means of the salt prepared from the chloroplatinate, which was crystallised six times; and No. X. that of the salt crystallised twelve times. The almost absolute identity of the two results prove in the clearest manner that the salt employed contains no chloride capable of producing a chloroplatinate less soluble than that of soda. This identity proves, moreover, that the double salt contains no chloride having a higher equivalent than that of chloride of sodium itself.

All the specimens, indiscriminately, of chloride of sodium experimented upon were colourless, and dissolved in water, producing a neutral and absolutely limpid liquid. I happened to preserve for a month in a stoppered glass vessel a solution of this compound, without perceiving the slightest trace of any deposit. I am, therefore, thoroughly convinced that it is impossible by the means at present at our disposal to prepare this chloride in a state of greater purity.

As the purest marine salt is slightly hygrometric, when I weighed it, I always heated it nearly to redness in a closed platinum crucible, and immediately introduced it into a tube closed at one end and furnished with a glass stopper ground in and pierced with a small hole. During cooling, this tube was placed under a bell-jar with sulphuric acid.

Proportional Relation between Silver and Chloride of Sodium.

Numerical Order.	Weight of the Chloride of Sodium in air.	Weight of the Chloride of Sodium reduced to a vacuum.	Weight of the Silver in air.	Weight of the Silver reduced to a vacuum.	Weight of the excess of Silver after the double decomposition.	Weight of the Chloride of Sodium corresponding to the excess of Silver.	100'000 parts of Silver are equivalent to
I.	grammes. 4'87865	grammes. 4'88128	grammes. 9'01103	grammes. 9'01159	grammes. 0'0071	grammes. 0'00384	54'2093
II.	5'34495	5'32781	9'83535	9'83595	0'0076	0'00412	54'2088
III.	4'40145	4'40382	8'12960	8'13012	0'0062	0'00336	54'2070
IV.	4'07495	4'07715	7'5266	7'52704	0'0056	0'00350	54'2070
V.	10'5754	10'5808	19'5326	19'5338	0'0146	0'00791	54'2070
VI.	6'7913	6'79496	12'5438	12'5446	0'0091	0'00493	54'2060
VII.	10'5192	10'5249	19'4293	19'4305	0'0145	0'00790	54'2076
VIII.	13'8862	13'8937	25'6483	25'6499	0'0194	0'01074	54'2081
IX.	18'1874	18'1972	33'59275	33'59483	0'0257	0'01393	54'2083
X.	6'3462	6'3496	11'7216	11'72263	0'0091	0'00493	54'2089
Mean							54'2078

Parts of Chloride of Sodium.

(To be continued.)

On the Adulteration of Butter with Animal Fats, by EDWARD BALLARD, M.D. Lond., M.R.C.P., Medical Officer of Health for Islington, and Parochial Analyst under the Adulteration of Food Act.

As the ether test for foreign fats in butter, described by Mr. Horsley in a recent Number of the CHEMICAL NEWS, is somewhat an expensive one, and, on this account the less likely to come into much practical use, I am induced to send for your readers a description of some other tests cheaper and more readily applied.

The most frequent adulterating fat by far is that of beef. We may call it beef "dripping" if we will, dripping being the form in which the fat I refer to is

best known. That such is the case may be seen by any one who will collect at 65° the residue of solution by ether and melt it in a watch-glass. It will, on cooling, be seen to possess the physical characters of beef dripping, with the exception of the odour and taste, both of which are wanting. These characters are, a dull, yellowish colour, granular texture, but less so than mutton dripping, melting slowly on the tongue, with a granular feel when the tongue is moved upon it. Its consistence is firm, and when the mass is broken by a spatula the granulations do not separate like those of mutton, and its consistence is more waxy. Mutton dripping is white.

The simple distinguishing character between pure butter and butter adulterated with beef dripping may

be noticed in the following particulars:—Its colour, consistence, odour, the appearance on washing with hot water, the occurrences on drying on blotting-paper, the physical characters when dried, and the results on pressure of the dried mass. To render these observations more precise in statement, I will give them in parallel columns, the adulteration of the butter for this purpose being supposed to be in equal proportions of butter and beef dripping.

Butter, newly churned and free from salt.

Colour.—Bright yellow—uniform.

Consistence.—Firm at 54°.

Odour.—Strongly butyraceous.

Appearance on washing, in a glass half-a-pint beaker, one oz. with boiling water.—A melted layer occurs on the surface, consisting, as viewed at the side of the beaker, of minute drops, which, lying closely together, give a finely cellular aspect to the layer. When cool the cake is a soft solid, fluctuating on agitation of the glass, or, at any rate, yielding readily when the glass is tilted. The water below is very opaque with milkiness. The cake is yellow.

On Transference to Blotting-paper.—The mass is wet, and the wet runs away slowly.

The blotting-paper acquires a marked yellow, greasy stain, which runs extensively over it. On transferring the butter to a second fold of paper, this stain is again imparted.

Physical Characters of Dried Butter.—Colour, bright yellow—odour, butyraceous—breaks under pressure of the spatula at 54° into fine mealiness—taste, butyraceous, not melting on tongue so smoothly as unwashed butter, but still quickly, and with little sense of granulation.

Butter Adulterated with Dripping.

Paler (paleness increases with the proportion of dripping)—often marbled yellow and white.

Firmer (increasing with proportion of dripping).

Butyraceous odour lessened (but still present even in large amounts of adulteration).

The melted layer consists of much larger drops, and the cellular appearance, therefore larger. On cooling, the cake is firmer, paler in colour, sometimes marbled, does not yield at all or much less readily to tilting of the beaker. The water below is less opaque with milkiness, (and often deposits dirt from the salt used in preparation).

The mass is almost dry on removal from water, and the remaining water rapidly runs away into the paper.

The stain to first blotting-paper is less marked, and that to second also. Both are of a less deep tint.

Colour less yellow—odour, that of dripping, but more or less butyraceous (this varying with the relative proportions of the two)—has a coarsely granular appearance in mass, and on gentle pressure with a spatula at 54° breaks down into independent granules very readily, these being the size of fine pearl sago and globular in shape—taste of the dripping distinctly perceptible (in low degrees of adulteration this is less marked)

Pressure of a little heap the size of a pea firmly and quickly by a spatula between a fold of blotting-paper gives an irregular, ill-defined, greasy stain, not very marked.

These tests are such as may be readily applied by a wholesale or retail purchaser. These are by no means all the signs easily applicable for distinguishing this form of adulteration. Should the present paper receive insertion and be acceptable to your readers, I shall be happy to furnish others which have resulted from my investigations on this subject. It is one especially interesting to me, since it is believed that at Belle Isle—a nest of nuisance factories in my parish—the material for the adulteration of butter is manufactured to a great extent for exportation. The "fresh" butters are those chiefly and to the greatest amount adulterated with this fat.

Parochial Laboratory, Islington, November 12.

[We shall be glad to receive further communications from this Correspondent.—ED. C. N.]

Researches on the Separation, by the Wet Way, of Gold or Platinum from Tin and Antimony. Reduction of Perchloride of Iron by Platinum, by MM. A. BÉCHAMP and C. SAINTPIERRE.

IT is not a matter of indifference as to which ferrous compound is made use of as a reducing agent. The sulphate and chloride reduce nitric acid of the metallic or organic nitrates, and gold from the chloride of gold, but they do not reduce nitro-benzole and analogous nitro-compounds, or bichloride of platinum. By substituting ferrous acetate for sulphate and chloride, all these compounds are reducible, and platinum especially is reduced to a metallic state.

Profiting by these indications, we propose to separate, by the wet way, gold and platinum from tin and antimony. We have previously proved that neither ferrous chloride nor acetate will reduce either chloride of antimony or bichloride of tin, even by boiling, and that qualitatively it is easy to precipitate gold and platinum by ferrous acetate in presence of these chlorides. The gold may even be precipitated, in the first instance, by protochloride of iron from a solution containing tin, antimony, and platinum, and the metal exactly estimated.

On our endeavour to estimate platinum which had been precipitated by acetate of protoxide of iron, we found this problem complicated by many difficulties which we first of all strove to overcome. The chief difficulty we met with was, that, though all the platinum was precipitated by acetate of iron, its weight was generally too low. We then reflected on the peculiar nature of platinum, and particularly on the fact that a body so difficult to attack might furnish a chloride irreducible by protochloride of iron. From this conjecture to the supposition that platinum might be a reducing agent there was but a step. In effect, platinum reduces perchloride of iron into protochloride, and is

—does not melt so smoothly or readily on the tongue, but for a long time a roughness remains upon it, increasing as it melts.

The stain is a circular, well-defined, oily spot, which soon becomes surrounded by a less greasy halo. (The perfection of this stain appears to vary with the quantity of the adulterant.)

itself dissolved in the liquid in the state of bichloride, according to the equation $\text{Pt} + 2 \text{Fe}_2\text{Cl}_3 = \text{PtCl}_2 + 4 \text{FeCl}$.

The weights of the reduced platinum were too low, because in freeing it by hydrochloric acid from the peroxide of iron precipitated with it a portion was redissolved, by virtue of an inverse reaction. This new and unexpected property of platinum we have established by the following experiments:—

1. Reduce 10 cubic centimètres of bichloride of platinum, containing 0.117 grammes of metal, by ferrous acetate. Next day add some perchloride of iron and a little hydrochloric acid, and after boiling two or three hours, all the platinum is redissolved.

2. Reduce in the same manner 5 centimètres of the same bichloride, containing 0.0585 grammes of platinum, and as soon as possible heat the precipitated metal with perchloride of iron. Ten minutes' boiling redissolves the whole.

3. Heat in a sand bath, with a solution of perchloride of iron acidulated with hydrochloric acid, 0.291 grammes of spongy platinum, prepared by the calcination of chloro-platinate of ammonia. Carefully add from time to time weak hydrochloric acid to maintain a constant volume. After eighteen or twenty hours' action between 80° and the boiling point of the mixture, all the platinum is dissolved.

4. 0.05 grammes of platinum black prepared by Liebig's process with hydrochloric acid, is not attacked after prolonged boiling with hydrochloric acid. Twelve hours boiling will completely dissolve it in perchloride of iron.

5. A platinum plate thoroughly cleaned with boiling hydrochloric and sulphuric acid, and then made red hot, weighed 3.294 grammes. Then it was heated in the same manner as indicated above with perchloride of iron. After twenty-one hours it was washed, heated, and weighed. Its weight was not more than 3.243 grammes, having lost 0.051 grammes. The platinum, then, is dissolved with difficulty in proportion as it is less divided and more dense.

The hydrochloric acid and perchloride we employed contained no free chlorine. That the dissolving of the platinum might not be attributed to the accidental presence of chlorine, we took care in each experiment, by means of red prussiate, that the perchloride of iron should be partially reduced into protochloride, forming Turnbull's blue. The perchloride was prepared by the process we published in 1859—the chlorination of a protochloride of iron solution by chlorine. This compound gives no precipitate, and not being coloured blue by red cyanide, the accidental presence of chlorine is then out of the question. Consequently, if protochloride of iron does not reduce bichloride of platinum, it is because platinum in this case is the more powerful reducing agent.

It would be interesting to study from the same point of view the metals partaking of the nature of platinum, palladium, iridium, &c. We propose to do this in continuation of the work which has been the subject of this memoir.—*Comptes-Rendus*.

On the Presence of Free Nitric Acid and Oxygenised Nitro-compounds in Atmospheric Air, by M. S. CLOEZ.

THE presence of free nitric acid and oxygenised nitro-compounds in atmospheric air has not been hitherto clearly and fully established. It is important for the

theory of nitrification that this apparently simple but really complicated question, confounded as it is with the question of ozone, should be experimentally resolved. To this end I have made the following observations:—

Ordinary air, collected by an aspirator at about a metre from the earth's surface, often makes the blue colour of wet litmus paper turn permanently red.

The tincture of blue-violet litmus, contained in a bulb-tube, through which air is passed, undergoes the same change. It has been proved that the red colour is persistent after heating the liquid to the boiling-point.

This phenomenon does not take place in all seasons indifferently, but it is frequent at the beginning and end of winter.

The nature of the acid producing this coloration is determined by passing the air into a solution of pure carbonate of potash, when a notable quantity of nitrate of potash will be found in the liquid, and generally traces of alkaline chloride, but no sulphate. The air must be filtered by passing through a pellet of asbestos.

Pure carbonate of lead substituted for carbonate of potash, and placed in a long glass tube, into which air passes, absorbs the acid vapours, forming nitrate of lead, soluble in cold water, and which is crystallisable.

Iodised starched paper has not been intentionally employed to detect the nitric acid of the air, yet this reagent is more sensitive to the action of nitrous compounds than litmus-paper; for it becomes coloured almost instantaneously in air, to which is added 0.00005 its volume of hyponitric acid, whilst litmus-paper, under these circumstances, would undergo no change.

The action of artificial nitrous air on solution of iodide of potassium produces, first, a saline compound, with a slight alkaline reaction, persisting in presence of iodine, but disappearing under the influence of an excess of nitrous air.

This singular phenomenon is similarly produced with air supposed to contain ozone. The phenomenon is explicable in both cases, by admitting the momentary formation of a small quantity of nitrite of potash, its reaction being, in fact, slightly alkaline. Whatever the true cause of the phenomenon, the inference is the same,—namely, that all the ozonometric observations made according to the process devised by M. Houzeau, are as inexact as all those previously made with Schœnbein's re-agent.

The recognised existence of free nitric acid in the atmosphere accounts for the presence of ammonia in certain specimens of rust, and the almost entire absence of it in others. It is very probable that the ammonia contained in the oxide of iron, formed in free air, is the result of the combination of nitrogen, proceeding from the reduction of nitric acid, with the hydrogen produced by the simultaneous decomposition of water. If this is the case, it is easy to understand that rust formed in humid air, freed from acid vapours, ought not to contain ammonia, like that formed in free air.

The formation of verdigris on unvarnished bells and bronze statues may be also, in a great measure, attributed to the free nitric acid of the air. An examination of green matter taken from a bell exposed to the inclemencies of the weather since 1793, has shown the presence of nitric acid.

In regard to agricultural science, the presence of nitrous compounds in the air is of the greatest importance: it accounts for the good effects of slight and superficial liming as well as marling; in short, it helps to explain the success of the agricultural practice of which M. Chevreul has recently given the theory and

set forth the advantages,—namely, the operation of *ecobuage* with combustion, originated by the Marquis of Turbilly, and carried out by him on a vast scale and with complete success.—*Comptes-Rendus*.

Rubidium and Cesium.

PROFESSOR BUNSEN announces that he has received a considerable quantity of a *lithioniferous mica*, containing more than three per cent. of cesium and rubidium, and, consequently, suitable for the preparation of these new bodies on the large scale. M. Seybel, the head of a large manufactory of chemical products at Liesing, near Vienna, has procured large quantities of lepidolite from Rozena, in Moravia, and of lithioniferous mica from Finnwale, in Bohemia, in order to extract from them *lithia*, *cesia*, and *rubidia*.

PHARMACY, TOXICOLOGY, &c.

On the Copper Test for Sugar, by JOHN HORSLEY.

A FEW weeks since the CHEMICAL NEWS contained a review of "Braithwaite's Retrospect," in which allusion was made to a new cupreous preparation, by Dr. Pavy, for the detection and estimation of sugar, which he recommended as being superior to either Barreswill's or Fehling's. In that respect I differ with him, having tried his method, and object to it on account of the excess of caustic potash it contains, having known instances where the copper was reduced *per se* on standing.

Like the Doctor, I have been so dissatisfied with the reduction by copper solutions as to direct my attention to other agents, till I found out that it was not so much the test itself as the mode of manipulation that required improvement, the copper test being delicate enough when well prepared and the saccharine fluid sufficiently pure. The ordinary method of procedure is, I believe, pretty much as described by Dr. Pavy,—viz., to boil and decolorise the cupreous liquor by the addition from time to time of the saccharine solution, it being generally supposed that the powers of the sugar are defunct at the moment the blue colour disappears, than which nothing is more fallacious, the sugar being then in excess, as may be proved by a further addition of the test and re-boiling. In fact, you may even make such an addition of the test as that, on boiling and filtering, the liquor shall pass through highly coloured and yet contain sugar, which will not cease to reduce copper till after two or three other boilings and filtrations have been made. The filtered liquid being permanently blue or bluish-green, and yielding no further red precipitate after several minutes' boiling, is the alone criterion of the exhaustion of the sugar, which is just the inverse of the present method. The filtered blue liquid may be again rendered available for other reductions by concentration or evaporation down to about the tone of the original copper solution used. The following is the formula by which I make the solution of copper:—Sulphate of copper, half-a-drachm; distilled water, three ounces. Dissolve by heat, then add, tartaric acid, half-a-drachm; and, when cool, caustic potash, one and a-half drachms; and, lastly, pure carbonate of potash, one and a-half drachms. An ounce of this liquor boiled with half-a-grain of grape sugar, dissolved in a little water, gave a scarlet red precipitate of one and a-half grains, every three parts representing one of sugar. The $\frac{1}{10}$ th part of a grain of sugar produces a decided precipitate.

Diabetic urine ought to be purified by precipitation with excess of sugar of lead, and any remaining lead thrown out by passing a stream of sulphide of hydrogen, the liquid filtered and evaporated to about half its bulk previous to applying the cupreous liquor; but if the quantity of lead salt be nicely adjusted, there will be no necessity for using the gas. High-coloured urine, supposed to contain sugar, requires, in addition to the precipitation by lead, to be decolorised by animal charcoal, and before using the test any lime which may have been acquired from the charcoal should be extracted by boiling the filtrate with a little carbonate of potash and filtering. Such urine will not afford, like true diabetic urine, a red precipitate, but a muddy, yellow one, if sugar exist at all, and the filtered liquid, instead of being blue, will be yellowish green.

Although it has been stated, on the authority of Brücké and others, that sugar obtains in ordinary or healthy urine, I have not been able to verify it by tests, owing probably to not having operated on a sufficiently large quantity. The only way of proving it, I presume, would be by carefully evaporating to an extract the purified urine and adopting the fermentation process.

Laboratory, Police Station, Cheltenham.

On the Properties of Sulphate of Aniline.¹

DR. TURNBULL, of Liverpool, read a Paper "On the Properties of Sulphate of Aniline"—a new remedy which he had used with success in the treatment of chorea, or St. Vitus's dance. He said that medical men had not acquired a knowledge of the properties of new remedies commensurate with the advances which had been made in other branches of medicine. The progress of organic chemistry had brought to light many new bodies worthy of investigation; and there could be little doubt that many of them would, if their properties were investigated, be found to prove remedies of utility. The artificial alkaloids were a numerous class, and from a resemblance in chemical constitution to the vegetable alkaloids, such as quinine, morphia, &c., it might reasonably be expected that some of them should have powerful and useful properties. He had made some trials of more than one of them, and especially of the sulphate of the artificial alkaloid aniline, which he had administered in cases of nervous disorder. He then proceeded to give an account of six cases of chorea which he had treated successfully with this new remedy. The first was a case of a girl, aged thirteen, who had involuntary twitching movements in all the limbs, and was for two months under the usual treatment, but without deriving the slightest benefit till the sulphate of aniline was given. She then recovered rapidly, and got quite well. The second was a case in which the left side was chiefly affected, and the recovery was perfect in twenty-one days. The third was a case in which the twitching movements were brought on by a fright, and the recovery took place in fourteen days. The fourth and fifth were cases of recovery from the ordinary form of the disease. The sixth was a case of unusual severity, with partial loss of power of the lower limbs and inability to speak. Rapid and perfect recovery was, however, effected in this case by means of the same remedy. He adverted to two other cases in which sul-

¹ Read before the British Association.

phate of aniline had been used with success by other medical men: and he expressed the conviction that it would be found by the profession to be a valuable new agent in the treatment of this class of nervous diseases. In regard to the physiological action, he stated that aniline appeared to act directly on the nervous system. The most remarkable effect, however, which it produced was a transient alteration in the colour of the skin and lips, which he attributed to oxidation of the aniline and the formation of a colouring matter in the blood. But this, it was observed, must be speedily eliminated from the system, as the natural colour was restored as soon as the remedy was laid aside.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, November 21, 1861.

Dr. A. W. HOFMANN, F.R.S., President, in the Chair.

Peter Macowen, B.A., Professor of Chemistry at Huddersfield College, was elected a Fellow.

Dr. THUDICHUM read a paper "*On Leucic Acid.*" He commenced by stating that the present paper contained an account of one of a series of researches on the products of decomposition of albuminous substances, either by reagents, or within the system. It had been known for some time that fibrin, under the action of hydrochloric acid, produced two bodies—leucine and tyrosine, and that various substances were produced within the body by decomposition under modifying circumstances, as, for instance, cholesterin, which sometimes forms biliary calculi. Leucine appeared to occur in different parts of the body, both normally and abnormally, and particularly in the pancreas; while, in the diseases classed under the name of atrophy, it appeared to be formed in the liver. The relation known to exist between alanine and lactic acid, the presence of which in the system is supposed to be the cause of rheumatic fever, had suggested the idea that there existed an acid body bearing a similar relation to leucine. This acid was already known, and a method of preparing it had been published in Liebig's *Annalen*—namely, by the action of nitric acid on leucine. In preparing this acid it was better to employ an excess of leucine; a mixture of leucic acid and nitrate of leucine would be obtained, from which the leucic acid might be separated by evaporating the solution, and treating the residue with ether; evaporating the ethereal solution, and drying the residue over sulphuric acid; or ether might be employed without previous evaporation; but the complete removal of the acid was then more difficult. The acid obtained in this manner was contaminated with an oily substance, which could be removed by pressure, or by solution in water. The properties of the acid were as follows:—It has when solid the consistence of hard soap; it reddens litmus, and decomposes alkaline carbonates; is soluble in water, alcohol, and ether, and fuses below the temperature of boiling water; on exposure to heat it gradually alters to an oily liquid, this same oily substance is always formed when a solution of the acid is heated. The composition of this substance is represented by the formula $C_{12}H_{12}O_6$, leucine being $C_{10}H_{12}NO_4$, from which it is evident that these bodies have the same relation to one another that lactic acid has to alanine. Nitroleucic acid was formed by the action of nitric acid; this substance was insoluble in ether, but if leucic acid was present it dissolved to some extent. Various salts of leucic acid were then described, more especially the lead and mercury salts, which when precipitated presented under the microscope an appearance resembling the yeast plant, and even seemed to grow in the same manner. The

composition of these salts was represented by the general formula $C_{12}H_{11}MO_6$.

Dr. H. BENCKE JONES read a paper "*On the Occurrence of Phosphate of Lime in the Human Urine.*" Various diseases had been supposed to give rise to such deposits in the urine, more especially diseases of the nervous system and the brain. Dr. Roberts had, however, pointed out that, by adding chloride of calcium and neutralization, a deposit of phosphate of lime could be obtained in the healthy secretion. It appeared that, by employing both these reagents together, a deposit could always be obtained, and that sometimes the addition of the lime salt alone was sufficient; at other times neutralization caused a deposit, no additional lime being necessary. Deposits were formed by mixing solutions of chloride of calcium and phosphate of soda; and the resulting compounds were compared with those obtained in urine. Doses of lime-water and of acetate of lime were given to a patient at St. George's Hospital who had been suffering from dyspepsia, and who had nearly recovered; a deposit was obtained with some difficulty, but a dose of one pint of lime-water daily, or thirty grains of acetate of lime, was always found to be sufficient.

Dr. THUDICHUM remarked that the crystals obtained by such treatment were often so minute that a microscope was necessary to discover their presence; also that bitartrate of potash and carbonate of soda, when given internally, would cause similar deposits, generally little globular concretions, in which the crystals radiated from the centre to the circumference, these concretions varying from the size of a pin's head to that of a pea, and consisting of pure phosphate of lime, oxalate of lime being also formed at the same time, and that the administration of nitric acid removed both these deposits.

The PRESIDENT inquired what was the composition of the phosphate of lime deposit?

Dr. BENCKE JONES replied, that that point had already been determined, the crystalline deposit being composed of two equivalents of lime, one of water, and one of phosphoric acid; and the amorphous compound of three equivalents of lime and one of phosphoric acid.

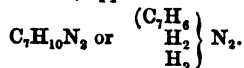
Mr. E. J. MILLS read a paper "*On Sparteine.*" A doubt had been raised as to whether the formula usually given for sparteine—namely, $C_{18}H_{13}N$ —was correct; in fact, Gerhardt, finding that it was difficult to represent the composition of this substance by his nomenclature, had concluded that the formula should be altered to $C_{17}H_{13}N$. It appeared that the best method of drying this substance was to treat it with sodium, for that exposure over sulphuric acid failed to remove all the water. A careful analysis of this body corroborated the formula originally given by Dr. Stenhouse; it appeared to be a diimine of a composition represented by the formula $C_{17}H_{14}N_2$, and homologous with nicotine, $C_{10}H_{14}N_2$. The salts of this base were described, the platinum salt had the composition $C_{17}H_{14}N_2 \cdot 2HCl + 2PtCl_2 + 2H_2O$; the gold salt, on the contrary, contained one equivalent each of chloride of gold and of chloride of sparteine. By the action of iodide of ethyl on sparteine, a compound of the composition $[C_{17}H_{13}(C_2H_5)_2N_2]^+I_2^-$ was obtained, which was not decomposed by caustic potash, and on treatment with oxide of silver gave the corresponding base. The platinum salt of this base was analysed, and compounds with chloride of zinc were formed. The action of iodide of ethyl on a solution of the base in methylated spirit gave a product, the composition of which was still doubtful.

Dr. STENHOUSE drew attention to the very small number of natural volatile bases that had been discovered, although a great many had been formed artificially.

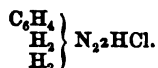
Dr. ODLING said that a great variety of volatile bases were formed during the spontaneous decomposition of nitrogenous bodies. He also alluded to volatile bases that had been obtained from sewage.

The PRESIDENT remarked that trimethylamine had been

obtained as a product of the decomposition of human urine. He afterwards alluded to some compounds that he hoped soon to be able to produce from benzol and its homologues, bearing the same relation to the aniline compounds that the diatomic ethylene bases did to ethylamine. The action of reducing agents on binitrobenzol had been tried by different experimenters, with the view of forming a diatomic base, but with unsatisfactory results. He had, however, lately received from Dr. Oppenheim a crystalline body contained in the refuse from some aniline works at Paris, which, on examination, appeared to have the composition



This compound had been produced on account of the impurity of the benzol employed in the manufacture of aniline; toluol being present in some quantity. He had found that the action of metallic iron and acetic acid on binitrotoluol completely verified this conclusion. Binitrobenzol was then submitted to the action of the same reagents, and was reduced and furnished the corresponding base, salts of this base had been prepared, the chloride had the composition



With chloride of cyanogen and other reagents, the new compound underwent similar changes to the aniline itself. It was possible that besides these compounds, others corresponding to the ammonias of the highest order could be produced by changing the bases already obtained to nitro-compounds and acting on these by reducing agents.

It was announced that at the next meeting papers would be read by Dr. OPPENHEIM "*On the Camphor of Peppermint*"; Mr. G. C. FOSTER "*On Piperic and Hydro-piperic Acids*"; Professor BOLLEY "*On the Physical Properties of some Alloys of Lead and Tin*"; and "*On the Dangers Produced by Feeding Steam Boilers with Certain Waters*."

NOTICES OF PATENTS.

617. *Obtaining Fresh Water from Salt Water.* D. HENSON and W. G. RAMSDEN, Liverpool. Dated March 13, 1861.

THE arrangement of apparatus for carrying out this invention is described as being particularly applicable on board steam-ships, for the purpose of procuring by distillation supplies of fresh water from the sea. The steam boilers ordinarily working in connexion with the engine are likewise placed in communication with a cylindrical or other conveniently-shaped vessel containing the sea-water to be submitted to distillation. The pipes conveying the steam may either dip directly into the sea-water, or pass through this second vessel in the form of a coil or multitubular arrangement. In any case, the object of the invention, as stated by the patentees, consists in the employment of steam as a means of heating and causing the evaporation of salt water. From the upper part of the distilling chamber an exit pipe is led off, which conducts the steam therein generated through a worm tube or other condensing arrangement immersed in cold water. In this manner, a supply of fresh, drinkable water is procured by distillation at ordinary pressures; but, in the event of the steam-ship being provided with engines having surface condensers, it is proposed to open a communication between these latter and the evaporating vessel, with the intention of carrying out the process at lower temperatures by the creation of a partial vacuum.

In the construction of this apparatus there is an obvious anomaly in respect to the intervention of the second receptacle for sea-water. A very simple inter-

mediate contrivance would effectually prevent the escape of particles of liquid and solid matters which are liable to accompany the steam generated in the ordinary engine boilers. It would, therefore, surely be easier to procure the evaporation of the salt water at once from the boiler, than to proceed upon an indirect system of conducting steam from a marine boiler into sea-water in order to supply the heat requisite for re-converting the contents of the second chamber again into vapour. With reference to the suggestion for conducting the process of distillation under circumstances of removed pressure, it must be remembered that the difficulties attending the condensation will, in the same measure, be augmented, and the action of the condensers themselves impaired by this adaptation to a double purpose. The aerating arrangement which constitutes an important feature of Dr. Alphonse Normandy's apparatus is here altogether omitted.

622. *Apparatus used in the Manufacture of Paper.* J. L. JULLION, Tynemouth, Northumberland. Dated March 13, 1861.

It is customary to use a felt pad of woollen material for the purpose of receiving, draining, and drying the newly made sheet of paper; the patentees employ as a substitute a fabric made of cotton or other suitable vegetable fibre.

640. *Tanning Leather.* A. F. MENARD, Rue de Strasbourg, Paris. A communication. Dated March 14, 1861. (Not proceeded with.)

In this specification, which refers to a more expeditious system of tanning and preparing leather, the inventor enumerates a variety of physical conditions under which the process may be conducted; as, for instance, with the employment of a vacuum, or under increased pressure; by the injection of carbonic acid gas combined with the transmission of galvanic and electric currents, &c. &c. The subject of the eighth claim is, however, of a chemical nature, and relates to the employment of bisulphide of carbon, which, being a solvent for fatty matters, is said to have the property of rendering the leather more supple and pliable, and of conferring upon it superior waterproofing qualities.

The application of this fluid in conjunction with gutta percha and india-rubber, has previously formed the subject of a provisional patent, which will be found described at page 82 of the present volume.

642. *Certain Improvements in the Manufacture of White Lead and Other Salts of Lead direct from Ores Containing Carbonate of Lead.* JOHN ARTHUR PHILLIPS, Earl's Court, Kensington. Dated March 15, 1861.

A PROTEST having been entered against this patent prevents us from giving the particulars of the specification.

650. *Obtaining and Utilising the Chemical Products of Spent Bark commonly called Tan.* W. LARBERG, St. Mary-at-Hill, Eastcheap, London. Dated March 15, 1861.

THIS invention relates to the treatment of waste tan or other woody fibre by destructive distillation. When submitted to this process, the following substances are obtained as products:—Charcoal, tar and other oily matters, pyroligneous acid, and gas. The tarry substances may by a second distillation be made to furnish paraffin, creosote, and wood naphtha. The crude acetic acid may be collected in the ordinary way, or by the intervention of a metallic vessel containing alkali between the retort and condenser, the acid may be retained, without condensing much of the aqueous vapour, and the corresponding acetate produced in one operation. The permanent gas liberated during the process consists chiefly of hydrogen

and carbonic oxide, and may therefore at once be employed as a source of heat in assisting the action of the furnace; or by suitable means can be made to burn with a luminous flame, and consequently be applied to illuminating purposes.

The mode of fixing the free acetic acid in the distillate is likely to be found serviceable, but the methods of disposing of the gaseous products from wood, by rendering them available as sources of heat and light, have been anticipated.

652. *Manufacture of Gas.* F. TRACHSEL and T. CLAYTON, Manchester. Dated March 15, 1861.

THE improvements described in this invention relate to certain methods of producing light, and to apparatus for rendering hydrogen gas, carbonic oxide, and even atmospheric air and other gases luminous by the introduction of volatile substances of a combustible nature, and in such a manner that the air or gases so impregnated may be conveyed through pipes and burnt like ordinary coal-gas at its issue from properly constructed jets. The materials available for these purposes are the following:—Alcohol, ether, camphor, turpentine, and other essential oils, wood spirit, coal tar, naphtha, benzol, petroleum, and other mineral oils, caoutchouc, and similar substances. These matters are used either singly or in combination, and will be better adapted for impregnating the air or gases after they have been rectified by distillation, the products obtained below the temperature of 212 degrees Fahrenheit being preferred. When common air is to be charged with any of these hydrocarbon vapours the supply may be drawn from the atmosphere by fan blasts, bellows, gasometers, air pumps, wind-mills, or any other blowing contrivance. For the special purposes of heating hollow calendering rollers and of singeing textile fabrics, the patentees procure a suitable gas by the decomposition of steam by red-hot charcoal or scrap iron, the apparatus being made self-regulating by the action of a float valve governing the admission of the steam. For general illuminating purposes it is preferred to employ hydrogen generated by the action of acids upon corrodible metals, or the mixture of hydrogen and carbonic oxide as procured by passing steam over heated charcoal; these gases being subsequently rendered luminous by the employment of one of the hydrocarbons enumerated.

The practical suggestions contained in this patent claim include much that has already been accomplished; thus, the mode of rendering air and non-inflammable gases combustible and luminous by impregnation with benzol or naphtha vapour was carried out many years ago by the late Mr. Charles B. Mansfield, as we took occasion to remark in connection with the patent of Mr. E. C. Shepard, dated January 9, 1861,¹ for a similar application. The production of a gas suitable for heating purposes by passing steam over red-hot charcoal is a fact long known and identified under the name of "White's Water Gas." And, if in this last operation iron be substituted for charcoal with a view to the preparation of pure hydrogen, we have but another version of the patent No. 593 to which we refer (page 267) for remarks equally applicable to the present specification. It could be shown, finally, that two of the substances enumerated, viz., alcohol and wood spirit, have not the property of rendering luminous the flame of hydrogen or "water gas."

Grants of Provisional Protection for Six Months.

1796. Joseph Heape Butterworth, Vicars Moss Mills, Rochdale, Lancashire, "An improved mode of, and apparatus for, heating air or for desiccating wet or moist substances."—A communication from Jérôme Eugène

Tourné, New Orleans, Louisiana, U.S.—Petitions recorded July 17, 1861.

2162. James Simmons Matthews, Moore Cottage, Bowling Green Street, Kennington Park, Surrey, "Improvements in the manufacture and preparation of starch, and in the apparatus employed therein."—Petition recorded August 30, 1861.

2363. Henry Cockey and Francis Christopher Cookey, Frome Ironfoundry, Somersetshire, "Improvements in apparatuses employed in the manufacture of gas."—Petition recorded September 21, 1861.

2540. Charles Noyes Kernot, Gloucester House, West Cowes, Isle of Wight, Southampton, and Martin Diedrich Rucker, Fenchurch Street, London, "Improvements in the method of obtaining ammoniacal salts and other valuable products from liquors or substances containing ammonia, and for utilising the residuum."—Petition recorded October 11, 1861.

2605. Hugh Macmeikan, Stratford, Essex, "Improvements in smelting, copper, gold, and other ores."—A communication from Roland Vounder Rodda, Alberton, near Adelaide, South Australia.—Petitions recorded October 18, 1861.

2654. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in preventing and removing deposits in steam boilers and other vessels, and in the apparatus employed therein."—A communication from Joseph Harrison, Philadelphia, U.S.

2656. Isaac Louis Pulvermacher, Oxford Street, London, "Improvements in apparatuses for the production of galvanic and magneto electric currents, and in machinery employed in making some of the apparatuses."

2661. Timothy Morris, Robert Weare, and Edward Henry Craddock Monckton, Trafalgar Square, London, "Improvements in magnets, induction coils, and in insulating wire, and metal for electric and other purposes."

2665. John McCall, Houndsditch, London, and Bevan George Sloper, Walthamstow, Essex, "An improvement in the preservation of articles of food."

2679. James Lobb, Gunpowder Works, Dartmouth, Devonshire, "Improvements in gunpowder suitable for blasting."—Petitions recorded October 25, 1861.

2695. Emory M'Clintock, Mulberry Street, New York, U.S., "Improvements in the manufacture of soda and sulphuric acid."

2707. Frederick Bennett, Bagillt, Flintshire, "An improvement in the manufacture of spelter."

Notices to Proceed.

1695. Peter Spence, Newton Heath, near Manchester, "Improvements in economising the manufacture of sulphuric acid, and in obtaining copper from ores used in such manufacture."—Petitions recorded July 3, 1861.

1709. Osmund Williams, Dursley, Gloucestershire, "An improved mixture or liquid for extinguishing fire, applicable also for preparing or saturating various kinds of materials and fabrics, for the purpose of rendering the same non-inflammable."

1822. Michael Henry, Fleet Street, London, "Improvements in the production of paper pulp, and in bleaching paper pulp and certain fabrics; also in apparatus for cutting wood applicable for carrying out part of the invention."—A communication from Marie Anne Pelagie Veronique Causique, Boulevard St. Martin, Paris.—Petition recorded July 19, 1861.

1845. Nicolas Etienne Dumesnil, Rue aux Loups Sotteville-les-Rouen, France, "Improvements in materials for lubricating parts of machinery, and for other lubricating purposes."—Petition recorded July 23, 1861.

1936. Joseph Lewes, Rathmines, Dublin, "Improvements in producing and treating printing surfaces, in producing and preparing transferring surfaces, in transferring, in producing impressions on an altered scale, in preparing or treating surfaces of lithographic stones, and

in obtaining devices or designs; also in agents and apparatus used in some of such improvements, parts of the invention being also applicable to photography, and to ornamenting pottery, porcelain, and glass."—Petition recorded August 3, 1861.

2162. James Simmons Matthews, Moore Cottage, Bowling Green Street, Kennington Park, Surrey, "Improvements in the manufacture and preparation of starch, and in the apparatus employed therein."—Petition recorded August 30, 1861.

2295. Henry Constantine Jennings, Great Tower Street, London, "Improvements in treating hides and skins."—Petition recorded September 14, 1861.

2482. Thomas Goulston Ghislin, Hatton Garden, London, "Improvements in the treatment or preparation of certain foreign plants or vegetable substances, and in the application of the same to various useful purposes for which horn, shell, whalebone, indurated leather, fish skin, ivory, bone, hard wood, and compounds of india-rubber or gutta-percha have hitherto been employed."—Petition recorded October 4, 1861.

2707. Frederick Bennett, Bagillt, Flintshire, "An improvement in the manufacture of spelter."

CORRESPONDENCE.

Test for the Purity of Acetic Acid.

To the Editor of the CHEMICAL NEWS.

SIR,—I shall esteem it a favour if you would kindly insert the following in your next publication, for the benefit of those interested in the manufacture of pure acetic acid.

Whilst making experiments on pyroligneous acid with a view to make pure acetic acid, I have been at a loss for a delicate test to ascertain when the acetic acid is entirely free from "emphyreuma," which, hitherto, I have found very difficult (when so dilute as not to be tasted or smelt), and I am not aware of a good test for that purpose; but I have found, by neutralizing the acetic acid suspected to contain "emphyreuma," or "emphyreumatic oil," by carbonate of soda or potash and adding a solution of permanganate of potash (Condy's fluid), that, if pure, it remains pinky and unaltered, but if it contains the slightest trace of emphyreuma, the permanganate of potash is immediately decolorized, and after standing a short time, a brown precipitate is produced, but this is not the case when the acetic acid is absolutely pure.

I am, &c.

JOHN LIGHTFOOT.

On the Copper Test for Sugar.

To the Editor of the CHEMICAL NEWS.

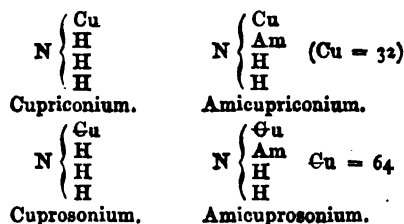
SIR,—In reference to the Copper Test for Sugar, I have just made a few experiments to ascertain its amount of sensitiveness, and I find that a sensible precipitate may be obtained by the $\frac{1}{10}$ of a grain of sugar, which is much greater than Dr. Bence Jones' estimate, which is only $\frac{1}{20}$. This can be proved by boiling a drop of the sugar solution with the copper fluid in a capsule, and then projecting it into a test-glass and observing the precipitate which falls on cooling.—I am, &c.

JOHN HORSLEY.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Compounds of Ammonia with the Salts of Copper and Cobalt.—Schiff considers (*Comptes Rendus*, t. liii. p. 410) that the compounds of ammonium with the salts of copper may be derived from four ammoniums. ($\text{Am} = \text{NH}_4$.)



The author has obtained a variety of salts of the two first bases by decomposing the sulphate of amicupriconium with the various salts of baryta, or by dissolving the cupric salts in ammonia.

The salts of the peroxide of cobalt which contain the triatomic radical, $\text{Co}''' = 59$, give triatomic ammoniums; and the protoxide salts containing the monatomic $\text{Co}' = 29.5$, give a monatomic base, which under the influence of ammonia and oxygen will form an intermediate diatomic base.

The author intends to show in future papers that the compounds of ammonia with the peroxide of chromium, aluminium, iridium, and other triatomic metals, may be described as the above.

II. ORGANIC CHEMISTRY.

Spontaneous Decomposition of Gun-cotton.—M. Bouet (*Comptes Rendus*, t. liii. p. 405) has remarked that the decomposition of gun-cotton in diffused light is preceded by the appearance of a reddish atmosphere, and also that the cotton prepared with nitrate of potash and sulphuric acid is sooner and more violently decomposed than that prepared with the mixed acids. The residue, he says, is also different in the two cases: that of the first is something like burnt sugar, and in the second case is of a straw colour. In both cases the sides of the bottle were nearly covered with small crystals of oxalic acid, and the air contained had an acid reaction. M. Bouet demonstrated the presence of carbonic and formic acids in the atmosphere of the bottle, and he thinks it probable that cyanogen was present. No oxide of nitrogen was discoverable. The solid residue after the removal of the oxalic acid was white, and dissolved in water like gum.

M. Chevreul agreed with M. Bouet, but insisted that light was an active agent in decomposition only when air was present; and among other things he insisted on the importance of light in a sanitary point of view, the salubrity of a house greatly depending on the free admission of light as well as air.

Luca found, however, (*Ibid.* t. liii. p. 298) that gun-cotton which had been kept in the dark had undergone decomposition with the evolution of nitrous vapours. Nearly the whole of this "modified" cotton he found to be soluble in alcohol and in water—14 per cent. in the former and 78 per cent. in the latter. The products of the decomposition he intends to study further.

Transformation of Propionic into Lactic Acid.—MM. Friedel and Machuca (*Comptes Rendus*, t. liii. p. 408) heated propionic acid and bromine in a sealed tube and obtained bromopropionic acid which they found to act readily on oxide of silver in the presence of water. They then passed sulphuretted hydrogen through the silver solution and so procured a strongly acid liquor, which neutralized with oxide of zinc and evaporated yielded crystals exactly resembling ordinary lactate of zinc. Analysis, and the formation of other salts, confirmed the identity of the lactic acid formed as above with that obtained by the usual fermentation process.

Patagonian Guanos.—M. Malaguti gives an account (*Comptes Rendus*, t. liii. p. 436) of a few guanos brought from a group of small islands lying off Port Desire, on the coast of Patagonia. His analyses do not differ much, except in one case, from those published here; but the

guano brought from Penguin Island appears to be remarkable from the amount of phosphate of alumina it contains. The whole amount of phosphates which this guano, according to M. Malaguti, contains never exceeds 35 per cent., and the greater part of this is phosphate of alumina. This, the author remarks, is not a common fact; but what makes it more remarkable is, that in consequence of the presence of this salt, the earthy part of the manure is more soluble in acids before calcination than after. This is an important fact for chemists who are expected to give analyses of manures in a very short time. The phosphate of alumina is seen in the guano as a multitude of small white globules, which are easily reduced to powder by pressure. Only from 4 to 4.35 per cent. of nitrogen is present in Patagonian guanoes.

Blue Colour from Cotton Seed Oil.—Cotton seed oil is bleached by treatment with either carbonate of soda or caustic lime. In both cases a considerable residue is left after drawing off the bleached oil. This residue is treated with sulphuric acid, and distilled at a high temperature. There is then left, according to M. Kuhlmann (*Comptes Rendus*, t. liii. p. 444), a compact mass of a deep greenish blue colour. On further treatment of this mass with strong sulphuric acid, the green tint disappears, and a very intense pure blue colour is produced. Other acids, phosphoric and hydrochloric, will bring about the same change. The blue mass is a mixture of the coloured substance with some sulphuric acid, sulphate of soda, and fats. The two former may be removed by washing with water; the latter by treatment with naphtha. Alcohol now dissolves the blue colour, and water precipitates it from the solution chemically pure.

According to the mean of the results of three analyses, the composition of the new body was

C 70.24
H 8.35
O 21.41:

results which nearly agree with the formula $C_{24}H_{24}O_9$. The new matter could not be obtained crystallized.

Unhappily, M. Kuhlmann has not yet been able to dye with this colour, but he does not despair of fixing it.

Transformation of Cane Sugar into Pectic and Malic Acids.—Schoonbroodt (*Bulletin de la Société Chimique*, &c. No. iv. p. 77) mixed two parts of cane sugar with one of hydrate of lime, and then added three parts of pure and dry chloride of lime. This mixture he made into a thin paste with distilled water, and then let it alone for a time. A lively reaction soon set in, the mixture became heated, and chlorine was evolved. When the swollen mass was afterwards treated with boiling distilled water, it formed a bulky jelly, in which the author found among various other things, pectate of lime. Here is the explanation of the reaction:—



When the quantity of chloride of lime was doubled, a different result was obtained. In this case the reaction was slower, and the solution, though slightly gelatinous, would go through a filter. When this solution was evaporated to a syrupy consistence, and afterwards treated with alcohol, it deposited a white powder, which M. Schoonbroodt satisfied himself was mostly malate of lime. The malic acid, when isolated, behaved exactly like the natural acid. The formation of the malic is explained by the following equation:— $C_{12}H_{22}O_{11} + 3 CaOClO = 3 (C_4H_2O_4, HO) + 2 HO + 3 CaCl$. The author wishes to draw the attention of chemists to the above facts, assured that their researches will fully confirm all he has advanced. He is anxious also to have his claim to priority of discovery acknowledged.

MISCELLANEOUS.

Chemical Society.—The next meeting of this Society will take place on Thursday, December 5, when Papers will be read by Dr. Oppenheim "On the Camphor of Peppermint;" Mr. G. C. Foster "On Piperic and Hydropiperic Acids;" Professor Bolley "On Some Physical Properties of Tin-Lead Alloys;" and "On the Dangers Produced by Feeding Steam Boilers with Certain Waters."

Poisoning Noxious Animals.—On account of the numerous accidents which have occurred from using arsenic and phosphorus for this purpose, M. Séverin Causé, at the request of the French Government, has been pursuing a series of investigations with the object of devising some means of poisoning which should be as efficacious and more safe than those in use. As general principles he lays down these rules:—1. That we must not risk mistakes by employing the poison in the form of biscuits, pills, or powder; 2. An excipient must be used which is repugnant in taste to man but not to animals; 3. Emetic substances should be added, so as, in case of the poison being swallowed accidentally, vomiting may be produced, which is not with gnawing animals like rats and mice; 4. The substance should be easily detectable by chemical analysis; 5. The composition he finally recommended as the best suited as a destructive agent, consists of tallow 786 grammes, tartar emetic 153 grammes, and euphorbium 51 grammes. These, mixed with a pinch of aucturine (added for facility of detection in case of human poisoning), by the aid of 10 grammes of cotton, are made into thirty-two candles, each of which would suffice to poison several rats or mice.—*Annales d'Hygiène*, vol. xv., p. 425.

Scientific Education.—Perhaps there never was a time when scientific knowledge was at a greater premium than it is at present. The system of competitive examination, now so common in the Government departments, and especially in the Indian Service, has created a considerable demand for scientific education. Unhappily, this demand has hitherto been very inadequately met. Scientific instruction has, with rare exceptions, been quite foreign to the ordinary curriculum of our upper and middle-class schools. Amongst the honourable exceptions must be classed the Scientific and Engineering School attached to the College at Chester, under the principalship of the Rev. Arthur Rigg, F.S.A. This gentleman, himself of high scientific attainments, was one of the first, if not the first, to recognise the increasing importance of scientific education to the youth of England, and it was at his suggestion and through his exertions that the Chester School was established. Its success has demonstrated the soundness of Mr. Rigg's views. The school has long been held in high repute by scientific men, and we are glad to learn that the Secretary of State for India has formally recognised it "as possessing an efficient class for civil engineering." There is one feature peculiar to this school which we should like to see more common,—a portion of each day is set apart to work in the laboratory and workshops. Thus, theory and practice are combined. We shall be glad if the success of the Chester School leads to a more general introduction of the scientific element into middle-class education.

Alchemy in the Nineteenth Century.—The following astounding *canard* is being extensively quoted in American papers:—"It has been a great mystery to English bankers, and to the Directors of the Bank of England, how the bullion of the Bank of France could be so greatly increased within the last three years, while the institution has been constantly sending gold to England,

to Germany, and to America. Not long since the Bank of France drew some fifteen millions francs in silver from the Bank of England, which it paid for in gold bars with the French Mint stamp on them. At its last report it showed a balance of one hundred and seventeen million francs in gold, while the amount one year ago was under eighty millions,—nearly one-third increase. It is whispered that this abundance in gold is the result of a scientific discovery, which the Emperor Napoleon has secured the monopoly of. Gold is at the present moment manufactured at Paris in a secret manner. The principal articles used are lead and arsenic, and though it is not known how extensively the precious metal is produced, yet several hundredweight of the material are taken to a certain place on the 1st of each month. Everything is conducted with the utmost secrecy. None of the workmen are allowed to leave, and nothing definite can be known; but the fact that gold is produced is beyond peradventure. How long Napoleon III. will be able to keep this wonderful secret remains to be seen."

Spontaneous Generation.—The question of spontaneous generation is one of the subjects proposed for competition by the Institute, and communications are occasionally forwarded to that body by those who are likely to appear as candidates for the prize. Thus, at a recent meeting, M. Pasteur presented a work in which he endeavours to prove that all organized productions which appear in previously heated infusions, are due to the microscopic organised particles floating in the air. Another communication was also received from M. Jodin. This gentleman states that, on May 12, 1859, he took six glass tubes five inches long, and of a diameter of from seven to eleven lines, and hermetically closed at one end. Each tube received equal parts of crystallized sulphate of soda and water to about one-fourth of their capacity. Three, moreover, received a weight of sugar-candy equal to one third of the sulphate of soda employed. All the tubes were then heated in a water-bath, to liquefy their contents, corked and kept inside a press, in a vertical position. At the expiration of a twelvemonth, M. Jodin examined the tubes and found that the contents of all those which had only received sulphate of soda were totally crystallized, while those which also contained sugar had remained liquid; but this liquid was covered with a pellicle formed by a well-developed mycodermic vegetation, possessing the peculiar greenish hue of the spores of the *Penicillium glaucum*. Five minutes after the corks had been carefully removed the liquid crystallized in one solid mass. Hence it appears that the germs of the genus *Mucedina* (or mildew) may be developed and organised in contact with a solution of sulphate of soda, the particles which are arranged in that peculiar way which constitutes a state of excessive saturation.

Canadian Iron Ore.—In an account which appeared in the *Times* of the 26th inst., describing the loss of the Montreal steamship, *North Briton*, there is an interesting statement made relative to the occurrence of vast deposits of iron ore on Mingan, one of the Paroquite Islands in the Gulf of St. Lawrence, near which the ill-fated vessel struck and foundered. The rocks in the vicinity are said to be so greatly impregnated with iron as seriously to affect the action of ships' compasses. If this observation be correct, it points not only to the existence of vast mineralogical treasure in these islands, but affords hopeful indications in regard to the quality of these beds of ironstone; since the magnetic ores are those which at the same time exert the most powerful attraction upon the compass needle, and are distinguished for their purity and freedom, especially from those contaminations which most seriously prejudice the quality of the iron when manufactured. The finest brands of Swedish and Russian iron are known to be smelted from ores of the magnetic oxide class, and iron is largely produced on the continent of North America from similar

sources. Large supplies of pig iron from the adjoining territory of Nova Scotia have been brought into the English market; and it would be interesting to know whether the beds of ore referred to in the foregoing narrative, have hitherto been turned to practical account.

Arsenical Poisoning.—An inquiry has this week been held by Mr. G. S. Brent, touching the death of Matilda Scheurer, a good-looking girl, aged 19, who died from poison imbibed into the system during her engagement in the manufacture of artificial flower leaves. Mrs. Louisa Scheurer, a widow, and mother to the deceased, deposed that on Thursday week her daughter was taken ill. She was seized with vomiting and the refuse of the stomach was of a very greenish colour. The witness took her to a doctor on the following morning, who told her she was again suffering from the effects of poison. She had been ill several times before, complaining of pains in the stomach and sickness, for the last year and a-half. She was engaged as an artificial florist in the warehouse of M. Bergerond, of Judd Street, Brunswick Square, and up to the time of her death she had been engaged in the leaf-making. She was in the greatest pain until she became insensible, when death put an end to her sufferings. Mr. James Thomas Paul, surgeon, of 26, Burton Crescent, said he had attended the deceased four times within 18 months, while suffering from the same causes. He had made a *post mortem* examination, and found that the lungs gave evidence of the presence of arsenite of copper, the liver being highly impregnated, as also the mesenteric glands. The cause of death was acute inflammation of the mucous membrane of the stomach, produced by the inhalation of the arsenite of copper. A sister of the deceased had died under similar circumstances. M. Bergerond said he employed 98 girls in his establishment, and for their protection he had suggested the wearing of masks, but it was objected by them as producing excessive heat. They, however, wore muslin over their mouths. It was stated by a juror that the arsenite of copper in such manufactures was prohibited in France, as injurious to workpeople. The jury returned a verdict to the effect that deceased died accidentally, from the effects of mortal disease in her stomach and other organs, occasioned by arsenite of copper used in her employment.—*Times*.

New Scientific Society.—A Society has recently been formed in Manchester, under the title of the "Scientific Students' Association." This Society owes its origin to a few scientific tyros, who, having experienced great pleasure and profit from attending the sectional and other meetings of the British Association, during the late visit to Manchester, determined to form a little Association for the benefit of the Manchester lovers of science. Mr. L. H. Grindon consented to act as Chairman, and a Committee and other officers were at once appointed, consisting mainly of the originators of the scheme. The members meet, for the present, once a fortnight, for the purpose of hearing and discussing papers on subjects connected with physical and natural science, which are illustrated, as far as possible, by specimens, experiments, diagrams, the microscope, &c. Excursions will also be occasionally arranged.—*Engineer*.

ANSWERS TO CORRESPONDENTS.

* * All Editorial Communications are to be addressed to the Editor; and Advertisements and Business Communications to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

H. W.—We do not think that there is any waterproof cement known which is sufficiently strong to be used for joining broken false teeth or similar small articles when they are to be subjected to rough usage. You will find a useful series of cements described in Beasley's Druggist's Receipt Book.

Otto Richter.—Received with thanks. In our next.

THE CHEMICAL NEWS.

VOL. IV. No. 105.—December 7, 1861.

THE COMPOSITION OF THE SOLAR SPECTRUM.

In another column will be found two papers on this subject, which merit serious attention on account of the modifications which they appear to render necessary in the recently-suggested explanation of the constitution of the sun's atmosphere. In Kirchhoff and Bunsen's original paper on "Spectrum Analysis," they state, that "the appearance of the spectrum can vary considerably. If the intensity of the light be increased, new lines appear in several of the spectra (of the alkalis), and the relation of the brightness of the old ones becomes altered. In general, an indistinct line becomes brighter upon increasing the illumination, more rapidly than does a brighter line, but not to such an extent that the indistinct line ever overtakes in intensity the brighter one. A good example of this is seen in the two lithium lines." This circumstance appears, however, to have attracted no particular attention, and many of our readers remember the incredulity with which Dr. Tyndall's first exhibition at the Royal Institution, of a magnificent blue band as a constituent of the lithium spectrum, was met by the chemists present. The almost unanimous opinion was, that he had by a *lapsus linguae*, used the word lithium for strontium throughout the evening. Dr. Frankland's article, with Dr. Tyndall's note, given in our present Number, fully clears up the apparent difficulty; whilst, at the same time, it tends to throw doubt upon some of the speculative generalisations which have been deduced from the recently-observed phenomena of the reversal of the bright spectral bands.

Taking Bunsen's estimates of the temperature of flame as correct, it is seen that the Bunsen's-burner flame (2350°C.) is not hot enough to produce the blue line of lithium, but the hydrogen flame (3259°C.) is able to bring it out. An increase of 909 degrees of temperature is, therefore, sufficient to increase by one-half the lines of lithium, whilst the heat of the electric arc raises the number of sodium lines from one to four. The same fact which Dr. Frankland has shown with regard to lithium and sodium we have proved in the case of potassium, calcium, strontium, barium, and copper; so that it may be fairly assumed, as a general law, that all spectra of incandescent elements become more complex as their temperature is raised. Now, the temperature of the sun's surface is, according to the most trustworthy authorities, stated to be equal to that due to the com-

bustion of $\frac{1}{4}$ th of a ton of coals per hour on each foot of surface; whilst Kirchhoff's theory necessitates a temperature at which iron would be a gas. In either case the heat would as far exceed the temperatures which can be produced by artificial agency as the sun's light surpasses our terrestrial means of illumination. What, then, will be the appearance of the spectrum of a metal when exposed to the exalted temperature of the sun's surface? We are not justified in assuming that its growth in complexity stops at a certain temperature, the heat increasing, but the lines remaining the same. Experiment tends to show that the spectrum of a solar element is quite different to that of the same body when experimented upon in our laboratories. It would contain, in addition to the lines capable of being rendered visible by terrestrial means, a vast number of lines which only the solar temperature can evoke; and, consequently, it is exceeding the legitimate inferences to be drawn from experiment, for physicists to assume that the reversal of a few prominent lines in artificial spectra proves the metallic constitution of the sun's atmosphere. Unquestionably certain metallic lines can be reversed, but the black bands thus artificially produced in a continuous spectrum are very dissimilar in sharpness, width, and, in some cases, according to M. Morren, even in position, with the Fraunhofer lines, with which they are supposed to correspond; whilst the sharpness, nearness together, and enormous number of the solar lines are such that it would be difficult to find a line which did not correspond in position to a reversed artificial line.

Granting, however, that the differences of width and sharpness between the artificially reversed and the solar lines may be accounted for by the relative temperatures of the luminous and intercepting flames not being properly proportioned one to the other, we still think that our knowledge is at present too imperfect to justify the classification of a metal as a constituent of the sun on the strength of a few observed coincidences between the solar and the metallic fixed lines. M. Morren has shown that the so-called soda line D is a constituent of several metallic spectra besides that of sodium; unless, therefore, it can be shown that every line capable of being evolved from an incandescent metallic gas at the highest attainable temperature is accurately represented by a black Fraunhofer line, we cannot admit the accuracy of the deductions, but must consider the coincidences as merely accidental, and in no way related to one another in the manner of cause and effect. For instance, the line D being a constituent of the mercury spectrum, the presence of sodium in the sun's atmosphere cannot be proved by this line alone, but must be shown to be probable by an examination of the blue bands mentioned by Dr. Frankland; and in a similar way only can the probable existence of other metals in the sun be shown, not by a reversal and comparison of one or two lines merely, but by proving that every line which it is possible to see at the highest temperatures can be reversed into its counterpart in the solar spectrum.

SCIENTIFIC AND ANALYTICAL
CHEMISTRY.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 283.)

Determination of the Proportional Relation between Silver and Chloride of Ammonium.—

Upon glancing at the determination of the proportional relation between silver and chloride of ammonium performed by M. de Marignac, it is perceived that they differ considerably from one another; between the minimum and the maximum there is a $\frac{1}{100}$ th, — a difference five times greater than the error which belongs to the method followed by this skilful chemist. When I commenced my researches I noticed an analogous fact. After long experimental trials I succeeded in discovering the cause. It is occasioned by the property which sal-ammoniac possesses of absorbing an excess of acid. This salt, which naturally has a very decided acid reaction, even when it has been produced in the midst of a very ammoniacal medium, holds an excess of acid when it condenses after having been very strongly heated in the state of vapour.

Heat in effect decomposes sal-ammoniac, with production of nitrogen and hydrogen, and a part of the hydrochloric acid which is set at liberty condenses with the vapour of the sal-ammoniac. This double property possessed by sal-ammoniac, in common with many other ammoniacal salts, of reddening test-paper, and taking, besides, a slight excess of acid, constitutes an additional difficulty in the determination of the proportional relation of this body. With the object of overcoming this, I attempted to determine in each specimen of the salt the quantity of hydrochloric acid which there was in excess; but I especially made all possible efforts to prepare sal-ammoniac without excess of acid, in order to avoid making a correction which is uncertain.

This uncertainty arises from two causes. The naturally acid reaction of sal-ammoniac, supposing it to be of neutral composition, evidently represents a certain quantity of base; but this quantity is unknown, and there is no means of determining the amount *a priori*, and it is only by comparison that one can acquire the certainty that the degree of acidity is not identical. The method which can be employed to estimate the excess of acid is not susceptible of the precision which it ought to have in order to give absolutely exact numbers. This is evident upon examining my results with attention.

The means which I have employed to estimate this degree of acidity consist in searching how much standard solution of lime must be added to bring a given weight of the salt to neutrality, measured by litmus or turmeric.

The standard lime-water contained per cubic centimètre a quantity of base represented by 0.0003201 grammes of hydrochloric acid. It was, therefore, excessively weak, so as to require a considerable volume of liquid, and also to allow me to appreciate the smallest quantities.

In the specimens of chloride of ammonium which I have prepared for my experiments the degree of acidity varied from the $\frac{1}{100}$ th to the $\frac{1}{1000}$ th part of the weight of the chloride, supposing, be it understood, that the acidity is due to loss of ammonia. But, as I have already observed, the chloride of ammonium formed in the midst

of a highly ammoniacal medium still reddens litmus. This supposition is, therefore, inexact, and the figure obtained necessarily includes an error, the amount of which may even be greater than the minimum quantity stated.

As I believe I have succeeded in preparing at will sal-ammoniac possessing an acidity which is scarcely superior to that which belongs to the definite compound, the difficulty which I attempted to overcome is no longer of any consequence. Indeed, I only mentioned it to explain the disagreement which the results obtained by M. de Marignac show between themselves, and the difference which may be remarked between the first four determinations and the ten following ones.

All chemists have remarked the difficulty which there is in procuring sal-ammoniac capable of volatilising without leaving any residue. Sometimes there remains a carbonaceous matter; sometimes a mixture of silica, alkaline chloride, and oxychloride of iron is obtained. Indeed, commercial sal ammoniac and the ammonia obtained from it contain compound ammonias which furnish this carbon; it also contains chloride of iron. The alkaline chlorides and the silica which is found in the residue of the volatilisation of sal-ammoniac are derived from the vessels in which the operation has taken place.

The precautions which I have taken in the preparation of the chloride to be used in my determinations have had for their object the getting rid of these impurities. It would be too long to detail here all the tedious processes which I have adopted to obtain sal-ammoniac free from all foreign substances; it would, moreover, be without interest. I will content myself with pointing out the methods employed to prepare the products which I have experimented upon.

I. Chloride of Ammonium from Commercial Sal-ammoniac.—I have purified commercial sal-ammoniac with a double object: first, to make use of it in my determinations; and, second, to extract from it pure ammonia with which to prepare the chloride of ammonium required for the treatment of the chlorides of potassium and sodium.

To destroy the compound ammonias existing in commercial sal-ammoniac, I had recourse to the following means:—A boiling saturated solution of sal-ammoniac, freed from iron and other metals by sulphide of ammonium, is mixed with a twentieth of its volume of concentrated nitric acid. The mixture is evaporated away from all organic dust. The residue being dried, is moistened again with a small quantity of nitric acid, and evaporated a second time to dryness. The compound ammonias are thus totally destroyed, with a very notable quantity of the sal-ammoniac itself.

It is from this salt, so purified, that I have extracted the pure ammonia employed in the preparation of the chloride of ammonium used in the treatment of the alkaline chlorides, by combining it direct with pure hydrochloric acid.

To transform the purified salt into pure chloride of ammonium, I sublimed it three successive times at the lowest possible temperature. The salt collected in the neck of the flask during the first sublimation always contains traces of bisulphite of ammonia. On this account I have taken to set apart all the chloride adhering at each sublimation to the neck of the flask. While the sublimed sal-ammoniac remains very warm, it is as transparent as glass, and possesses extraordinary elasticity. Upon cooling, it becomes more or less opaque, and then loses its elasticity in a remarkable manner.

The determinations Nos. I., II., III., and IV. were made with the aid of sal-ammoniac thus obtained. It was perfectly colourless, and volatilised without the least residue. As may be seen by a comparison of the results, they possessed very different degrees of acidity. The specimens submitted to experiment were taken from portions considerably removed one from the other. A portion of sal-ammoniac was sublimed a fourth time, but in an atmosphere of dry ammonia. The result given at No. V. refers to this chloride.

Another portion of the same salt was dissolved three successive times in ammoniacal water, and the salt, after drying, heated until it agglutinated. The result which it yielded on analysis is given at No. VIII.

II. Chloride of Ammonium from the Action of Pure Ammonia and Carbonic Acid upon Pure Chloride of Calcium.—I have had recourse to this mode of preparation in order to obtain at the same time a certain quantity of pure carbonate of lime.

Some beautiful white marble was dissolved in pure hydrochloric acid. The chloride of calcium so obtained was boiled with a little milk of lime prepared by means of lime from the calcination of marble in a platinum vessel. The solution of the basic chloride of calcium was filtered and evaporated to dryness; the residue was fused in a platinum vessel.

This chloride was perfectly colourless. Taken up with cold water, the alkaline and turbid solution having become clear by standing, was poured into pure water in which I had dissolved pure ammonia extracted from sal-ammoniac which had been twice treated with nitric acid. The mixture was submitted to a current of carbonic acid to the complete precipitation of the lime. The colourless and slightly ammoniacal solution was evaporated to a pellicle. The crystallised salt, separated from its mother-liquor, was dried and then twice sublimed. It was absolutely colourless at the first sublimation, and left no trace of residue at the second sublimation. No. VI. refers to the result furnished by this salt.

A portion of the preceding salt was dissolved in ammoniacal water and the solution evaporated *in vacuo* over fused potash. The resulting chloride was sublimed in platinum at the lowest possible temperature. No. VIII. gives the result of this experiment.

Another portion of the same salt was dissolved in ammoniacal water. After the crystallisation of the chloride, the alkaline mother-liquor was decanted, and the salt, which already had a decided acid reaction, was exposed under a bell-jar with caustic potash. After thorough drying, it was submitted, in the same tube in which it was to be weighed, to a current of pure dry ammonia for an entire day. The next day the current of ammonia was replaced by air freed from all foreign substance, and the current was continued for several hours after the air had ceased to carry off ammonia. The result of this experiment, made with extreme care, is found at No. IX.

III. Chloride of Ammonium Prepared by the Direct Combination of Pure Ammonia with Pure Hydrochloric Acid.—The ammonia displaced by an alkali from the chloride purified by means of nitric acid and carefully washed in water, was dissolved in water contained in a platinum vessel. The liquid, the ammoniacal odour of which differed completely from that of ammonia extracted from commercial sal-ammoniac, was then nearly neutralised with pure hydrochloric acid. Whatever might be the excess of ammonia existing in the liquid, the salt which was deposited by the action of the hydrochloric acid always presented a

decided acid reaction. This salt was dried in a current of dry ammonia and the ammonia then eliminated by a current of air. It took ninety-six hours to drive off the last trace of condensed alkali. The result given at No. X. refers to this salt.

This same chloride of ammonium volatilised in a platinum vessel in an atmosphere of ammonia and then weighed *in vacuo* gave the result at No. XII.

Wishing to satisfy myself of the influence of the ammoniacal atmosphere during the volatilisation of the salt, I sublimed the preceding chloride of ammonium a second time in platinum, the salt being weighed equally *in vacuo*. The result I obtained is shown at No. XIII.

Finally, I wished to form sal-ammoniac from ammonia produced by the reduction of nitric acid. With this object, I reduced nitric acid by a mixture of zinc, sulphuric acid, and water. The solution of the sulphates of zinc and ammonia thus obtained was concentrated and then poured on to hydrate of lime contained in a large retort. The mixture was heated, to drive off the liberated ammonia, and the gas, after being washed in water, was dissolved in pure water. The ammoniacal solution presents, as to its odour, the same difference from the ammonia extracted from commercial sal-ammoniac that I have recently mentioned in the case of that derived from the purified salt. It is incontestable that the gas possesses a much purer and infinitely less disagreeable odour.

This ammoniacal solution was nearly saturated with pure hydrochloric acid and allowed to crystallise under a bell-jar with solid caustic potash. The alkaline mother-liquor was poured off and the salt dried under a bell-jar with solid potash, and then in a current of dry ammonia, afterwards replaced by a current of dry, cold air.

The resulting chloride of ammonium was of a dazzling whiteness. It was weighed in the air and gave the result at No. XI.

I evaporated to dryness the voluminous mother-liquors from which the salt had been deposited, and mixed the resulting chloride with a great excess of fused and pulverised potash. The ammoniacal gas, dried by passing through two long U-shaped tubes filled with fused and pulverised potash, was directly combined with dry, pure hydrochloric acid, in a platinum digester holding five litres, taking the precaution always to keep the ammonia in excess. The sal-ammoniac produced, which was of extreme whiteness, was heated in an atmosphere of dry ammonia until it cohered together. It was then introduced into a glass tube and submitted to a current of cold and dry air until it had lost all the condensed ammonia. It was then weighed *in vacuo* and furnished the result given at No. XIV.

To enable the following Table to be understood, I must explain that it first shows the proportional relation between silver and sal-ammoniac given by the experiments, without taking account of the excess of acid which this compound retains or may retain; and, second, according to the hypothesis that the excess of acid is saturated with ammonia, and that the weight of the salt is proportionally increased. From a comparison of the two results, it is evident that chloride of ammonium can retain an excess of hydrochloric acid, but they show, at the same time, my inability to measure exactly the degree of acidity.

Upon examining this Table, it will be seen that the results are nearly constant from No. V. When I executed the first four determinations I was ignorant of the property possessed by chloride of ammonium of retaining an excess of acid under the above-named circum-

Proportional Relation between Silver and Chloride of Ammonium.

Numerical Order.	Weight of the Sal-ammoniac in air.	Weight of the Sal-ammoniac reduced to a vacuum.	Weight of the Sal-ammoniac reduced to a vacuum and calculated as neutral.	Weight of the Silver in air.	Weight of the Silver reduced to a vacuum.	Weight of the excess of Silver after double decomposition.	Weight of the Sal-ammoniac corresponding to the excess of Silver.	100,000 parts of Silver equivalent to Chloride of Ammonium:	
								A. Calculated from the crude Salt.	B. Calculated from the supposed neutral Salt.
I.	grammes. 3'3885	grammes. 3'3911	grammes. 3'39373	grammes. 6'8504	grammes. 6'85083	grammes. 0'00950	grammes. 0'004712	49'568	49'606
II.	3'73025	3'73312	3'73437	7'54061	7'54108	0'01290	0'00645	49'581	49'605
III.	3'00675	3'00906	3'01141	6'0791	6'07872	0'0090	0'004464	49'572	49'610
IV.	4'99675	5'00060	5'00398	10'10115	10'10093	0'01505	0'00742	49'577	49'611
V.	6'2398	6'2446	6'24652	12'6090	12'6098	0'01875	0'00930	49'595	49'610
VI.	8'0765	8'0827	8'0852	16'3206	16'3216	0'0224	0'01110	49'588	49'604
VII.	3'99975	4'00283	4'00435	8'08304	8'08354	0'0119	0'00595	49'591	49'608
VIII.	1'92130	1'92278	1'92333	3'88237	3'88261	0'0055	0'00273	49'593	49'606
IX.	11'0004	11'0088	11'0103	22'22200	22'22336	0'0300	0'01490	49'600	49'608
X.	10'92155	10'92896	10'93151	22'06600	22'06734	0'0280	0'013888	49'599	49'602
XI.	12'25095	12'26038	12'26064	24'74838	24'74991	0'0305	0'01512	49'598	49'603
XII.	22'8606	22'8769	22'88778	46'1777	46'1814	0'0567	0'02812	49'597	49'600
XIII.	23'9534	23'9714	23'9734	48'3878	48'3908	0'0533	0'0262	49'591	49'599
XIV.	24'9272	24'9257	24'9278	50'3141	50'3173	0'0551	0'0273	49'592	49'601
Mean of the last nine Experiments ...								49'5944	

stances. In the ten following operations I succeeded in avoiding the excess of acid. Only the latter experiment could show if the compounds that I employed in the determinations could not in their turn retain an excess of ammonia. I can only say that I have used all my endeavours to avoid this cause of error.

Upon comparing the results furnished by those specimens of chloride of ammonium which were crystallised from a liquid, a slight difference may be remarked according to whether the weighing was effected in air or in a vacuum. The weighing in air is affected by an error on the plus side, owing to the condensation of this fluid by the salt. This cause of error will be easy to rectify by a subsequent experiment; I perceived it too late to take account of it.

If it is wished to calculate the mean of my experiments, it seems to me, in this case, the last ten only ought to be taken into consideration. This mean is 49'5944. Whence it results that the proportional relation between silver and chloride of ammonium is as 100,000 to 49'5944.

Determination of the Proportional Relation between Nitrate of Silver and Chloride of Potassium.—I have attempted to determine the proportional relation between nitrate of silver and chloride of potassium, in order to submit my syntheses of nitrate of silver to a rigorous control. It may be conceived that if the salt produced in these syntheses retains an excess of acid or of water, its presence could be proved in this manner. This research, moreover, would serve to verify Prout's law.

On this subject I have made three series of experiments. The first was executed with nitrate of silver prepared from pure silver, and which, after having been fused, was crystallised a great many times. The last crystallisation was hastened by cooling the liquid rapidly, so as to obtain the salt in a fine state of division. The salt, after having been kept away from the light for more than six months, under a bell-jar, with sulphuric acid, was fused and kept in this state in a current of air deprived of organic matter until its weight was constant.

The second series was made with the nitrate of silver described under No. VI. of the Table of the synthesis of this salt (CHEMICAL NEWS, Vol. IV. p. 244).

For the third series I employed a new nitrate of silver, obtained by dissolving in nitric acid the silver produced by the electrolysis of argento-cyanide of potassium. The salt was fused and dried as in the synthesis of the nitrate. The chloride of potassium employed was a portion of the same body which had served to establish the comparison with silver.

The nitrate of silver of the three experiments was white, lamellar, and of a pearly lustre. Its solution was neutral to test-paper. Before being weighed it was broken into small fragments, then heated in a platinum crucible, and introduced, as warm as possible, into the tube in which it was to be weighed after cooling. The tube was placed under a bell-jar with concentrated sulphuric acid. At each weighing of the nitrate I have stated the uncertainty which it presents. Nothing is easier than to get the weight of ten, twenty, or thirty grammes of chloride of potassium to almost the tenth of a milligramme; but, on account of the hygrometric property of nitrate of silver when fused and broken up into small fragments, I am not certain of the weight of an equal quantity of this salt to about one and a-half milligrammes. It was on this account that I determined to make a third series of experiments, weighing the broken-up nitrate of silver this time *in vacuo*, previously heated and then cooled likewise in a vacuum.

The weights of the nitrate and chloride were taken according to the relation deduced from Prout's law, taking for the equivalent of nitrate of silver 170, and that of chloride of potassium 74'5.

The nitrate was first introduced into a flask, and then dissolved in thirty-five times its weight of pure water, to ascertain its complete solubility.¹ The solution was mixed with four cubic centimetres of pure nitric acid per gramme of nitrate, and decomposed by the alkaline chloride. The operation was then concluded absolutely

¹ Nitrate of silver may contain traces of chloride and still produce a clear solution if dissolved in less than ten times its weight of water.

in the same manner as for the proportional relation between silver and the alkaline chlorides.

Proportional Relation between Nitrate of Silver and Chloride of Potassium.

Numerical Order.	Weight of the Chloride of Potassium in air.	Weight of the Chloride of Potassium reduced to a vacuum.	Weight of the Nitrate of Silver in air.	Weight of the Nitrate of Silver reduced to a vacuum.	Weight of the excess of Nitrate of Silver.	Weight of the corresponding Chloride of Potassium.	100,000 parts of Silver equivalent to Chloride of Potassium.
First Series.							
I.	grms. 4.7218	4.72461	grms. 10.77850	10.78100	grms. 0.01321	0.00381	43.878
II.	5.1431	5.14618	11.74020	11.74295	0.01400	0.00315	43.875
III.	10.082	10.086	23.0100	23.0155	0.02709	0.01190	43.875
IV.	15.3118	15.3210	34.9532	34.9614	0.04142	0.01817	43.874
						Mean ..	43.8755
Second Series.							
V.	4.5159	4.5186	10.1086	10.1110	0.0063	0.00425	43.864
VI.	8.0457	8.0483	20.4140	20.4190	0.02126	0.00996	43.869
VII.	13.6911	13.7014	31.2514	31.2650	0.03685	0.01600	44.876
						Mean ..	43.870
Third Series.⁽³⁾							
VIII.	7.9796	7.9844	18.2210	18.2195	0.0112	0.0112	43.884
IX.	11.4214	11.4302	26.0855	26.0853	0.0123	0.0154	43.878
X.	12.5890	12.59655 ⁽²⁾	28.7440	28.7438	0.0256	0.0177	43.885
						Mean ..	43.885
						Mean ..	43.8758

(1) In the Third Series all the weighings were performed in *vacuo*.
(2) Thus in the original paper: evidently a misprint for 12.59655.
(3) Determination of the Proportional Relation between Nitrate of Silver and Chloride of Ammonium.

I have sought for the proportional relation between these two bodies to control, at the same time, my syntheses of nitrate of silver and the relation which I found to exist between this metal and chloride of ammonium. I took for these determinations a portion of the nitrate of silver which served for the first two series of experiments with chloride of potassium.

I employed the sal-ammoniac of No. XI. for No. I.; of No. XII. for No. II.; and of Nos. XIII. for Nos. III. and IV.

Proportional Relation between Nitrate of Silver and Chloride of Ammonium.

Numerical Order.	Weight of the Chloride of Ammonium in air.	Weight of the Chloride of Ammonium reduced to a vacuum.	Weight of the Nitrate of Silver in air.	Weight of the Nitrate of Silver reduced to a vacuum.	Weight of the excess of Nitrate of Silver.	Weight of the corresponding Chloride of Ammonium.	100,000 parts of Silver equivalent to Chloride of Ammonium.
I.	grms. 6.56790	6.56144	14.64974	14.65117	0.00098	0.00285	31.489
II.	6.98077	6.9774	22.1707	22.1819	0.01400	0.00440	31.490
III.	7.9001	7.91217	25.1372	25.1430	0.01496	0.00470	31.487
IV.	6.5788	6.5819	20.9171	20.9220	0.01230	0.00380	31.486
						Mean ..	31.488

(To be continued.)

Chemical Researches on the Assay of Silver by the Moist Way, by G. J. MULDER, *Math. Mag. Phil. Nat. Medicinæ, Artis Obstetriciæ et Pharmaceuticæ Doctor, Professor of Chemistry at the University of Utrecht, Grand Cross of the Order of the Netherlands Lion; of the Luxembourg Order of the Couronne de Chêne; of the Swedish Orders of Wasa and Polar Star, &c., &c.*

(Continued from page 232.)

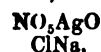
In both the experiments above mentioned,—viz., 1 gramme and 3 grammes silver, temperature 15° to 17° C., the quantities of 20 and 60 drops of the decimal solutions were respectively required in order to produce, just to the extreme limit thereof, the mutual precipitate. 20 drops are exactly contained in 1 cubic centimetre; hence what holds good for 1 gramme of silver holds good also for 1 milligramme of silver, or an equivalent quantity of common salt,—viz., 0.54 milligramme; for 3 grammes of silver it likewise holds good for 3 milligrammes of silver or 1.62 milligrammes of chloride of sodium. With these quantities, viz.:

1 milligramme of silver, 3 milligrammes of silver, or 0.54 milligramme of salt or 1.62 milligrammes of salt are equivalent:

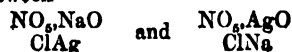
1.33 milligrammes ClAg; 3.99 milligrammes ClAg;

These, to be sure, are quantities which may even be of some consideration in the practice, while as far as pure science is concerned, it is the indication of the measure of that quantity, which gets withdrawn from the precipitation.

We will now see more about the solubility of chloride of silver in solution of nitrate of soda. We have already seen that, at least at higher temperature, this solubility of chloride of silver in nitrate of soda does really exist. If we are forced to assume that, according to what we have already learned, the solution, whether brought to the neutral point, or above, or below it, contains,—



then the whole matter is elucidated, provided the solubility of chloride of silver in nitrate of soda be proved at the same time, for nobody can practically demonstrate a difference between



It is first to be seen to what amount the solubility of chloride of silver in nitrate of soda ought to go in order properly to account for the phenomenon.

1 milligramme of silver yields 1.33 milligrammes of chloride of silver. 0.541 gramme chloride of sodium in 100 cubic centimetres of the standard solution, yield 0.787 grammes nitrate of soda dissolved in 100 cubic centimetres of water. Under the circumstances just mentioned, 0.787 milligramme of nitrate of soda dissolve 1.33 milligramme of chloride of silver, or 592 parts of nitrate of soda 1 part of chloride of silver.

If chloride of silver, at a temperature of from 15° to 17° C., is to such an extent soluble in solution of nitrate of soda, then the whole phenomenon must be referred to solution,—viz., in the sense defined by Berthollet.

This solubility of chloride of silver in nitrate of soda was rendered more evident by the following experiments, which also were made to illustrate the practical bearing of the phenomenon here spoken of:—

1. One gramme of pure silver was dissolved in 5 cubic centimetres of nitric acid, and afterwards just brought

to the utmost limit of precipitation, first by means of 100 cubic centimetres of the standard solution, and then a few drops of the decimal solution of salt. The temperature was 17° to 18° C. After this 22 drops of decimal solution of silver, and again 22 drops of decimal solution of salt were needed in order to precipitate every time unto the limit.

II. The same experiment as that just mentioned was made at the same time with 1 gramme of pure silver also, but to the solution of silver were added 2 grammes of pure crystallised nitrate of soda, 100 cubic centimetres of the standard solution, and a few drops of the decimal solution of salt produced the precipitate to the utmost limit. When, now, immediately afterwards, the decimal solution of silver and salt were added in every case to the limit of perceptible precipitate, then with silver again, and again salt, the drops being added two by two, the following result was obtained:—

Silver.	Salt.
A 38	B 38
C 38	D 38

Instead of 1.1 cubic centimetres or 22 drops, as in Experiment I., there was required 1.9 cubic centimetres of the solutions of silver and salt in order to produce the precipitate to the limit. From the common salt contained in 100 cubic centimetres of the standard solution, 0.787 grammes nitrate of soda is made by its reaction upon the nitric acid of the nitrate of silver. In every ordinary assay, therefore, that quantity of nitrate of soda is present.

In Experiment II. 2 grammes of nitrate of soda had been added, so that with the 0.787 gramme formed during the assay, 2.787 grammes were present. If we wish to assume the solubility of chloride of silver in nitrate of soda, we are already previously informed that in an ordinary assay 1 part of chloride of silver is soluble in 592 parts of nitrate of soda dissolved in 100 cubic centimetres of water. Experiment II. taught us that 2.787 grammes of nitrate of soda dissolve 1.9 milligramme of silver, equal to 2.527 milligrammes of chloride of silver, —i. e., 1 part of chloride of silver finds 1103 parts of nitrate of soda dissolved in 100 cubic centimetres of water.

There remains now to inquire what effect water alone has in dissolving chloride of silver. In order to elucidate this, two experiments were made at the same time and under fully the same circumstances, the only difference being that 100 cubic centimetres of pure water were mixed with the solution of silver, which contained 1 gramme of pure silver. The solution to which water had been added required 5 drops more of the decimal solution of salt than the solution prepared without this extra addition.

In this experiment, made at 16.5° C., we see that the water alone, which dilutes the nitrate of soda, increases the solubility of the chloride of silver in the former salt. On applying the decimal solution of silver, in order to bring the solution last mentioned just to the limit of precipitability, but no further, I found that 29 drops were required for the solution to which 100 cubic centimetres of water had been added, while the other only required 20 drops. The difference of nine drops, expressed in silver, is 0.45 milligrammes,—i. e. 0.6 milligramme more of chloride of silver had been dissolved by 100 cubic centimetres of water.¹ From these particulars, in connection with those previously stated, it follows:—

¹ We see there is a difference of half a drop between 5 and 9; 5 must either be 4, or 9 must be 10. From what has been already said, it is evident that the one must be just the double of the other.

1st. That it is in fact the nitrate of soda which gives rise to the production of the mutual precipitate of silver and salt.

2nd. That when the quantity of nitrate of soda is increased in the solution, the quantity of dissolved chloride of silver is also increased.

3rd. That the quantity of water present at the same time has an influence as well as the quantity of nitrate of soda.

I have, in speaking of this phenomenon, used the expression "solution of chloride of silver in nitrate of soda." It will hardly be necessary to remind the reader that this is precisely an instance of what has been more clearly indicated by Berthollet. What takes place in the experiments for the assay of silver on a smaller scale, finds place on a larger scale when, for instance, sulphate of potassa and chloride of calcium, both in solution, are mixed. Then there is formed chloride of potassium and sulphate of lime; but the sulphate actually precipitated is far less in quantity, than what has been formed in reality. We will keep, for simplicity's sake, the word "solution," the true signification of which we have given in the earlier pages of this treatise.

I subjoin the quantities of chloride of silver in relation to the solvent proportions of salt and water, as found by experiments; temperature 15° to 17° C.:—

Soluble Chloride of Silver. Milligrammes.	Nitrate of Soda. Grammes.	Water. Cubic centimetres.
1.33	0.787	100
1.93	0.787	200
3.99	2.361	300
2.53	2.787	100

The first of the above series of figures also refers to the quantities of chloride of silver, which may be precipitated by either solution of chloride of sodium, or nitrate of silver, both in equivalent proportions to each other, and therefore also equivalent with the chloride of silver of the first column, from liquids of the same composition as referred to in the second and third columns above mentioned.

The solubility of chloride of silver in nitrate of soda is no more to be relied on as soon as the chloride of silver has been discoloured by the light, for then there has been set at liberty some chlorine; hence the quantity of silver required is larger than what is equivalent to the last-added quantity of salt; while, secondly, the solubility of that chloride of silver which turned violet is greatly decreased. In the liquid obtained by dissolving one gramme of pure silver, to which precisely enough solution of salt had been added, in order to be brought to the neutral point, I observed, after twenty-four hours, when the chloride of silver had become pretty violet, that 0.575 milligrammes of silver solution were required to neutralise the chlorine set free; while I also found that the quantities of decimal solution of silver and salt which are required to precipitate each other mutually at 17° C. to the utmost limit, decrease from 20 drops or 1 cubic centimetre, to 13 drops, or 0.65 cubic centimetre.

I do not doubt a single moment that chloride of silver (provided it be first decomposed by light as much as possible) is insoluble in nitrate of soda. The remarkable phenomenon bearing upon the assay of silver just brought under notice deserves to be considered also from other points of view.

(To be continued.)

TECHNICAL CHEMISTRY.

On the Carburation of Gas.

THE following Report has been addressed to the Commissioners of Sewers of the City of London, by their Engineer, Mr. W. Haywood :—

"In pursuance of the resolution of the Court of the 23rd April last, I have directed an experiment with the view of testing the value of the application to the public lamps, of the process patented by the United Kingdom Carburing Gas Company.

"The patent of this Company is for placing near to the gas burners a receptacle containing coal naphtha, the gas passing through or over this takes up, and becomes enriched by the addition of the volatile hydrocarbons contained in the naphtha, and the illuminating power of the gas is thereby increased; the quality of the naphtha employed determines mainly the degree of illuminating power gained, and the chemical and photometrical experiments laid before me show that it varies from 25 per cent, to 77 per cent. (See Appendix.)

"The experiment being one in which the Gas Companies are much interested, I applied to the Chartered Company, who light Moorgate Street, suggesting their co-operation in conducting it with me; the suggestion was at once acceded to, and their Inspector, Mr. Johnson, was placed in communication with me for that purpose.

"Moorgate Street was selected as well adapted for the experiment, there being an equal number of lamps upon each side of the way, but one or two private lamps only in the street, and but few shops; the street is, therefore, after an early hour of the evening almost entirely without artificial light, excepting that which it derives from the public gaslights.

"The patentees stating that by the application of their process equal light would be given with half the ordinary consumption of gas, the burners were regulated accordingly.

"The lamps experimented upon were twelve in number, six upon the western side, which were fitted with the ordinary batwing burners, calculated to consume upon the average of the night 5 cubic feet of gas per hour, and six upon the eastern side, fitted with batwing burners, calculated to consume $2\frac{1}{2}$ cubic feet per hour. The latter burners having attached to them the carburing apparatus of the Company, each of the twelve burners had a meter attached to it, to ascertain the actual consumption. No pressure regulators were fixed upon the lamps.

"The registration commenced upon the 19th of June, and terminated upon the 19th of July inst., the experiment extending therefore over thirty nights, and gave the following results.

"That the burners without the carburing apparatus consumed about 4.39 cubic feet per hour.

"That the burners fitted up with the carburing apparatus consumed 2.09 cubic feet per hour.

"No photometer was employed, the equalisation of the amount of light given by the two classes of burners was a matter of judgment. The District Inspector of the Commission, who saw the lights nightly, reports his opinion that the light given was perfectly equal, and that his opinion is strengthened by collecting those of certain residents in the neighbourhood. My own opinion is that the light of the $2\frac{1}{2}$ feet burners was upon the average of the month inferior, although but very

slightly so, to that of the 5 feet burners. The Inspector of the Chartered Company coincides with me in this.

"No chemical analysis was made of the naphtha used; but it is stated by the patentees to have been of the best quality.

"My deduction from the experiment is, that with naphtha of equal quality to that used, that during the warm months of the year 3 cubic feet of carburated gas may be considered as about equal to 5 cubic feet of gas not carburated.

"Assuming this to be data applicable to all seasons of the year, I have estimated the saving to be effected by the process, and, after allowing for the cost of the apparatus, and for periodically filling it with naphtha, and, after giving credit at the present price of the gas supplied to the public lamps for the quantity not consumed, it shows that the reduction in the cost of each public lamp will be at least 1*l.* per annum; and there being 2825 lamps within the City, that a saving of 2825*l.* would be annually effected.

"The only disadvantage observed during the experiment was that the reservoir, as constructed, throws a disk of shadow round the base of the gas lamp standard, but the depth of shadow is but slight; this disadvantage may be largely rectified by an alteration in the form of apparatus.

"It should be understood that I do not pledge myself to any of these figures as exact, for the experiment as conducted cannot lay claim to be considered minute or exact in its character; but I believe it may, nevertheless, be taken as giving a close approximation to the truth; it is the mean of the rough results of practice, and the refined processes of the laboratory from which reliable data are generally drawn; in this case the results of the experiment are supported by laboratory experiments, and consequently there seems but little doubt that this mode of applying naphtha to the public lights (for the naphthalisation of gas itself is by no means new) may lead to a considerable reduction in the cost of public lighting; but what that reduction ultimately would be, would depend upon points which can only be determined by the application of the process to a considerable number of lamps for some length of time, and at different seasons."

The following is Dr. Letheby's Appendix to the above Report :—

"The apparatus consists of a chamber for holding coal naphtha, and of a contrivance for directing the stream of gas over the surface of the naphtha. By this means the gas becomes charged with volatile hydrocarbons, and acquires a higher illuminating power.

"Three sets of experiments were made for the purpose of determining the value of the apparatus. In the first set a naphtha rich in benzole was employed, and the results were, that at first it raised the illuminating power of ordinary twelve-candle gas to twenty-four candles, and in the course of three days the power fell to eighteen candles, the mean of the whole being twenty-one candles. This is an increase of 77 per cent., and it was effected by giving 10.77 grains of naphtha to each cubic foot of gas.

"In both of the other sets of experiments an inferior kind of naphtha was used, and in one case the average increase of illuminating power, during a period of ten days, and after the passage of a thousand cubic feet of gas, was 25 per cent. In the other case, after a duration of five days, the average increase was 30 per cent. The former was effected by the addition of four grains of

naphtha vapour to each cubic foot of gas, and the latter by 6.56 grains.

"These data are sufficient to indicate the general capabilities of the apparatus; for they show that with a good naphtha, supplied in proper quantity, and furnishing from ten to eleven grains of vapour to each cubic foot of gas, the illuminating power of an inferior gas may be nearly doubled. A less volatile naphtha, giving only from four to seven grains of vapour per cubic foot, will increase the power of twelve-candle gas from 25 to 30 per cent. I am, therefore, of opinion, that the apparatus is of practical value as a carburetting agent, and that if supplied with good naphtha, in proper quantity, there will be no difficulty in sustaining a power of twenty candles with ordinary coal gas."

Upon receiving this Report and Appendix, the Commissioners of Sewers resolved that it should be referred to the Engineer and Medical Officer of Health, to consider the conditions of the contracts for public lighting; having special reference to the increased illuminating power of the gas to be supplied, and to the possibility of carburing the gas by the process of the Carburing Company.

These gentlemen, Dr. Letheby and Mr. Haywood, have now reported upon this subject in the following terms:—

"Before considering the general conditions of a contract, it is necessary firstly to obtain the determination of your honourable Board to the leading principles upon which the contract should be framed, and it is to those we now specially address ourselves.

"As regards that portion of the reference which relates to the possible reduction of the consumption of gas in the street lamps, we are of opinion that, if the carburing process is not applied, the increase of the illuminating power proposed by the Metropolis Gas Act of 1861, does not render it expedient to diminish the amount of gas to be supplied at the burners of the public lamps; and that the contract should therefore remain as heretofore in this respect, unless the Companies alter the quality of the present supply, and furnish Cannel gas to the public lamps, as the Act of Parliament empowers them to do: under which circumstances it will be necessary to readjust the contract and mode of supply accordingly.

"With regard to the carburing process, we are of opinion, from the data obtained by the laboratory experiments quoted in the report to the Commission of the 30th July last, and the experiments made on the public lamps in Moorgate Street, during the months of June and July last, that the process of carburation appears to be capable of economising the use of gas in the public lamps, to the extent of from 40 to 50 per cent. This conclusion is founded on the assumption that the best quality of naphtha is to be used, namely, a naphtha which will give to the gas continuously a proportion of about ten grains of volatile hydrocarbon to each cubic foot of gas: these being the average results of the laboratory experiments. If an inferior kind of naphtha be employed, the results will be less satisfactory; for the laboratory experiments show that a naphtha yielding four grains of volatile hydrocarbon will increase the illuminating power of the gas to only about from 15 to 20 per cent.

"It is manifest, therefore, that the practical efficacy of these results will be entirely dependent on the perfection of the apparatus and the quality of the naphtha,

and we are of opinion that these essential conditions can only be secured during the earlier application of the process by an arrangement with the Carburing Company for the supply of the apparatus and the naphtha, as also for the maintenance of the same in complete working order, according to the terms of a contract founded on the preceding data, namely, that a burner consuming three feet of the naphthalised gas per hour shall give continuously the light of a burner consuming five feet per hour of the same gas not naphthalised; and to secure this, the naphtha should be of such quality as to furnish continuously not less than seven grains of volatile hydrocarbon to each cubic foot of the gas. If the Company is willing to undertake such a contract upon suitable terms, we see no difficulty in the practical application of the system.

"If these suggestions are adopted, it will be necessary to contract both with the Gas Companies and the Carburing Company; the terms of such contracts, which should have due relation to each other, must be a matter for future consideration."

PHARMACY, TOXICOLOGY, &c.

On Kerosolene, by EDWARD PARRISH.

SOME of the most useful scientific and practical discoveries have originated in accidents, the first results of which have involved disappointment or inconvenience. To evolve from these the lessons by which they may be made to subserve useful ends should be one of the chief aims of the experimentalist.

To the accidental anæsthetic effects produced upon a workman employed to clean a cistern in a kerosene oil works in Boston we owe the recent discovery of a new use for an abundant product of the destructive distillation of coal. This product, though heretofore procured in a crude condition at a certain stage of the process, in the works alluded to and perhaps in others, has not until recently been rectified and thrown into the market, because no demand existed for it for any known use of which it was capable.

With the enterprise so characteristic of New England character—a trait which so happily gave to science and humanity one of the greatest blessings of modern times—ether as an anæsthetic—the Boston physicians have taken measures to investigate the utility of this article for similar purposes, pending which it naturally claims the attention of pharmacutists in its physical and chemical characters.

Two bottles of the liquid have been sent me, the first by a medical friend residing in Massachusetts, and the second, at my request, by the manufacturer, Joshua Merrill, superintendent of the Downer Kerosene Oil Company, from whom I learn that kerosolene, as it is named, is a product of the destructive distillation of "Albert coal," at temperatures varying from 600° to 890° F. The very volatile portion, separated by careful fractional distillation from the more fixed products which are sold as kerosene, is purified so as to remove its unpleasant odour, and thoroughly rectified.

As thus produced, kerosolene is a tasteless, colourless, though highly refractive, inflammable liquid, burning with a bright smoky flame. It is very mobile and volatile, and the bead disappears immediately. A vial, when grasped by the palm of the hand, gives off small bubbles of vapour, raising the stopper. Its odour is very faint, reminding of chloroform at first, though less

agreeable as it evaporates, leaving not the slightest odour when it dries. Its sp. gr. varied in the two specimens at first received, that of No. 1 being .6346 at 76° F., of No. 2 .6325 at 72° F.; but by exposure for two weeks in a partly filled tincture bottle, occasionally opened, the specimen marked No. 1 reached a sp. gr. of .6420 at 72° F.

From the last named fact and its known origin and mode of preparation, it would be supposed that this liquid had not a uniform composition, but must be a mixture of different carbohydrogens of varying density and volatility. The truth of this was proved by the following experiments:—Specimen No. 1 was heated in a water bath, and its boiling points carefully noted. At 84° F. it commenced to boil, the temperature rapidly rising to 90°, and afterwards slowly, without becoming stationary, to 150°, when boiling ceased, about one-sixth of the original liquid remaining, which was evaporated at about 200° without boiling. Specimen No. 2 was subjected to similar experiments. It commenced to boil at 82° F., ceased at 94°, recommenced at 100°, and ceased at 105°, when the remainder, about two-thirds, was evaporated without boiling, at a temperature not exceeding 180°.

The extremely low sp. gr. of this substance and the temperature at which it begins to boil forbid the idea of its containing the ordinary coal tar products in considerable proportion. The light naphtha first obtained in the distillation of coal tar has a sp. gr., according to Mansfield, of .900 to .950, and when purified by agitation with sulphuric acid and potash, or by the process of Gregory, in which peroxide of manganese is used, and rectified, its density does not fall below that of benzene, the sp. gr. of which, in its liquid form, is variously stated at .85 and .8991, at different temperatures.

The largest proportion of kerosolene is evidently composed of those very light carbohydrogens not present in coal tar, and not produced at the high temperatures employed in the production of illuminating gas.

In the books I have consulted I find no detailed account of these almost gaseous liquids, and I can imagine no subject presenting more difficulties to the chemist. Professor Antisell, in his thoroughly practical work on Photogenic and Hydrocarbon Oils, remarks that "the specific gravity and chemical constitution of the light and heavy oils vary in relation to the temperature at which they are distilled; and perhaps no two distillations give exactly the same relative mixture of the various hydrocarbons of which they are composed." The composition of these is further varied by the nature of the crude materials, which may be highly bituminous, like the "Albert coal," from which the specimens under consideration were obtained, or more completely formed coals like the more common varieties.

The proportion of matter volatile at redness in the "Albert coal" has been stated at 61.74 per cent., that of the "Breckenridge coal," of Kentucky, at 60.27 per cent., while Pittsburg coal yields but 32.95 per cent.

Chemists have obtained from coal tar by fractional distillation several substances of great utility. Of these benzene or benzol C_6H_6 , which, as before stated, is the chief constituent of the light naphtha, is also a constituent of kerosolene, as ascertained by testing it with nitric acid, which developed the odour of nitrobenzol. The chief constituents of the photogenic liquids as thrown into commerce under the name of coal oils or kerosolene, are toluol C_7H_8 , which has a sp. gr. of .870 and boiling point 237° F. (Gerhardt 230), and Cumol $C_{10}H_{12}$, which is also lighter than water, and boils at 314°. It is not

improbable that traces of these may be present in the new liquid, though they vary from it so much both in sp. gr. and boiling point. Experiments to detect aniline C_6H_5N in the specimens under examination failed to indicate it as might be anticipated, its sp. gr. being stated at 1.020, and the process of rectification with sulphuric acid being adapted to separate any alkaloids if originally present.

Compared with the commercial article of benzol, now so extensively sold, kerosolene presents several marked points of difference. Three specimens of benzol I have examined have respectively, at 60° F., the sp. gr. .7521, .7661, and .8708. These differ in sensible properties from each other almost as much as they do from kerosolene. The most agreeable in odour I imported from England at a much higher price than that paid for the American varieties, which I have generally found disagreeably "balsamic," sometimes resembling impure illuminating gas, and leading to the belief that the gas was escaping into the room. One specimen became almost intolerable by age, while the colour became gradually yellow. Kerosolene, as before stated, is almost free from odour; it is so very mobile, light, and colourless that it could not be mistaken for benzol, and still less for kerosene, its properties having relation to the latter substance not unlike those of ether to oil of turpentine.

Under the name of Eupion chemists formerly described a product of the destructive distillation of wood, which was believed to be C_8H_{10} , but it has since been shown by Voelekel that the portions rising below 212° F. consisted chiefly of acetate of methyl with acetone, a little benzol, xylite, and mesite.

The sp. gr. of eupion was stated at .655, its boiling point at 113° F., rising as high as 336° in some samples. Though stated to be the lightest of known substances, it will be seen that this product was rather more dense than our new carbohydrogen product. Accounts differed in regard to its miscibility with water and alcohol. It was, however, described as freely miscible with ether and oils, and as being of a faint and agreeable odour and without taste, which properties correspond with those of the liquor under consideration. That kerosolene contains no more than a slight trace of any oxygenated body was shown by immersing sodium in it, a fresh surface of the metal was only slightly tarnished by the contact.

The solvent properties of this new substance are of course of primary importance. They appear to me to present the true key to its usefulness. It dissolves fixed oils, apparently, in all proportions, common resin freely, though with flocculent residue, wax and cocoa-butter freely, mastic and caoutchouc slightly. It mixes freely with alcohol, oil of turpentine, ether, and chloroform, but not with water. Iodine dissolves in it sparingly, yielding a remarkably rich, though changing, purple colour.

For the common uses to which benzol is now applied, kerosolene appears to be equally adapted. By the absence of disagreeable odour it is rendered generally preferable. It is, however, unsuited to the purposes of illumination, or to generating heat, being too volatile, though its vapour does not appear to be explosive. Its extreme volatility makes it important that it should be excluded from the kerosene or other photogenic oils, and renders it a residuary product, heretofore without value, of those works in which it has been produced in a crude condition.

How far this product could be furnished by the manu-

facturers, of uniform quality, or whether it could be produced at other kerosene works, and by those numerous establishments in which the so-called rock oils now pumped in such vast quantities in the western slopes of the Alleghanies, are rectified, is a practical question yet to be tested. Its relations are evidently to this class of bitumens rather than to coal tar.

In view of its use in medicine, the fact of the great uncertainty of its composition, its being a mixture of different and undetermined proximate constituents, must interfere with its general adoption and recognition.—*American Journal of Pharmacy.*

*On the Preparation of a Natural Iodine Wine,
by Dr. BOINET.*

ORIGINALLY administered in the natural state and dissolved in water by Coindet, iodine has been exhibited much more efficaciously through the intervention of iodide of potassium, as recommended by Dr. Lugol. This latter salt, indeed, has been almost completely substituted for it, and it is nearly exclusively in this form that internal treatment with iodine is effected.

Reason, however, points out that advantage is gained in giving medicaments in a state as nearly as possible like that in which they are found in nature; and, indeed, iodide of potassium, on account of the special action which it exerts on the mucous membranes of the eye, nose, and larynx, offers real inconveniences, and is impossible to be taken by some persons.

Under these circumstances, M. Debauche has opened a way which promises to be of service in showing how to render iodine soluble in tannin without the aid of iodide of potassium. From this has arisen M. Boinet's idea of administering iodine in liquids or syrups containing tannic acid, such as the wines of horse-radish, quinine, gentian, walnut, or orange peel.

Arguing upon the good results effected by iodine contained in mineral waters, certain salts and plants, and burnt sponge, although these only contain very small quantities, remembering the results which he had already obtained by the incorporation of this metalloid into articles of food (iodised alimentation); and, as he has repeatedly said, to get farther from the laboratory and nearer to nature, M. Boinet has been induced to search for a preparation of this kind which would not have upon the tongue, the throat, nor the stomach, the ill effects of ordinary iodine wines, which are usually prepared by the addition of tincture of iodine or iodine of potassium. This is what he does:—

At the time of vintage commence by picking the grapes so as only to have them ripe and fit to make good wine. They must not be picked off the bunch, on account of the tannin and salts contained by the stalk, which serve to preserve the wine and aid fermentation. They are placed in a large wooden tub capable of holding many barrels of wine. At the bottom is placed a thick layer of grapes, then above a layer of powdered seaweed, or even a layer of ashes of these plants; then a fresh layer of grapes, then another layer of sea-weed, and so on, until the tub is full. The whole is then covered with a layer of chopped straw to keep the air away and favour fermentation. The important operation of working now commences, which lasts from fifteen to twenty days, according to the temperature. When the wine is fit to draw off—that is to say, when the fermentation is complete and the grape sugar changed into alcohol and carbonic acid, and it has dissolved and

assimilated to itself the iodine contained in the weeds—it is placed in casks, keeping it as much as possible from contact with the air. To effect this, it must be drawn from the tub by means of a tap. The concluding operations are effected as with ordinary wine. This wine is, however, nothing more than a tincture of iodine, obtained like other tinctures, as that of aloes, opium, &c., by maceration and fermentation.

This wine M. Boinet calls natural iodine wine. It keeps as well as ordinary wine, and has not, like syrups, the inconvenience of fermenting and decomposing. It contains so much iodine that it can even be mixed with other wine before being administered. The dose may be two or three spoonfuls unmixed per day for adults, and the same in coffee for children. M. Boinet usually administers it in the following manner:—In the wine usually drank at meals; or in any other beverage, a spoonful of this iodine wine is mixed at each meal. This wine has no disagreeable taste, and were invalids not told of it, they would not suspect that they were taking iodine or wine containing iodine.—*Bull. de Thérap.*

PHYSICAL SCIENCE.

On Spectrum Analysis, by M. MORREN. (Extract from a letter to the Abbé Moigno, Editor of "Cosmos.")

WHILST I fully appreciate the beautiful researches of M.M. Bunsen and Kirchhoff, I cannot but feel that their conclusions in respect to the constitution of the solar photosphere are somewhat premature and hasty. I will, in the first place, state that the yellow ray D, which is used as an argument in favour of admitting sodium as a constituent of the photosphere will prove as much for many other bodies. I might name several, but will confine myself to iron and mercury. This latter metal gives the yellow ray D, double and in a marvellous manner, together with the most beautiful violet ray that can be imagined. Another thing which ought to make us speak with extreme reserve is, that we have as yet only very incomplete knowledge of the spectra of metals. Some eminent physicists (I mention those quoted by the Heidelberg savans), M.M. Van der Willigen, Masson, and Angström, have only seen an inconsiderable number of rays belonging to many metals, especially iron; and this may be perfectly imagined at the present time, for, indeed, in the voltaic arc produced by the battery many parts must be distinguished.

1. An incandescent, very luminous portion formed of the electrodes, of dazzling lustre if they are of carbon, and fused if they are metallic. This portion only furnishes a very brilliant, continuous spectrum, free from lines.

2. The gaseous aureola, which in a powerful battery is very large, and which alone yields rays. This aureola, when the electrodes are of carbon, is formed, as is known, of two oval, concentric parts, the one violet in the centre, and the other orange throughout. It is reasonable to suppose that each of these has its distinct lines, and that the surrounding medium may also play some part in it. It is, moreover, certain that those physicists who, for want of a sufficiently powerful battery, have experimented with the electrodes too near together, have only been able to see a very limited number of brilliant rays; being overpowered by the presence of the continuous spectrum, dazzling and free from rays, formed by the incandescent liquid or solid portion of the electrodes. Hence all these differences. This week I have been

working with a battery of 65 Bunsen's elements, large size, and I have obtained with iron an admirable aureola, in each part of which, even in a very small space, the rays were in excessive number. With a powerful magnifying apparatus, I have seen skilful persons unable to count them in the field of the telescope without great difficulty. In the presence of rays so close together and so little known, it is difficult to establish without error unexceptionable coincidences.

I think, therefore, that before affirming that such or such a metal produced the negative spectrum of the sun by its presence in the photosphere, we should be perfectly acquainted with the spectra of metals, gases, and all bodies; whereas we have at present but the rough outline of such a work which is called for by the state of science. It is to be desired that these important spectra should be made with extreme care, under the control of the several savans who have studied this branch of physics, and especially under perfect and comparable conditions. Having drawn such spectra, I know how delicate and difficult is such a work; the most skilful at it are liable to error. For instance, I will mention to MM. Bunsen and Kirchhoff the extreme red ray of potassium. These gentlemen have said that it exactly corresponds to the solar line A. That is not so. The red ray of potassium is remarkable in that it is so much less refrangible than the line A of the solar spectrum that it is difficult to see, when no other precautions are taken to see it than those which suffice to illuminate and show in their beauty all the red rays of solar light. Owing to the length of this letter, I must defer entering upon details until another occasion. The red ray of potassium is at this moment the least refrangible ray of light known. I have, besides, made this experiment for the first time in the presence of M. Plucker. We first placed the micrometer wire over the solar line A; then, without touching the apparatus we observed in it the red ray of potassium. Having measured this, we returned once more to the ray A. The difference of minimum deviation between these two rays is at least 20'.

P.S.—Later researches enable me to state that the blue part of the flame of the candle, alcohol, lighting gas, paper, &c., is due to the combustion of *proto-carburetted hydrogen*, and not of *oxide of carbon*, as has been hitherto supposed, and as I myself have stated before. Spectral analysis proves this in an incontestible manner.

*On the Blue Band of the Lithium Spectrum,¹
by Professor FRANKLAND, F.R.S.*

ON throwing the spectrum of lithium upon the screen yesterday I was surprised to see a magnificent blue band. At first I thought the chloride of lithium must be adulterated with strontium, but on testing it with Steinheil's apparatus it yielded normal results without any trace of blue band. I am just now reading the report of your discourse in the CHEMICAL NEWS, and I find that you have noticed the same thing. Whence does this blue line arise? Does it really belong to the lithium, or are the coke points or ignited air guilty of its production? I find three blue bands with chloride of sodium, but they have not the definiteness and brilliancy of the lithium band. When lithium wire burns in the air it emits a splendid crimson light; plunge it into oxygen

and the light changes to bluish-white. This seems to indicate that a high temperature is necessary to bring out the blue ray.

P.S.—I have just made some further experiments on the lithium spectrum, and they conclusively prove that the appearance of the blue line entirely depends upon temperature. The spectrum of chloride of lithium ignited in a Bunsen's burner-flame does not disclose the faintest trace of the blue line. Replace the Bunsen's burner by a jet of hydrogen—the temperature of which is higher than that of the Bunsen's burner—and the blue line appears, faint, it is true, but sharp and quite unmistakable. If oxygen be now slowly turned into the jet, the brilliancy of the blue line increases until the temperature of the flame rises high enough to fuse the platinum, and thus puts an end to the experiment.

Note by Dr. Tyndall.

[On the occasion referred to by Dr. Frankland, it was a general impression among the chemists present at the lecture that I had used the word lithium for strontium throughout the evening. This induced me to ask Dr. Miller to test my chloride of lithium, which he found quite pure. I afterwards showed the blue band, the splendour of which is unrivalled, to my class at the School of Mines. The coal points without the lithium show nothing of the kind; with the lithium the band always appears. Either, therefore, the substance itself is so altered by the exalted temperature that new periods of oscillation are possible to it, or the medium in which it vibrates is so changed in elasticity as to permit of the same thing. The observation appears to be one of considerable significance. I would also draw attention to the experiment by which the absorption of the yellow band by the sodium flame was effected on the same occasion as one of the most striking class experiments in the whole range of optics. It is very easily performed, a band 18 inches long and three-quarters of an inch wide being quite attainable within ordinary lecture-room limits. A salt flame 10 feet thick produced no such effect. Dr. Miller, I am informed, repeated this experiment with success before an evening meeting of the British Association at Manchester.]

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY OF DUBLIN.

Monday, November 25, 1861.

The Honourable J. P. VEEKER, F.R.S., President,
in the Chair.

The minutes of the former meeting having been read and confirmed, the PRESIDENT called on Professor C. A. Cameron, M.D., M.R.I.A., to deliver a discourse "On Bunsen and Kirchhoff's Method of Spectrum Analysis."

Dr. CAMERON commenced by observing that he was induced to bring the subject before the Society by the consideration that it has not, up to the present time, been brought publicly under the notice of any of the learned Societies in Ireland, although this has frequently been done on the other side of the Channel. He then made some observations on the nature of light, and the mode of forming the solar prismatic spectrum. He pointed out the dark spaces termed "Fraunhofer's lines" in the spectrum, and demonstrated the coincidence of these with the luminous bands observable in the spectra of certain of the metals, and, likewise, how they were caused by what might be termed a negative spectrum of the luminous envelope of the sun. He then exhibited the spectra

¹ In a letter from Professor Frankland to Dr. Tyndall, published in the last number of the *Philosophical Magazine*.

produced by burning salts of sodium, potassium, calcium, &c., and explained the method of using the spectroscope, which, in this instance, was the form of apparatus described by Mr. Crookes, and supplied by Spencer Browning and Co. Dr. Cameron showed that, by means of this instrument, the presence of inconceivably small quantities could be easily and expeditiously detected. Thus it was found that soda was almost always present in the atmosphere, and that the metal lithium, hitherto supposed to be found only in a few rare minerals, such as Killinite (found at Killiney Hill), was universally diffused; and also, by its means, three new elementary bodies have been discovered, two by MM. Bünsen and Kirchhoff, and one by Mr. Crookes. The discourse was illustrated by diagrams and numerous experiments, and at its close there was warm applause.

A paper was then read by Mr. EMERSON REYNOLDS, Assistant-Chemist to the Society, "*On a New Process for Photographic Printing.*" The author commenced by apologising for bringing a photographic subject before the Society at all. He said that he would not have done so, only that his process was intimately connected with a subject which he would have the honour of communicating at the next meeting of the Society. Mr. Reynolds then went on to say that in the early part of last year accident had led him to the study of some of the compounds of oxalic acid with the oxides of iron, and, while pursuing his investigations, he had observed a peculiar decomposition to take place under the influence of the solar rays,—namely, the reduction of the peroxalate of iron to the state of protoxalate. At first he was under the impression that no one had noticed this fact before, but subsequently he ascertained that Dobereiner had pointed it out in the earlier part of the present century, however, no one thought of applying the reaction to photographic use. This he has done, and has been very successful in working out the subject. His process is substantially as follows:—Paper is saturated with a solution of ammonio ferric oxalate in the usual way, when dried in the dark, it is exposed under a negative, and after being sufficiently printed, is taken out of the pressure frame and thoroughly washed with water. The picture, which is now of a light yellow colour, is then immersed in a very dilute solution of ammonio-nitrate of silver, containing as little ammonia as possible, when it is immediately developed, and appears of a fine dark colour, all that now remains to be done, is to wash, dry, and mount, when the picture is finished. The author states that the whole process from the commencement of the printing till the picture is mounted, on a fine day, does not occupy more than two hours, in this time he has obtained pictures, which up to the present, have not faded in the least. Several specimens of the process were shown and were much admired by those present.

Mr. CHARLES R. C. TICHBORNE then read a paper "*On the Naphthalene Colours.*" This paper is supplementary to one, by the same author, which appeared in a late number of the CHEMICAL NEWS. Mr. Tichborne did not confine himself to his own experiments, but gave a general and very lucid description of the various products of the destructive distillation of coal. He explained the means which are adopted for purifying the crude naphthalene, and exhibited some beautifully crystallized specimens. He then proceeded to notice M. Roussin's paper on artificial alizarine, and produced some of that colouring matter by the action of a reducing agent on the binitro-naphthalene, and directed particular attention to the precautions to be observed so as to obtain the largest yield of the dye. The author then went on to say that it is from madder we get the colours known as Adrianople or Turkey red, which is the most durable vegetable tint known. The discovery is traceable to India. The process was afterwards practised in Asia and Greece, and, about the middle of the last century, works were established

near Rouen. In 1765 the French Government caused an account of it to be published. Its introduction into England, at the end of the eighteenth century, was by a Manchester house, but the proper method of treating it was made known by a Frenchman (Papillon), who brought it into Glasgow. This latter town is still celebrated for this class of dyeing. Mr. Tichborne then went on to show the very close connexion which exists as regards physical and chemical properties between alizarine—the most important tinctorial principle of madder—and the colouring matter produced by the action of reducing agents on binitro-naphthalene, although their identity in chemical constitution was not proved; for, as yet, no analyses have been made of the dye. Attention was then directed to the formation of a blue colour by means of cyanide of potassium, which the author has found may be converted into a beautiful green tint by the addition of some di-nitraniline. Dinitraphenic acid and the other yellow tar-products are not applicable. He concluded by exhibiting a number of specimens of ribbon and woollen fabrics which he had dyed with these colours, and are likely to prove most formidable rivals to the aniline compounds.

ROYAL INSTITUTION OF GREAT BRITAIN.

General Monthly Meeting, Monday, December 2, 1861.

WILLIAM POLE, Esq., M.A., F.R.S., Treasurer and Vice-President, in the Chair.

James Bass, Esq., Rev. George Bowes Macilwain, B.A., and David Ricardo, Esq., were elected Members of the Royal Institution.

NOTICES OF BOOKS.

A Manual of Chemistry, Descriptive and Theoretical. By WILLIAM ODLING, M.B., F.R.S. Part I. London: Longman and Co. 1861.

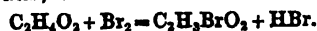
THIRD NOTICE.

AMONG carbon compounds we may cite the following as examples of heterologous series, taking the homologous methylene and ethylene as starting-points:—

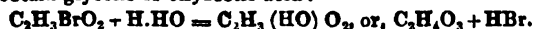
Methylene . .	CH ₂ .	Ethylene . .	C ₂ H ₄ .
Formic aldehyde	CH ₂ O.	Aldehyde . .	C ₂ H ₃ O.
Formic acid . .	CH ₂ O ₂ .	Acetic acid . .	C ₂ H ₃ O ₂ .
Carbonic acid .	CH ₂ O ₃ .	Glycolic acid :	C ₂ H ₃ O ₃ .
		Glyoxylic acid :	C ₂ H ₃ O ₄ .

Professor Odling makes frequent use throughout the volume of tabular statements, which present at once, to any one capable of reading chemical formulæ, the clearest view of the relations between the bodies cited, and express in a few lines what would require a page or more to explain in words. Not only do these tables include numerous examples of homologous, isologous, and heterologous series, but our author arranges in like manner the various combinations of the same radicle. The following illustration refers to peroxide of hydrogen, the eurythane of Laurent:—

"The compound radicle, peroxide of hydrogen, H₂O₂, is equivalent to the simple radicle, chlorine, Cl, and in a great number of reactions is exchangeable for chlorine and its congeners. One of the most generally useful modes of oxygenating different compounds consists in first substituting a halogen in exchange for hydrogen, and then, by means of water, substituting peroxide of hydrogen in exchange for the halogen. Thus, by the action of bromine upon acetic acid, we obtain bromoacetic acid :



Then by the action of water upon bromoacetic acid we obtain glycolic or oxyacetic acid :



It is observable that water may be regarded as a derivative of chlorhydric acid, in which the compound radicle HO replaces the simple radicle Cl, and so in other instances, thus :

Chlorhydric acid	H.Cl	H.HO	Water
Chlorine	Cl.Cl	HO.HO	Perox. of hyd.
Chloride of ethyle	C ₂ H ₅ .Cl	C ₂ H ₅ .HO	Alcohol
Chld. of benzoyle	C ₆ H ₅ O.Cl	C ₆ H ₅ O.HO	Benzoic acid
Chld. of sulphuryl	SO ₂ .Cl ₂	SO ₂ .(HO) ₂	Sulphuric acid
Chld. of phosphoryl	PO ₂ .Cl ₃	PO ₂ .(HO) ₃	Phos. acid."

An excellent table of the sulphates, showing the anhydrous sulphates and those containing various amounts of water of crystallization, will be found on page 176.

In the accounts of the oxides and hydrates, which is altogether thoroughly well-written and full of interesting matter, the reader will find the classification of the peroxides, and the relations of the oxides to the hydrates and the acids described with greater completeness than elsewhere.

The author has avoided a fault into which some advocates of the Unitary Notation repeatedly fall. It is very well to insist on the fact that formulæ do not necessarily indicate the constitution or building up of the substances to which they have been assigned, but that they chiefly serve to show the atomic relations of the constituent elements; yet the connection of one compound with another is lost sight of by the student whose teacher ignores compound radicles, and jots down before his class the formula of sulphuric acid sometimes as H₂SO₄, sometimes as 8H₂O₄, and occasionally goes so far as to break it up, in a playful but not instructive manner, into as many pieces as possible. Those who have just liberated themselves from the traditions of the older chemical theories, should take care that their very liberality does not degenerate into dogmatism, for it must not be affirmed that the Unitary System is itself already perfect, and cannot admit of further development.

Dr. Odling discourses with ease and confidence in these pages, and not infrequently with a clearness and force that are truly admirable. He has shown, in many directions, the advantages accruing from the new system; and it is with pleasure rather than disappointment, that we see how, here and there, he expresses a doubt about the correctness of some of the views which he holds. We cannot help feeling sorry that Professor Odling has banished so completely from the volume before us, the great chemists, the results of whose labours are there collected and arranged. The student should occasionally be reminded of the careful and exact Berzelius, the brilliant Davy, the sagacious Dalton, and a few other illustrious names. Even Laurent and Gerhardt, the author's immediate teachers, scarcely get due credit in the body of the work itself, and a young reader might come to the conclusion—a conclusion which Professor Odling would be the last to encourage—that the author himself was the chemist of chemists. We also miss a clear, compact account of the grounds on which the change of systems is based. But the author continually shows a peculiar power of appreciating the bearings and relations of chemical facts discovered by others, and the task which he has undertaken is one for which he is well fitted. We have derived so much pleasure from examining Part I. of this work, that we wait anxiously for Part II.

NOTICES OF PATENTS.

Grants of Provisional Protection for Six Months.

2407. Joseph Tessier, Rue de St. Nicolas d'Antin, Paris, "Improved means of treating barley, corn, and

other cereals, for brewing and other purposes."—Petition recorded September 26, 1861.

2574. Thomas Forster, Sparrow Hall, Streatham, Surrey, "Improvements in reworking waste vulcanised india-rubber."—Petition recorded October 16, 1861.

2692. Charles Stevens, Charing Cross, London, "Improved apparatuses for indicating escapes of lighting gas from the conveying pipes, and determining the precise leaking places of the said pipes."—A communication from Charles Antoine Fournier, Paris.—Petitions recorded October 26, 1861.

2700. George Mowbray Gilbert, Albany Terrace, Britannia Square, Worcestershire, "Improvements in preparing blue colour and in apparatus for applying such colour to water."—Petitions recorded October 28, 1861.

2706. John Bibbington, Rochdale, Lancashire, "Improvements in machinery for breaking and crushing limestone and other hard substances."

2716. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in the preparation or treatment of skins and hides."—A communication from Dr. Friederich Knapp, Munich, Bavaria.—Petitions recorded October 29, 1861.

2718. Marc Antoine Francois Mennons, Rue de l'Ecliquier, Paris, "An improved composition for igniting lucifer matches."—A communication from Messrs. Auguste Halinbourg and Léopold Ber, Faubourg Poissonnière, Paris.

2739. William Clark, Chancery Lane London, "Improvements in photographic albums."—A communication from Claude Mamés Augustin Marion, Boulevard St. Martin, Paris.

2744. Robert Mushet, Coleford, Gloucestershire, "An improvement or improvements in the manufacture of cast steel or of homogeneous iron."

2754. John Charles Wilson, Bucklersbury, London, "Improvements in machinery or apparatus for the manufacture of sugar."

2761. George Evans, Gloucester Terrace, Portman Square, London, "Improvements in treating peat to render it useful as fuel and for illuminating and metallurgical purposes."

2764. Joseph Bowden, Hulme Street Works, Salford, Lancashire, "Certain improvements in dyeing and in apparatus employed in dyeing."

Notices to Proceed.

1706. Bevan George Sloper, Hackney, London, "An improved method of, and machinery for, amalgamating and for effecting the separation of gold from earthy and other matters containing the same."—Petition recorded July 4, 1861.

1732. Thomas Cogley, Meerholz, Hesse, Germany, "An improved process for the manufacture of fluo-silicates and silicates of lead and baryta, and for the application of the same to various purposes in the arts and manufactures."—Petitions recorded July 8, 1861.

1733. Thomas Telford Macneill, Cockspur Street, London, "Improvements in the construction of barometers, and in apparatus for registering the indications of the same."

1734. Thomas Cogley, Meerholz, Hesse, Germany, "An improved process for the treatment of silicates of metallic and non-metallic bases and other siliceous compounds of slags, scorix, and such like, in order to recover the said bases, and also in the production of fluo-silicic and silicic acids."

1756. Thomas John Smith, Queen Street, Cheapside, London, "Improvements in photographic albums."—Petition recorded July 12, 1861.

1772. Thomas Cogley, Meerholz, Hesse, Germany, "Improvements in the manufacture of metallic or earthy silicates or siliceous compounds of the same from the metallic and earthy bases or their salts and soluble alkaline

silicates, the formation of alkaline acetates or caustic alkalies, and application of the same."

1773. Thomas Cobley, Meerholz, Hesse, Germany, "An improved process for preserving indurated timber, wood, and other vegetable matters, and for rendering the same unflammable."

1776: Thomas Cobley, Meerholz, Hesse, Germany, "An improved process for the production or manufacture of fluo-silicates of tin, zinc, and baryta, and their application as pigments for glazing, enamelling, and in the manufacture of glass."—Petitions recorded July 15, 1861.

2110. Richard Archibald Brooman, Fleet Street, London, "An improved method of treating the hop plant to obtain a material resembling wool."—A communication from Barthélemy Picard, Nancy, France.—Petition recorded August 23, 1861.

CORRESPONDENCE.

Fixing Colouring Matters.

To the Editor of the CHEMICAL NEWS.

SIR,—I observe, in No. 103 of your useful Journal, Notices of Patents taken for Fixing the Colouring Derivatives of Tar on Vegetable Fabrics, by Messrs. Miller, of Springfield, Glasgow, and by several others, all of which embody a common principle of fixing those colours by the formation of a tannate of tin from an alkaline stannate, in the fabrics—if we except that of Lloyd and Dale's, and of Church, who substitute antimony for tin—still retain the same principle.

I feel called, in duty to myself, and it may be the case with other practical men, to say, that ever since the introduction of mauve by Perkins, and of roseine by Simpson, I have followed the above plan, with considerable success.

I am, &c.

JOHN HAMILTON.

Armitage Dye House, near Huddersfield.

Sparteine.

To the Editor of the CHEMICAL NEWS.

SIR,—Would you do me the favour of correcting an error in your last Number, contained in a report of the Proceedings of the Chemical Society? In a paper on Sparteine I am made to state that this base appears to be "homologous with nicotine." This was merely a suspicion, entertained prior to investigation,—at the end of which sparteine was found to be $C_{12}H_{26}N_2$. As nicotine is $C_{10}H_{14}N_2$, the bodies have, clearly, no relation of homology. The mistake is trifling enough in itself; but some of your readers might further imagine that it had been asserted in the original paper.—I am, &c.

EDMUND J. MILLS.

December 2.

On the Adulteration of Butter.

To the Editor of the CHEMICAL NEWS.

SIR,—It is not with any feelings of rivalry that I refer to the paper of Dr. Ballard in the last Number of the CHEMICAL NEWS, alluding to that of mine on the Adulteration of Butter, but, on a comparison of the two papers, it must be evident that the simplicity and practical utility of mine are not in the slightest degree diminished by the publication of the Doctor's.

Dr. Ballard takes objection to the ether process on the score of expense. This is a matter of assumption merely, a teaspoonful or so of ether being ample for an experiment. Surely that quantity is not so expensive as to condemn its utility. More than that, I had a particular object in view in writing my paper—to show, in answer to a Correspondent, how easily lard—for that was the question—may be detected; and then I thought I might as well

experiment on, and possibly include, other kinds of fat, which, I found, could all be readily identified thereby; but not so by the plan of Dr. Ballard, who considers "Beef Dripping" to be the general fatty adulterant used, which does not accord with my experience.—I am, &c.,

JOHN HORSLEY, F.C.S.

The Laboratory, Police Station, Cheltenham.

MISCELLANEOUS.

Gas from Water.—Arrangements are being made, it is understood, at the Leeds Gasworks for the manufacture of gas from water, much on the principle carried out, some years ago, by Mr. White, in Manchester. Steam is decomposed in passing over incandescent coke or charcoal, and the liberated hydrogen is afterwards carburated by being passed through resinous or other carbonising matter.—*Engineer.*

The Warmest and Best Clothing.—Most persons suppose that fabrics made of coarse wool are the warmest and most durable. This is a mistaken idea. Owing to the lower prices of coarse wool, fabrics of this material are usually made heavier than those of fine wool, hence their greater thickness deceive persons respecting their qualities for warmth and wear. There is no heat in the wool itself; its property of what is called "warmth" is due to its non-conducting qualities. If we grasp a bar of iron on a frosty morning it produces a disagreeable cold sensation because it is a good conductor of heat, and the warmth of the hand is rapidly carried off by the metal. On the other hand, a piece of woollen cloth, especially if it has a long nap upon it, does not feel cold because it is a good non-conductor, and prevents the heat passing rapidly from the hand. Now the warmest fabric for clothing is that which is the best non-conductor; and Count Rumford made a great number of experiments with different materials in order to find out the best. According to his observations, the down of the eider duck, which the Esquimaux use in their clothing, is unrivalled in this respect; and the finer the fabric of woollen cloth used, the more imperfectly did it conduct the heat from the human body. As fine woollen cloth is superior to that of coarse wool as a non-conductor, it is therefore the best for clothing in keeping the body warm during cold weather. We are also positive that cloth made of fine wool equal in thickness to that manufactured from the coarser material will wear much longer. The finest woollen cloth, although dearer at first, is cheapest in the end, because it is more durable and warmer; and according to Liebig, so much heat saved is so much meat gained. It must not be overlooked, however, that there may be a very great difference between what is called "firm cloth" and cloth made of fine wool. Fine wool is our theme; it feels pleasant and soft to the touch, and it has a rich velvety appearance. There has been a great demand recently for coarse wool to be used in the manufacture of common army and other cloth, but every effort should rather be made to obtain plenty of cheap fine wool, because it is the warmest and best for clothing.—*Scientific American.*

ANSWERS TO CORRESPONDENTS.

* * All Editorial Communications are to be addressed to the EDITOR; and Advertisements and Business Communications to the PUBLISHER at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

J. Horsley.—We shall be pleased to receive the paper you speak of, at your convenience.

F. Harrison.—You will obtain better results by having forty Groves' cells of the ordinary size, than by using the same surface of platinum in four large cells. This would be an admirable "quantity" arrangement, but would be very deficient in "intensity," and for brilliant lecture experiments the latter is as necessary as the former. No addressed envelope arrived.

THE CHEMICAL NEWS.

VOL. IV. No. 106.—December 14, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 107.)

Synthesis of Nitrate and Sulphate of Lead.—Before explaining how I proceeded with the synthesis of nitrate and sulphate of lead, I ought to point out the means which I made use of to procure pure lead.

Preparation of Pure Lead.—The preparation of pure lead offers perhaps even more difficulty than that of pure silver. After a considerable number of trials, I had recourse to these methods to obtain the metal employed in my experiments. Commercial acetate of lead was dissolved in warm water, contained in a large leaden digester, and kept at a temperature of 40° to 50° in contact with very thin sheets of lead until all the copper and silver were precipitated. The solution was then filtered and poured into nearly boiling water, strongly acidulated with sulphuric acid. The sulphate of lead was washed by decantation until the washing waters contained no trace of sulphuric acid. The salt was then transformed into carbonate by means of a mixture of sesquicarbonate of ammonia and solution of ammonia; for this it is only needful to diffuse it in the water containing the alkaline salt, an effervescence takes place, which lasts as long as there remains sulphate of lead undecomposed. At this point I decant the solution of sulphate of ammonia and the excess of carbonate of ammonia, and wash it with pure water as long as the washing waters contain sulphate in solution.

The carbonate of lead thus formed dissolves entirely in dilute nitric acid. It is perfectly free from foreign metals, except traces of iron, which adhere to the sulphate of lead in spite of the excess of sulphuric acid employed. To separate the iron, I transformed a portion of this carbonate of lead into oxide, by heating it carefully in a platinum vessel. Another portion was dissolved in dilute nitric acid, taking the precaution to leave some of the carbonate undissolved. The solution of nitrate was heated to ebullition, and the oxide of lead was then gradually added to it. The oxide in dissolving precipitated traces of iron. The liquid was filtered and an excess of sesquicarbonate of ammonia poured into it. In the carbonate of lead thus obtained it has been impossible for me to discover the slightest trace of foreign metal. It is this carbonate which I have converted into metallic lead. To effect this, after having dried it, I project it, by small quantities at a time, into pure fused cyanide of potassium. As this reduction ought to be effected in a white, unglazed, porcelain crucible, which is very liable to break, I have had recourse to the same plan which I used in the reduction of silver from its chloride, and in the fusion of the pure metal,—that is to

say, I fixed the porcelain crucible in the centre of another larger one, interposing between them calcined pipe-clay, which had been reduced to powder, cementing the whole together by the addition of five per cent. of fused and powdered borax.

The lead obtained by the first operation is heated a second time with pure cyanide of potassium until it presents at the bottom of the cyanide a surface as convex and brilliant as pure mercury. When it is somewhat cool I cast it in a polished ingot-mould of cast steel.

When the lead contains the slightest trace of oxide or sulphide of this metal it does not present a convex surface when it is fused.

Pure lead is much whiter and more soft than the ordinary metal. It appears to tarnish very rapidly when exposed to the air.

Instead of reducing the carbonate of lead by cyanide of potassium, I have employed for this purpose the black flux obtained by carbonising purified Seignette salt. To entirely deprive this salt of foreign metals, I first pass a current of hydrosulphuric acid through its boiling solution; I then pour in a few drops of sulphide of sodium solution. The coloured solution, left to itself for fifteen days in a well-stoppered bottle, becomes completely decolorised by depositing the metallic sulphides. The decanted solution is agitated with tartrate of lead as long as it contains the slightest trace of sulphide of sodium. Before calcining the Seignette salt, I always test it to see that it contains neither sulphide, sulphate, nor foreign metals.

The carbonate of lead mixed with the pulverised black flux is reduced by the action of heat. The temperature necessary for the fusion of the alkali being tolerably high, there are almost always a certain quantity of alkaline metals reduced, which alloy with the lead. To remove these metals, I heat the lead for some time in contact with the air, continually stirring the metallic bath with a rod of pipe-clay. When I have thus oxidised a certain quantity of the lead, I pour on the mass fused cyanide of potassium, and heat the whole until a great portion of the cyanide is volatilised. The lead is then allowed to cool to near its point of solidification, and then run into an ingot-mould of polished steel.

I have also obtained lead by the reduction of the chloride. For this purpose I treated with an excess of dilute hydrochloric acid the carbonate of lead prepared by the action of sesquicarbonate of ammonia upon the sulphate of lead. The minute traces of iron contained in the carbonate remain dissolved in the excess of hydrochloric acid. I made use of two different plans to reduce the chloride. One consisted in mixing it with two-thirds of its weight of pure carbonate of soda and projecting the intimate mixture into fused cyanide of potassium. The metallic lead produced was poured into another crucible with a fresh quantity of pure fused cyanide of potassium. It is there kept at a high temperature and continually agitated until the surface, flat and dull as it is at first, becomes strongly convex and

brilliant. When sufficiently cool it is poured into an ingot-mould and kept away from moist air.

The other plan of reducing the chloride consists in heating an intimate mixture of it with carbonate of soda and black flux. The resulting lead was fused and agitated for some time in contact with air, and then heated with cyanide to remove the last traces of oxide.

Synthesis of Nitrate of Lead.—I effected the synthesis of nitrate of lead in absolutely the same manner as indicated in the case of nitrate of silver, taking all the precautions necessary to arrest the last traces of lead carried off by the gas during the solution of the metal, and sometimes during the evaporation of the solution of lead in nitric acid.

In general, to effect the solution of the metal, I employed the most concentrated acid which was capable of attacking it. Lead decomposes concentrated nitric acid, but very slowly, but, on the other hand, this only dissolves traces of the nitrate. This salt as it is formed precipitates in the form of a crystalline powder. The precipitation of the nitrate of lead during its formation is necessary for the success of the synthesis. This salt, in fact, not supporting an elevated temperature without decomposition, ought to be dried at a comparatively low temperature. The probability of a complete desiccation of this salt is only acquired with difficulty if it is deposited in even not very voluminous crystals, as is the case during the evaporation of a solution of the nitrate. The attack of 150 or 200 grammes of lead by a proper quantity of acid more or less concentrated, and under the influence of a continuous heat of 70° to 80°, requires from thirty-six to forty-eight hours. I have, moreover, found out that when once the action has commenced the heat must be kept up night and day, until the complete desiccation of the resulting salt. If this precaution is neglected the operation may be considered lost; for in this case when the saline mass is dried it slowly and continuously loses weight for six weeks, as I have found. In all the syntheses which are detailed in the Table I have taken the precaution to keep up the heat night and day after the action has once commenced until it no longer disengages a trace of acid under the influence of a temperature of 125° to 130°. By keeping the globe continually at a temperature notably below the boiling-point of the liquid which it contains, it requires from four to six days to bring to dryness the product of the attack of 150 to 250 grammes of lead. When brought to apparent dryness, 160 to 200 grammes require a continuous temperature of 140° to 155°, to be kept up during eight to twelve days in a current of dry and pure air to be brought to a constant weight. In this state of desiccation the temperature can be raised to nearly 200° in a current of air without experiencing further loss; but a little above that temperature acid vapours are evolved in a continuous manner.

I wished to know to what degree I could rely upon the desiccation of nitrate of lead in the state of a crystalline powder, and I found that 180.557 grammes of this salt precipitated by nitric acid from an aqueous solution, and consequently in the form of a crystalline powder, dried first at 125°, and then heated for three days at a temperature comprised between 125° and 140° lost 33.3 in weight. The same salt raised for eight days to 155° experienced a farther loss of 100.000ths of its weight. I kept it during thirty-six hours at 155° without causing it to undergo the slightest alteration in weight. From 195° to 200° it again commenced to disengage acid vapours, and did not cease until it was in part reduced to the state of basic nitrate. The limits

between the point at which nitrate of lead loses its last trace of acid and water and that at which it decomposes are therefore very narrow. From what I have above stated, this limit does not exceed 55°. The difficulty which is found in drying the nitrate without decomposing it explains why the results of the analyses of this salt by Berzelius, Anderson, and L. Svanberg agree so imperfectly. I have myself performed a great number which have equally given me the most contradictory figures; but this is of no present importance.

I have made two series of syntheses. In the first series all the nitrate was dried in a current of air. Having conceived some doubts of the possibility of thoroughly drying this salt without decomposing it, I undertook, a year and a-half later, a second series of experiments, but effecting this time the desiccation *in vacuo*. With this object, after having first heated to 80° for eleven days and nights the product of the solution of lead in nitric acid, to bring it to the state of dryness, and after having kept the salt at from 140° to 155° for a hundred and twenty hours, I applied a caoutchouc connector to the flask containing the salt and exhausted the air from it. The salt remained *in vacuo* from eight o'clock in the morning till eleven o'clock in the evening, the temperature being kept all the time at about 155°. During this time I allowed air to enter, and pumped it out again five times. I remarked that the air pumped out of the flask reddened litmus paper contained in a tube which the air had to pass through before arriving at the cylinders of the pump.

These are the details of one experiment. They show what confidence may be placed in the syntheses of nitrate of lead dried in the air:—

	Grammes.
Weight of the flask	493.862
Weight of the lead in air	100.007
Weight of the globe with the nitrate dried at 155° until the weight of the salt was constant	653.8215
Weight of the flask after the salt had been heated to 155° <i>in vacuo</i> for three hours	653.815
Weight of the flask the next day	653.616 ¹
Weight of the flask, the salt having been again heated to 155° <i>in vacuo</i> for three hours	653.812
Weight of the flask, the salt having been heated a third time for two hours	653.810
Weight of the flask the next day	653.8105

Thus, the greatest difference between the weight of the salt dried in air and *in vacuo* is 0.0115 grammes for 159.970 grammes of nitrate.

I repeat, that whatever may be the care which I have taken to obtain the complete desiccation of nitrate of lead, however remarkable is the concordance between the different syntheses, I conceive, however, that there may remain a legitimate doubt as to the possibility of absolutely drying such large quantities of salts when they cannot be raised to a high temperature without decomposition. Further experiment will decide this. All that I dare predict is, that chemists who place themselves under the same conditions as myself will find absolutely the same results.

It remains to be seen whether these conditions are favourable to a research of this kind, and even if the nitrate of lead is not too unstable to be used for a rigorous determination. The lead employed in the syntheses was prepared—

¹ Thus in the original, probably a misprint for 653.816

For Nos. I. and II. with the pure carbonate and cyanide of potassium.

For Nos. III. and IV. with black flux and carbonate of lead.

For Nos. V. and VI. with chloride of lead, carbonate of soda, and cyanide of potassium.

For Nos. VII. and VIII. with black flux and carbonate of lead.

For Nos. IX. and X. with chloride of lead, carbonate of soda, and black flux.

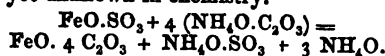
Synthesis of Nitrate of Lead.

Numerical order.	Weight of the Lead in air.	Weight of the Lead reduced to a vacuum.	Weight of the Nitrate of Lead in air.	Weight of the Nitrate of Lead reduced to a vacuum.	100,000 parts of Lead yield Nitrate of Lead.
First Series.					
I.	103'0075	103'000	164'748	164'773	159'973
II.	140'695	140'6887	225'033	225'0674	159'975
III.	110'2722	110'2672	176'381	176'408	159'982
IV.	141'999	141'9927	227'118	227'1527	159'975
V.	148'623	148'616	237'6695	237'702	159'968
VI.	124'3535	124'348	198'894	198'924	159'973
			Mean ..		159'974
Second Series.					
VII.	100'007	100'000	159'948	159'970	159'970
VIII.	200'014	200'000	319'885	319'928	159'964
IX.	250'0175	250'000	399'843	399'8975	159'959
X.	250'0175	250'000	399'860	399'914	159'965
			Mean ..		159'9645

(To be continued.)

On the Oxalate of Iron,¹ by H. CROFT, D.C.L., Professor of Chemistry in University College, Toronto.

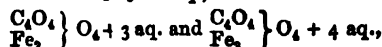
In No. 43 of the CHEMICAL NEWS, amongst extracts from foreign journals (*Comptes-Rendus*, 51—17), there appeared a short note on the so-called Quadroxalate of Iron, by Dr. Phipson, the formula being given as $\text{FeO} \cdot 4 \text{C}_2\text{O}_3$. The correct name representing its composition being given, it does not appear that there can be any typographical error in the formula, which would otherwise seem likely from the excessive improbability of the existence of an anhydrous quadroxalate of a heavy metallic oxide. The absurdity of the formula becomes still more apparent if the method of preparation be considered, viz. by precipitating ferrous sulphate by oxalic acid, or better by oxalate of ammonia. In this latter case, these two neutral salts must so decompose each other as to produce a highly acid insoluble compound and set free three equivalents of ammonia, by which the supernatant liquid must become strongly alkaline—a species of decomposition the writer believes to be as yet unknown in chemistry.



It is scarcely necessary to say that nothing of the kind takes place. The salt is described as being yellow, and giving with ferricyanide of potassium a green substance, owing to partial decomposition—a fact which is not altogether incomprehensible when it is remembered

that CfdyK_3 gives a blue colour with salts of FeO , and that blue and yellow produce green.

It is a pity that Dr. Phipson, when writing on the subject, should not have previously made himself acquainted with facts which have been known for nearly five-and-twenty years, the salt being nothing more than the ordinary oxalate of iron, containing two equivalents of water $\text{FeO} \cdot \text{C}_2\text{O}_3 + 2 \text{aq}$. This compound has been described by Vogel and Berzelius, its composition shown by Döbereiner and Rammelsberg, who appear to have proved that it differs from the rare mineral Humboldtite in containing half an equivalent more water, $\text{FeO} \cdot \text{C}_2\text{O}_3 + 1\frac{1}{2} \text{aq}$, and $\text{FeO} \cdot \text{C}_2\text{O}_3 + 2 \text{aq}$, or



corresponding to the manganous oxalate. More recently it has been examined by Souchay and Lensen in their extended investigation of the oxalates.

In order to test the matter still further, the salt was prepared by my pupils, Mr. Ramsay and Mr. Thompson, in both ways indicated by Dr. Phipson, using a large excess of oxalic acid in the one case and precipitating both from concentrated and from very dilute solutions. In both cases the precipitation takes place almost immediately.

The subjoined quantities of FeO were obtained. It will be observed that the amount of FeO is a little below the requisite quantity both in these analyses and in those of Rammelsberg which are appended, owing to the salt retaining a certain amount of hygroscopic water, unless dried for a long time at 100° ; and also to the fact, that when heated it is not perfectly oxidised unless the heat be continued for a long time or the residue be moistened with nitric acid and again heated, as was done in No. I. No. IV. was only air dried.

	Calculated.	I.	II.	III.	IV.
FeO	36	40	39'70	38'91	38'68
C_2O_3	36	40	—	—	—
2HO	18	20	—	—	—
	90	100			
RAMMELSBURG.					
FeO	38'78	39'10	38'92	38'84	39'48

Additional Note by the Author.

The 17th number of the *Comptes-Rendus* having come to hand, the writer is enabled to give the analyses on which the above formula is founded, viz:—

FeO	19'35	19'44
C_2O_3	80'65	80'56
	100'00	100'00

From the exact accordance of the analyses, it is evident that the oxalic acid was not determined, but only calculated from the loss, and as the quantity of FeO is exactly one half of that obtained by other chemists, it seems probable that Dr. Phipson has made the ludicrous mistake of calculating the protoxide corresponding to the obtained peroxide as FeO , and not as Fe_2O_3 , and considered all the remainder as oxalic acid.

On the Chemico-Microscopical Characteristics of Butter and Lard, by JOHN HORSLEY, F.C.S.

HITHERTO I have had considerable difficulty in applying the microscope to the distinction of these substances, but according to the following method the thing is easy

¹ Canadian Journal of Science, XXXI, 18.

enough:—Take a piece of each pure butter and lard and dissolve them in a little benzole contained in separate vessels, and allow the benzole two or three days to evaporate as much as possible to near dryness. Then take a small shallow glass dish, place in the centre of it a little of the butter, cover this with a drop or two of strong ammonia, shake the dish for a minute or two to distribute the contents equally, and place it under the microscope. By using a half-inch power, the object glass will be seen to be studded with long black tufts of acicular crystals, such as I alluded to in my former paper, the ammonia appearing to have little or no action on them. The other portion of the butter will be in large transparent masses, which by reflected light are opaque.

Lard, on the other, presents no such appearance, there being only a few small crystalline tufts observable, which disappear after a time, and the whole becomes more or less transparent flakes of an irregular and indented character.

Being called away to the Gloucester Assizes on a professional case, I have not had time to examine other fats; but no doubt, by a similar proceeding, I shall get at their characteristics under the microscope, which I shall have pleasure in communicating if worthy of notice.

The Laboratory, Police Station, Cheltenham.

On Formic Acid and the Hemihedralism of Formiate of Strontia, by M. JACOBSEN.

FORMIC ACID, prepared from starch and neutralised with carbonate of strontia, produces, as is known, fine crystals of formiate of strontia with two equivalents of water, which are hemihedral, some being right-handed and others left-handed. M. Pasteur, who noticed this fact, equally remarked that the aqueous solution of this formiate has no action upon polarised light, which he explains by the presence of the two hemihedral forms.

When prepared from oxalic acid and from glycerin, formic acid gives rise to a formiate of strontia similar to the preceding, at least in appearance, but which nevertheless differs sufficiently from it to prevent the admission of the absolute identity of the two acids, and consequently the two salts. The former formiate gives in preference left-handed hemihedral crystals, while the crystals produced by the formiate from glycerin is especially distinguished by right-handed hemihedral facets, the left-handed facets being in this instance in quite a subordinate proportion, while in the formiate from starch the left-handed facets preponderate.—*Annal. de Physik und Chem.*, vol. cxiii., p. 493.

On Certain Properties of the Platinum Metals, particularly Ruthenium, by Dr. CLAUS.

(Continued from page 258, vol. III.)

On the other hand, is not rutheniate of potash still more closely connected with ferric salts by its decomposing so readily, and by its similar decomposition also into O_3 and Ru_2O_3 ? Iron and manganese have also like degrees of oxidation, MO and M_2O_3 , similar to the platinum metals, and the combination MO_2 , which is wanting in iron, is met with again in manganese. The green solution of manganate of potash has also certain analogies with the blue solutions of a less positive pair,—namely, that of rhodic and iridic acids. But the most striking analogy is that existing between ferrocyanide of potassium and

double cyanides with a composition similar to the osmic pair. If the ferrocyanide of potassium, instead of being yellow, were colourless, like the cyanides of these other metals, they might be considered identical, so similar are their reactions. Between the two last terms the iridium and rhodium pair occupy the middle, being connected on one hand with platinum pair and on the other with the osmium pair. I have not been able to discover the relations which may exist between these metals and copper, which is always found in platinum mines, in spite of the isomorphism of the copper with the ferrous and manganous oxides. It is possible that copper ought to be placed between the two terms of the middle of the second series. The property possessed by osmium and ruthenium of forming, with oxygen volatile combinations constituted in an identical manner, removes these metals from the platinum series and from all analogy with iron and manganese and connects them with the metalloids; while, on the contrary, their faculty for forming bases disconnects them from the metalloids. The following Table may help to elucidate the foregoing:—

Horizontal Series.

Positive end of the Horizontal Series.	Principal Series	Osmium	Iridium	Platinum	Vertical Series	Negative end of the Horizontal Series.
	Secondary Series	Ruthenium	Rhodium	Palladium		

Our knowledge of platinum metals has been considerably advanced by the new modes of analysis described by French chemists, particularly by the use of peroxide of barium for attacking osmide of iridium, by the new process of pulverising the ore, and by the large number of analyses of platinum ores already made. I have been particularly interested by the description of analyses of varieties of osmide of iridium,—analyses which lead to the conclusion that the quantity of ruthenium in these ores augments with the increase of osmium, and diminishes as the iridium augments,—a result incontestably proved by my own researches, and which is explained by the close connection existing between ruthenium and osmium. It has been equally well ascertained that osmide of iridium, in the form of clear, shining scales and tablets, is the ore containing the largest quantity of these two metals: while the dark gray, dendritic and dull ore often contains very little osmium or ruthenium, though, according to my researches, it is never entirely free from ruthenium. I was, consequently, greatly surprised that French chemists should in two cases have found a Russian ore containing no traces of it. The evident explanation of this fact is the difficulty there is in discovering ruthenium when it exists in the ore only in very small quantity. Fusion with saltpetre and potash would not in this case suffice to reveal the presence of ruthenium. In ores containing much osmium nothing is easier, because the osmium favours the attack on ruthenium. In this case, then, after fusion with saltpetre and potash, a solution of an intense orange colour is obtained, containing hardly any iridium. It is otherwise with ores containing little osmium and relatively, also, little ruthenium. According to the quantity of osmium present, these yield clear yellow, dark green, or even quite blue solutions, which contain much iridium dissolved to the state of iridiate of potash; so that in a blue solution the small quantity of ruthenium there present is entirely masked by the colour of the iridium. Moreover, all the ruthenium does not pass into the aqueous solution at the first fusion of the

powdered ore, but a notable portion of it remains with the undissolved iridiate of potass. This method becomes still more defective when the substance on which we act contains no osmium,—when, for instance, we commence, as our authors have done, by eliminating the osmium and then melting the residue of iridium, containing ruthenium, with potass and saltpetre. These circumstances, unknown to the French chemists, may explain why ruthenium has escaped their analyses.

When ruthenium cannot certainly be discovered by fusion with potass and saltpetre, and when it is necessary to separate it from the other metals, I make use of another method, which I have described elsewhere,—namely, by precipitating ruthenium from a solution of the metallic chlorides, by the addition of a small quantity of ammonia and by boiling. This method also has the disadvantage of succeeding only when there is a considerable quantity of ruthenium; nevertheless, there is at least the advantage of being able to separate the greater part of the other metals which impede the precipitation either by partial crystallisations or by consecutive treatments by water, after which a solution is obtained sufficiently rich in ruthenium to allow of its determination, not only qualitatively, but, in an approximate manner, quantitatively. To this end it is necessary to be acquainted with the following conditions on which this process is founded:—

1. Ruthenium is precipitated as sesquioxide by boiling from solutions rendered feebly alkaline by ammonia. This precipitate is a black, ruthenious sesquioxide. The same effect is produced with osmic chloride, which invariably gives the precipitate OsO_2 . Other metals, particularly iridium, give no precipitates under these circumstances. Red ruthenic chloride also yields in this way an ochre-yellow hydrate of ruthenioxide $\text{RuO}_2 \cdot 5\text{HO}$, but this precipitation is incomplete; it is much more certain in presence of ruthenious sesquichloride. Potash behaves like ammonia in this reaction, but its use is not advisable, on account of the facility with which it dissolves the oxide RuO_2 . No precipitate is produced if there is much iridium in the solution, but it is easy to determine the presence or absence of ruthenium. In fact, a solution of pure iridium, containing no ruthenium, is instantaneously decolorised by the addition of excess of caustic potass or ammonia. It remains for a long time thus perfectly transparent, and then, after several days, takes a beautiful blue colour. But if the iridium contains traces of ruthenium, the colour of the solution becomes fainter during the reaction, but does not disappear; it passes to a yellow-brown or reddish hue, and does not become decolorised for some time, at which point the solution becomes troubled and a slight brown or yellow precipitate is formed. If the iridium contains more ruthenium, yet not sufficient to produce a visible precipitate, its presence is betrayed by the intense red-purple colour assumed by the liquid, especially if potass is used. These are the most certain reactions for the qualitative determination of ruthenium.

2. Double salts of iridic chloride are not isomorphous with double salts of ruthenious sesquichloride; these salts then never crystallise together, but always separately. The latter are decidedly the most soluble. They are not so easily precipitated when a solution of ruthenious sesquioxide in hydrochloric acid is treated with this addition of ammoniacal salts in solution, as when salts of iridium in a solution of iridic chloride are used. Crystals of a salt of iridium are also much larger than those of a salt of ruthenium, to which circumstance the greater solubility of a salt of ruthenium is partly due. When

both salts are deposited as crystals, they are each insoluble in a dilute solution of sal-ammoniac, which affords a ready means of freeing them entirely from salts of rhodium and palladium chloride (not palladio chloride), which are freely soluble in the above solution.

3. The double salt of ruthenic chloride is isomorphous with the iridium salt, and can enter as a constituent part of an iridium combination. A small quantity of this salt is always produced simultaneously with the sesquichloride, when ruthenious sesquioxide is dissolved in aqua regia, and the larger the proportion of this menstruum, the more freely it is formed. This being the most soluble of all the double salts of the platinum metals, a large portion remains, even in presence of much iridium, in the mother-water, after the crystals of other salts are deposited.

Fractional Process of Precipitation and of Crystallization.—Osmide of iridium is melted in the ordinary way, with saltpetre and potash, and moistened with water; then carefully neutralised with sulphuric acid in order to precipitate the metallic acids in solution, always leaving the liquid with a slight alkaline reaction; then alcohol is added, and the whole boiled. Throw the mixture on a filter and carefully wash, in order to free it as far as possible from potash salts. The black metallic powder thus obtained still contains much potash, in the state of acid iridiates of potash, which is capable of converting the greater portion of the iridium into double iridic chloride, during the subsequent solution of the metallic powder. For this reason we evaporate to a considerable extent the aqua regia solution separated from the powder, until all the osmium is disengaged as osmic acid; then, if left to cool, the residue is deposited, most of the iridium is deposited in the state of double salt, and the liquid contains all the ruthenium, in a much greater degree of concentration, although always naturally associated with iridium.

We afterwards filter and add powdered sal-ammoniac, which causes the deposition of yet more iridium, as a black crystalline precipitate. The solution generally contains ruthenium so concentrated as to be separable from the iridium by precipitation. Now evaporate again and add more sal-ammoniac until the liquid begins to lose its colour, and most of the platinum metals are deposited as double salts; then leave it for several days, collect the residue on a filter, and wash it with water containing sal-ammoniac in solution, which completely eliminates iron and copper, and does not affect the ruthenium, and we get a residue of salts of iridium containing ruthenium, which, dissolved and boiled, with the addition of a little ammonia, yields a precipitate of ruthenious sesquioxide.

Fractional Method of Extraction.—This process is generally conducted in the same manner as the foregoing. The first quantities of double potassic salts, usually containing no ruthenium, are allowed to crystallise, and the liquid is separated from the crystals. Evaporate the liquid almost to dryness, adding the necessary quantity of sal-ammoniac, and wash the residue with a concentrated solution of sal-ammoniac, to free it from iron and copper. Treat the salt of iridium, containing ruthenium, first with a few drops of hydrochloric acid, so that the ruthenic combination undergoes no decomposition, then with a little water in a flat dish, and leave it for several days. Then filter, and we get a liquid containing most of the ruthenic salt, which, heated with ammonia, as indicated in a foregoing paragraph, yields a voluminous black precipitate, which

does not colour the liquid deeply. The salt of iridium, always charged with ruthenium, which remains on the filter, is again to be treated in a flat dish with a little water, and so on, repeating the same operation so long as ruthenium is dissolved, and until the salt of iridium is obtained pure. Collect all the ruthenium precipitates on the same filter. The aggregate is not then entirely pure, containing a small proportion of iridium; nevertheless, the surplus of iridium is compensated for by the loss always sustained in operating on solutions highly charged with iridium, in which a certain quantity always remains unprecipitated. The first solutions contain most ruthenium, yielding the largest and purest precipitates, without producing much coloration of the liquid. The later solutions, during precipitation, occasion the intense red colour characteristic of the mixture of salts of iridium and of ruthenium. After a time this reaction also disappears, but the solutions continue to yield the ruthenic reactions already described. It is better not to heat these solutions, but, adding a slight excess of ammonia, to leave them at rest at the ordinary temperature of the air, when they soon become turbid and produce a light brown or yellow precipitate, sparingly voluminous, consisting partly of a mixture of sesquioxide of ruthenium and ruthenic oxide, and partly of pure ruthenous oxide. In fact, the salt of iridium, an isomeric constituent with ruthenic chloride, is dissolved, and ammonia, without precipitating iridium, eliminates the yellow ruthenic oxide.

I employed this complicated method to obtain ruthenium from poor ores when unable to obtain rich ones. I have also made use of this mode of analysis in the absence of any better. Not till after the discovery of volatile hyper-ruthenic acid could I employ a simpler and more certain method,—a method based on the volatility of the above-mentioned combination, and on the supposition that the presence of iridium in no way hinders the formation of hyper-ruthenic acid. I was led to the use of this new method by an observation I made ten years ago, that a solution of ruthenium, treated by chlorate of potash and hydrochloric acid, loses all its ruthenium, under the form of a volatile product. Still, this method cannot be regarded as very simple, seeing that the newly-discovered volatile ruthenic combination necessitates certain precautions to avoid loss by volatilisation. There is no appreciable loss in melting ruthenium with caustic potash and saltpetre. A piece of paper soaked with alcohol does not blacken if held even for a long time over the heated mixture, and no smell of hyper-ruthenic acid is observable, though during the solution of the mixture a feeble odour of it is always given off. If ruthenious sesquioxide, which is obtained in treating this solution by alcohol, is dissolved in hydrochloric acid, the solution takes place without forming a volatile ruthenic combination; but there is some likelihood of loss if aqua regia is used as a solvent, for ruthenium is then placed under circumstances similar to those produced by the simultaneous action of chlorate of potash and hydrochloric acid. When it is necessary to estimate ruthenium precisely, the osmium must not be expelled previously by means of aqua regia, otherwise there would be some loss of ruthenium. Keeping these circumstances in mind, the following mode of analysis may be proposed, always admitting the supposition that other metals exercise no influence on the result. For the determination of ruthenium, a larger quantity of the metal than is generally used is required,—that is to say, at least ten grammes, which must all be reserved for the search for ruthenium, disregarding all the other consti-

tuents. Then melt the pulverised ore with potash and saltpetre, dissolve it in water, carefully neutralise it with an acid, and, adding some alcohol, heat it; then carefully wash the precipitate, which is a black metallic powder, in water, to eliminate the saltpetre and other potash salts. Dissolve this powder in hydrochloric acid, heat it in a retort with chlorate of potash and hydrochloric acid, and collect the volatile products in condensers containing alcohol. By this means ruthenium and osmium are obtained together, but their separation can be effected by alcohol, which instantly decomposes the ruthenic acid, though it takes some time to reduce the osmic acid; or, the two metals may be precipitated from their solution by sulphuretted hydrogen, and the sulphides thus obtained heated in a small, platinum vessel, in an oxygen current, which disengages sulphur and osmium as acids, while the ruthenium remains as ruthenic oxide. This is only a proposition, which I submit to other chemists, especially French chemists who have ruthenium at their disposal, and who take so much interest in the analysis of osmide of iridium. They will perhaps be induced to undertake these analyses when they read the present Memoir. My own attention being absorbed by other questions, these researches have for me but a secondary interest.

PHARMACY, TOXICOLOGY, &c.

Researches on Hypophosphite of Lime,
by M. GERARD JANSSEN.

1. Preparation.—The hypophosphites are seldom met with chemically pure. This is due to those terrible explosions to which the most skilful operator exposes himself in their preparation. Prompted by a well-grounded fear, he dare not conduct the process to the end, and the product obtained is more or less altered by foreign substances.

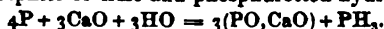
The properties attributed to such preparations cannot be those of the hypophosphites chemically pure. The author of these researches sought a new method of preparation, which would permit of obtaining these salts perfectly pure and without danger. He first investigated the causes of explosion. These he found to be due to, first, the action of heat and the oxygen of the air on the phosphuretted hydrogen (PH_3); second, the production of biposphuretted hydrogen (P_2H_4); third, the formation of a peculiar and very explosive compound, to which he gives the name of triphosphuretted bi-hydride (P_3H_2). This substance crystallises in hexagonal prisms. As long as the temperature of the mixture remains constant, there is no fear of explosion; but if, after the heat has decreased, it be then raised, the triphosphuretted bi-hydride is instantly decomposed into phosphorus and phosphuretted bi-hydride (PH_3), which takes fire spontaneously. Alcohol is the only compound which prevents these explosions, because it not only arrests the action of the oxygen of the air on the phosphuretted hydrogen and the production of biposphuretted hydrogen, but also, what is most important, the formation of phosphuretted bi-hydride.

To prepare the hypophosphite of lime, which may be taken as a type of these salts, a sufficient quantity of hydrate of lime is mixed with three times its weight of distilled water, to which a third of pure alcohol has been added. The mixture is introduced into a long-necked flask, heated gently in a sand-bath. When the mixture has attained a temperature of 50° to 60°C.

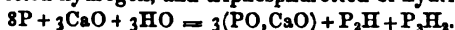
small pieces of phosphorus are gradually added until the action has entirely ceased. The apparatus is allowed to cool and the solution filtered through asbestos. The filtered liquid is freed from alcohol by distillation in a retort, the residual solution evaporated to perfect dryness, and the white powder thus obtained preserved in well-stoppered glass bottles.

The hypophosphite may be crystallised in the retort by a slow evaporation.

The action of the alcohol is thus explained:—The phosphorus, acting on the hydrate of lime by heat, gives hypophosphite of lime and phosphuretted hydrogen,

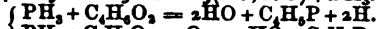
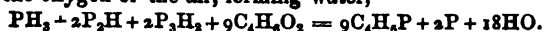


Two intermediate compounds are also formed,—biphosphuretted hydrogen, and triphosphuretted bi-hydride,



The alcohol decomposes these two compounds, forming phosphide of ethyl.¹ This again is decomposed by the base into hypophosphite and alcohol, a part, however, remaining mixed with the alcohol recovered by distillation. This may serve for a subsequent preparation.

The action terminates by a feeble evolution of hydrogen from the decomposition of the phosphuretted hydrogen, one part becoming combined during the operation with the oxygen of the air, forming water,



2. Properties.—Hypophosphite of lime is a white powder, inodorous, but of an acrid and burning taste. Deliquescent and very soluble in water; scarcely soluble in ether and absolute alcohol. Soluble in fixed and volatile oils, it is precipitated when they become rancid. It crystallises with difficulty; the crystals contain two equivalents of water ($PO, CaO, 2HO$). Its solution, exposed to the air, is transformed into carbonate and biphosphate of lime. It immediately decolorises a solution of sesquisulphate of manganese ($Mn_2O_3, 3SO_3$).

Sulphur, selenium, and tellurium also decompose it into phosphite, then into phosphate, with formation of sulphide, selenide, or telluride of calcium.

Boron, silicon, and carbon, decompose it by heat; borate, silicate, or carbonate of lime is formed, and phosphorus evolved in vapour.

Hydrogen decomposes it into phosphate and phosphuretted hydrogen.

Hydrochloric, hydriodic, hydrobromic, and hydrofluoric acids decompose it into phosphate, phosphuretted hydrogen, and a haloid salt.

Nitric acid converts it into phosphate, hyponitric acid being evolved.

The alkaline bases decompose it into phosphate and phosphide.

The alkaline carbonates when boiling, and the alkaline sulphates in the cold, transform it into an alkaline hypophosphite and carbonate or sulphate of lime.

It precipitates metallic gold from its solutions.

¹ The phosphide of ethyl thus obtained, when separated from the alcohol, is a limpid fluid, of a phosphuretted odour and burning taste. It is very volatile, burning at 9° C. It is soluble in alcohol; the solution has an opaline appearance. In water it separates in drops, like chloroform, which it also resembles in its anesthetic properties. It burns with an emerald-green flame, exhaling an intensely disagreeable odour. A little powdered tin, bismuth, or antimony, placed in the flame, causes the colour to pass to pale violet, and finally to pure white. The great danger which attends the preparation and manipulation of this substance has not yet permitted the author to study all its properties.

In a solution of nitrate of silver it produces a white precipitate, which rapidly turns brown; if the hypophosphite be added in excess in the cold it reduces the silver to the metallic state after some time; by the aid of heat the reduction is more rapid.

In a dilute solution of bichloride of mercury it produces a crystalline precipitate of chloride. If added in excess it reduces the mercury to the metallic state.

From a solution of sulphate of copper it precipitates the oxide, which a continued ebullition reduces to the metallic state.

3. Adulterations.—Hypophosphite of lime may contain phosphite, phosphate, or carbonate of lime. These three adulterations are separated by cold, distilled water, in which they are insoluble. The first gives off hyponitric acid when acted on by concentrated nitric acid; the second, calcined with a soluble salt of lead, gives globules which crystallise in regular polyhedrons; and the third gives off carbonic acid when treated with a strong acid.

It may also be adulterated with chloride of sodium, sulphate of lime, carbonate of magnesia, and oxide of zinc. These can be recognised by their usual tests.—*Répertoire de Chimie.*

On the Chemical Constituents of Coca Leaves,
by JNO. M. MAISCH.

THESE interesting leaves are beginning to attract considerable attention both in Europe and America; the accounts of learned travellers and of physicians, who have lived for a number of years in the districts where Erythroxylon coca is cultivated, and where the leaves are employed for the preservation of the bodily strength amid continued hardships, all these accounts agreeing in the main points, it was natural to expect that the reflecting physician would desire to investigate their adaptation for the cure of disease. The greatest drawback to such investigations is undoubtedly the limited supply of the leaves, but I believe a sufficient quantity might be found in the United States for experiments in our hospitals.

Several papers have within the last year found their way into the scientific journals; among these an analysis of the leaves performed by Dr. A. Niemann, in page 47, Vol. IV. of the CHEMICAL NEWS. The principal object of his investigations was to search for an alkaloid, which he separated in a pure state and analysed. The wax and tannin were separated incidentally in the process for liberating the alkaloid, so that we are still in the dark about its other constituents.

A few months ago, I obtained through the kindness of Professor Procter, a few ounces of coca leaves for the purpose of analysing them. I should have desired very much to pay attention likewise to those constituents which are probably of less importance in regard to their medical properties, but at all events I had to follow the course marked out by Dr. Niemann, so as not to lose the opportunity of obtaining a little of the new alkaloid, and study the other substances isolated by him.

Accordingly, 1500 grains of the coarsely-powdered leaves were exhausted in a funnel-displacer by alcohol mixed with a little sulphuric acid. The dark coloured brown-green tincture was agitated with an excess of milk of lime, which assumed a brownish-green colour; the filtrate therefrom, together with the washings with cold alcohol, was carefully neutralised with sulphuric acid, filtered from the greenish sulphate of lime and distilled. The aqueous residue separated but little resinous

matter, the filtrate therefrom was precipitated by carbonate of soda and the alkaloid gained in an impure state by shaking the mixture with ether and evaporating the light straw-coloured ethereal solution. The yield was four grains, and the alkaloid showed the behaviour described on page 43, Vol. IV. CHEMICAL NEWS.

On adding the milk of lime to the acid tincture, the peculiar tea-like odour of the leaves was strongly developed; but on the addition of the carbonate of soda another heavy and narcotic odour is observed; this is described by MacLagan and Niemann as reminding of nicotina. I observed this slight resemblance, but still the odour is very different.

I likewise found Dr. Niemann's statements correct regarding the vegetable wax and the tannin; though, owing to the small quantity operated on, I was unable to obtain the former in a snow-white condition. The second acid, which Niemann considers as tannin modified by nitric acid, I obtained also in solution contaminated with some of the latter acid.

The alkaloids of coffee and cacao, which, though in a less degree, possess properties similar to those of coca, contain more nitrogen than any other alkaloid known. The small per-centage of nitrogen ascertained in cocaina by Dr. Niemann, suggested the idea that it might probably be a product of decomposition, and that the odour developed by carbonate of soda, might perhaps be a nitrogenous body or another alkaloid. To ascertain this, and to find out whether one or two acids were contained in the tincture, the above process was repeated with the balance of leaves, weighing less than an ounce. The liquid, together with the precipitate occasioned by carbonate of soda, was introduced into a retort, the neck of which was connected with a bent glass tube, dipping into a little dilute muriatic acid. When heated over the spirit lamp, the concussions were so violent that it was deemed advisable to place the retort in a sand bath, so as to supply from the sides sufficient heat to distil the volatile substances, until a syrupy liquid remained behind, from which the alkaloid was separated by ether.

The muriatic acid, which was perfectly clear and colourless, now had an odour somewhat different from the original odour of the alkaline liquid, but it resembled that of the residue in the retort. The acid solution was rendered alkaline by carbonate of soda and agitated with ether, which took up the odorous principle leaving it behind on spontaneous evaporation as a soft resin-like coating. The odour of this mass is heavy, narcotic; the taste similar, somewhat acid; the behaviour to test paper, neutral, but exposed to the damp atmosphere of a rainy day, it had almost completely lost its odour and acquired an acid reaction and taste, and was now easily soluble in water and alcohol; the solubility of the odorous mass in these menstrua had not been tried. I omitted to mention that the hydrochloric acid solution was tested with the usual reagents for alkaloids: phosphomolybdic acid, and the chlorides of gold, mercury and platinum, without being disturbed by either.

This odorous principle, which I regard as a product of decomposition of some constituent, is therefore volatile with the vapours of water in a slight degree, but very changeable at least in a moist atmosphere. It is likely to be of great importance, as it is stated by several observers that an infusion of the leaves has different properties from the same leaves chewed with an alkali. Whether or not it is nitrogenous, must at present be left undecided.

The solution from which the alkaloid had been removed by ether, was digested with precipitated carbonate of

baryta, which removed all the sulphuric acid, and acquired a pale greyish-white colour. The filtrate was neutralised with acetic acid, and after the expulsion of the carbonic acid, rendered very slightly alkaline by a few drops of liquor ammoniac. An aqueous solution of neutral acetate of lead was now carefully dropped in, as long as a dirty greenish-brown precipitate was produced, which is voluminous, but settles in a few minutes and may be readily filtered and washed. On the further addition of neutral acetate of lead, a precipitate is separated which is at first yellowish-white, but settles readily to a greyish yellow.

The liquid filtered from this last precipitate is not disturbed on the further addition of sugar of lead, but yields with basic acetate of lead a nearly white voluminous precipitate, which is insoluble in water, but readily soluble in acids. Corrosive sublimate and deuto-nitrate of mercury do not disturb it; protonitrate of mercury yields a whitish precipitate, insoluble in water, almost entirely soluble in nitric acid; the white precipitate by nitrate of silver is scarcely taken up by nitric acid, but readily by ammonia. The liquid contains a small quantity of muriatic acid, but besides it an organic acid which imparts to it a brownish-yellow colour. This colour becomes deeper on exposure to the air, and after twenty-four hours' standing neutral acetate of lead produces a precipitate which resembles in colour the yellowish-white precipitate spoken of before. The filtrate was now of a pale yellow colour, and was not further disturbed by acetate or subacetate of lead after exposure to the air.

The first of the above-mentioned lead compounds dried to a dirty yellowish-brown powder; the second to a powder of a similar, but lighter colour.

The alkaloid which had been separated by ether from the alkaline solution left behind in the retort, was obtained as a transparent mass of the consistence of balsam tolu, a somewhat lighter colour, and of a heavy narcotic odour. All attempts to crystallise it failed, but it had still an alkaline reaction to test paper, was insoluble in water, but soluble in acids, which it completely neutralised. The solution in hydrochloric acid yielded with alkalis and their carbonates a white precipitate soluble in ammonia, with phosphomolybdic acid a voluminous yellowish-white, with bichloride of platinum a yellowish-grey, with terchloride of gold a bright yellow, with bichloride of mercury a voluminous white, afterwards flocculent, with iodine an orange-brown precipitate; no coloration was produced by this alkaloid with oxidising agents.

It will be seen from these experiments that this uncrystallisable alkaloid had very nearly the same behaviour as pure cocaina, the principal difference being the reaction with iodine, which, with the former, is of a somewhat lighter colour with a slight yellow tinge. Heat, it appears, therefore, renders it amorphous; what part the water and carbonate of soda take in this change must be determined by experiments with the pure alkaloid. But it is very likely that the yellowish-brown matter, which Dr. Niemann found so difficult to separate from cocaina, is this same alkaloid in an amorphous state.

My experiments likewise render it more than probable that the substance named cocatannic acid by Dr. Niemann, is a product of oxidation, the original compound of which yields with acetate of lead from a slightly ammoniacal solution no precipitate, but after previous oxidation by the atmosphere, precipitates it, and the precipitate has, according to the length of exposure, a colour varying from white to yellow and brown. As it

is so readily affected by the atmospheric oxygen, great care is necessary in its separation from the other constituents.—*American Journal of Pharmacy.*

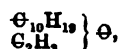
PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, December 5, 1861.

Dr. W. A. MILLER, F.R.S., in the Chair.

Dr. OPPENHEIM read a paper "*On the Camphor of Peppermint.*" This substance had been known for some time, but had lately been introduced in larger quantities than formerly. It was imported chiefly from Japan, packed in earthen vessels and presented an appearance resembling crystallized sulphate of magnesia, which in fact was sometimes employed to adulterate it. When washed with warm water and fused, it resembles common camphor; it melts at 36°C ., and boils at 210° ; these properties coinciding with those of the stearoptene examined by Dumas, obtained from American oil of peppermint. Its composition, as determined by analysis, agreed with the formula $\text{C}_{10}\text{H}_{16}\text{O}$. The corresponding hydro-carbon had also been prepared, its composition being $\text{C}_{10}\text{H}_{18}$. Some doubt still existed as to the constitution of this camphor, which appeared to border both on fatty and aromatic bodies; it differs from the camphor of Borneo by two equivalents of hydrogen, from ordinary camphor by four equivalents, and from capric acid by two of oxygen; it turns the plane of polarization to the left, and when crystallized from an alcoholic solution of soda, forms long needles. When exposed to the action of gaseous sulphurous or hydrochloric acid a compound is formed, but after some time the gas is given off and the camphor left; concentrated acids dissolve it freely, and it is reprecipitated by dilution or by neutralization with alkali, as an oily liquid which soon changes to the solid state. When heated to 170°C . with concentrated acetic acid the two combine to an oil which is not decomposed by water, and which distills at about 223° ; the same reaction takes place with the anhydrous acid, and the composition of the body formed is represented by the formula



one equivalent of hydrogen being substituted by acetyl. This substance turns the plane of polarization to the left more strongly than the camphor, an alcoholic solution of soda decomposes it with reproduction of the original camphor. Butyric acid also combines with this substance, and corresponding compounds with hydrochloric and hydriodic acid exist. When exposed to the action of sodium, hydrogen is evolved and a compound containing about one equivalent of sodium is obtained, which is decomposed by water but dissolves in alcohol and iodide of ethyle; on heating the latter solution crystals of an organic body containing iodine are deposited, and eventually iodide of sodium is formed. From these properties it was concluded that this camphor was a monatomic alcohol belonging to the series represented by the general formula $\text{C}_n\text{H}_{2n}\text{O}$: the name "menthol" was proposed for this body. By the action of anhydrous phosphoric acid or chloride of zinc, the corresponding hydro-carbon was obtained, and by exposing this to the action of bromine, a compound, $\text{C}_{10}\text{H}_{17}\text{Br}$, was formed, which was decomposed by potash with production of the hydro-carbon, $\text{C}_{10}\text{H}_{18}$. The corresponding acid had not as yet been formed, oxidising agents attacking the carbon of the camphor instead of the hydrogen.

Dr. ODLING thought that the experiments detailed did not prove this body to be an alcohol.

Dr. OPPENHEIM replied that, according to Berthollet's definition, an alcohol is an organic body which does not contain nitrogen, and which combines with acids giving up an equivalent quantity of hydrogen.

Mr. CHURCH remarked that the process for the formation of campholic acid by passing camphor vapour over soda-lime, contained in a tube and heated, yielded it in very small quantities, and that he should be glad to hear of a better process.

Dr. FRANKLAND having taken the chair,

Mr. J. C. FOSTER read a paper, "*On Piperic and Hydropiperic Acid.*" From the analyses that had been formerly made of piperic acid, three formulæ had been proposed, namely, $\text{C}_{22}\text{H}_{12}\text{O}_8$, $\text{C}_{30}\text{H}_{22}\text{O}_{10}$, and $\text{C}_{30}\text{H}_{22}\text{O}_{10}$, the last being preferred; the formula $\text{C}_{12}\text{H}_{10}\text{O}_4$ had also been given, the acid being considered monobasic. Mr. Foster's analyses confirmed the formula $\text{C}_{12}\text{H}_{10}\text{O}_4$ for the acid, and $\text{C}_{12}\text{H}_8\text{MO}_4$ for its salts. He had thought it possible that by the action of reducing agents a compound of the formula $\text{C}_{12}\text{H}_{10}\text{O}_2$ might be formed, and with this view the action of hydriodic acid was tried; this substance, however, charred the acid at a temperature below 100° . When submitted to the action of an amalgam of sodium, piperic acid yields a substance containing two additional equivalents of hydrogen, for which he proposed the name of hydropiperic acid, the composition being $\text{C}_{12}\text{H}_{12}\text{O}_4$. The hydropiperic acid melts at 64° and solidifies at about 56° , is not volatile without decomposition, is very slightly soluble in cold, more soluble in hot water, and on cooling the hot aqueous solution, it separates as an oil, which afterwards solidifies; it is soluble in alcohol and ether and may be obtained in large crystals from these solutions; with strong sulphuric or nitric acid it gives a red colour, and with dilute nitric acid a nitro product; when heated with excess of alkali, an acid of the formula $\text{C}_7\text{H}_6\text{O}_4$ was formed, which had already been prepared from piperic acid by the action of potash, and had been named proto-catechuric acid. Various salts of the hydropiperic acid were then described, among others an acid potash salt prepared by boiling carbonate of potash with an alcoholic solution of the acid; this salt was gradually decomposed by recrystallization, and completely by solution in water, the neutral salt being produced. The ether was prepared by passing hydrochloric acid gas into an alcoholic solution of the acid, a brown oil being formed, which, by the action of gaseous ammonia, furnished a substance which was probably hydropiperamide. With chloride of acetyl a neutral oil was obtained in which one equivalent of the hydrogen of the acid was replaced by acetyl.

Dr. FRANKLAND remarked that there was still a possibility of a reduction of piperic acid, although one or two reducing agents had failed; for since weak oxidizing agents were sometimes found to act more completely on certain bodies than powerful ones, the same might be found to be the case with reducing agents.

Dr. ODLING inquired whether hydropiperic acid was an appropriate name for a substance having such a relation with piperic acid.

A paper by Dr. DAUBENTY, "*On the Power Possessed by the Roots of Plants of Rejecting Poisons,*" was then read. The results of some former experiments in which the poisonous substances were applied to the plants after they had grown up, had already been communicated to the Society; in the present experiments these were introduced into the soil before the seed was sown; the substances tried being nitrates of baryta and of strontia, and arsenic acid instead of the arsenious acid used in the former experiments; barley and turnips were the crops employed, and the introduction of the poisonous bodies seemed rather favourable than otherwise as to the quantity of crop obtained. Neither of the substances tried were absorbed under these circumstances, and the only question that remained was whether the quantities employed were sufficient; a larger dose of arsenic, however, killed the plants,

but he proposed to try the effect of larger doses of the baryta and strontia salts.

Dr. FRANKLAND remarked that perhaps the plant absorbed very small quantities of these substances, and afterwards rejected them, so that the amount present in the plant at any one time was not sufficient to be detected.

The next paper was by Professor BOLLBY, "*On the Physical Properties of Alloys of Lead and Tin.*" The specific gravity of these alloys was subject to great variation, but an alloy containing equivalent quantities of lead and tin appeared to stand lowest in the series, an increase in the quantity of either metal causing an increase in the gravity. From this it was concluded that it was unsafe to infer what were the proportions of the metals in the alloy, from the specific gravity. With respect to the melting points there were great discrepancies in the determinations, but the solidifying point seemed to be more trustworthy; two solidifying points were observed to exist in every case, for an alloy of the composition Sn_2Pb remained liquid, the excess of metal, whether tin or lead, solidifying first. This alloy solidified at 181°C , which was lower than the melting point of either tin or lead.

The next paper was also by Professor BOLLBY, "*On the Dangers Arising from Feeding Steam Boilers with Certain Waters.*" It had hitherto been considered that waters containing only the carbonates of lime and magnesia were better for feeding boilers than those containing also sulphate of lime, because the sulphate made the deposit more stony, and the usual remedy recommended in the case of a water containing sulphates, was an alkaline carbonate, which decomposed the sulphate of lime; he had, however, found that some boilers which had been fed with water containing no sulphates, formed a stony incrustation to such an extent that the iron flue passing through the boiler became red hot. On inquiry it appeared that the refuse of some bleaching works was discharged into the river from which the supply was drawn. This refuse had a small quantity of fatty matter with it, which, although in very small quantity, completely prevented the deposit in the boiler being wetted by the water, and in fact, by digesting the deposit with ether, a perceptible quantity of fat was obtained. The same stony deposit was formed in another boiler in which the feed water was taken from the condenser of an engine, and in this case, also, a small quantity of grease was found to be the cause, and the addition of a little carbonate of soda to the water completely overcame the difficulty. The flues had only become red-hot when coal was substituted for wood, which had been previously employed as fuel, the coal giving a more intense local heat.

Mr. PERKIN remarked that he had found common clay, added in small quantities to the feed-water, prevented the adhesion of the deposit to the boiler.

Dr. FRANKLAND alluded to a similar case of a boiler forming a thick stony deposit, which had come under his own notice; in this case the feed-water was contaminated with organic matter, which caused it to have a sort of glutinosity; it might be poured from a great height without breaking into spray. The organic matter in this case was volatile, and was derived from the refuse of a starch factory, which was discharged into the river from which the supply was drawn.

NOTICES OF PATENTS.

663. *Manufacture of Gas from Coal, Oil, or Oleaginous Substances.* J. I. TAYLOR, Manchester. Dated March 16, 1861.

THE present invention relates to improvements upon an apparatus for manufacturing gas from the substances named

in the title, for which the patentee formerly obtained letters patent bearing date March 21, 1860. These improvements consist in the insertion of one or more diaphragms within the body of the retort, which otherwise retains the shape of that formerly used, the object of such addition being the increase of the heating surface, and, by a longer passage for the gas, more thoroughly effecting the conversion of the cily substances. With a view to economising the waste heat of the furnaces supplementary drying ovens and water-tanks are provided, the latter being connected directly with the retort and serving as a condenser for the liquid products of decomposition.

678. *Soap.* C. N. KOTTULA, Holborn, London. Dated March 18, 1861.

THIS invention refers to the introduction into soap of one or more of the following substances:—Bees'-wax, Japan wax, stearine, sperm, and similar materials. The patentee recommends also the combination of resin with soap, employed alone, or conjointly with one or more of the substances already named. These materials should be dissolved previously to their being mixed with the bulk of the soap.

The addition of yellow bees'-wax, or the white wax of Japan, would raise the cost of production without contributing in the same measure to improve the excellence of the soap. It is possible that from the high fusing points of these materials the compound soaps would be firmer, or could be made to hold a larger proportion of water. The addition of resin is an old expedient, and frequently practised in the manufacture of the lower qualities of soap.

717. *Soap.* F. G. WAGON, Issy, near Paris. Dated March 22, 1861. (Not proceeded with.)

THE inventor purposes to employ, as an earthy base in the manufacture of soap, carefully-washed kaolin, or China clay. This is recommended to be mixed by trituration with caustic potash, dissolved in water in the proportion of about one ounce of potash in the pint of water, and then added to the bulk of soap. For toilet use, the employment of glycerin and the ordinary perfumes is specially advised.

Of the mineral substances employed in the adulteration of soap, kaolin is that most frequently introduced. Its addition, like that of resin described in the preceding specification, is not calculated to improve the quality, but rather to increase the weight of the soap, serving only the same purpose as was pointed out by us¹ in reference to the manufacture of paper according to certain systems adopted in foreign countries.

684. *Preparation of Fish and Marine Animals for Manure.* J. JERRELL, Molde, Norway. Dated March 19, 1861. (Not proceeded with.)

By means of crushing rollers and suitable machinery it is proposed to break up the bony structure of the fish, and to subject the comminuted material to the action of a centrifugal drying apparatus and to an after desiccating treatment. By such a process it is stated to be possible to preserve the animal matters from decomposition until they are actually spread on the land to serve the purpose of manure.

697. *Preparing Caoutchouc for Dental Purposes, &c.* R. A. BROOMAN, Fleet Street, London. A Communication. Dated March 20, 1861. (Not proceeded with.)

THE inventor treats the ordinary red caoutchouc of commerce in the following manner to render it suitable for moulding dental pieces, (artificial gums,) and other objects to which the flesh-coloured material is applicable:—The red caoutchouc, coloured with vermilion or other pigment,

is first softened by sulphide of carbon, ether, chloroform, or other solvent employed in proportions varying from one quarter up to an equal weight with the caoutchouc; when so softened or dissolved an appropriate quantity of any of the white pigments specified is added for the purpose of altering the colour from red to pink, and to increase the brilliancy a little carmine may occasionally be added with advantage. Oxide of zinc is used alone or in conjunction with carbonate or sulphate of baryta, sulphate of strontia, or of manganese, phosphate of lime, calcined alumina or silica, or the latter in its precipitated condition. The decolourising agent is usually introduced in quantity about equal by weight to that of the red caoutchouc, and in some cases a small proportion of ordinary caoutchouc solution is required for the purpose of giving the paste more elastic qualities. After moulding, the dental piece or other object must be solidified by exposure in a closed vessel to temperatures ranging between 300 and 335 degrees Fahrenheit, from half to three-quarters of an hour being generally sufficient to communicate the requisite degree of hardness. The flesh colour will be improved by exposing the piece to the influence of the sun's rays, either directly or when immersed in a bath of alcohol.

Grants of Provisional Protection for Six Months.

2356. George Roberts, Bessborough Place, Pimlico, Middlesex, and Frederick Lambe, Cushion Court, Old Broad Street, London, "Certain improvements in lamps and lamp wicks, whereby they are adapted for more effectively burning animal, linseed, and other heavy and also glutinous and bituminous oils, and other burning fluids employed for illuminating purposes."—Petition recorded September 20, 1861.

2527. William James Williams, Warnford Court, London, "An improved process of charging illuminating gas with the vapour of the hyduret of carbon, for the purpose of increasing its illuminating properties."—Petition recorded October 10, 1861.

2553. Robert Charles Furley, Edinburgh, Midlothian, N.B., "An improvement in coating pills for rendering them tasteless."—Petition recorded October 12, 1861.

2725. William Cook, Beaufort Villas, Brixton, Surrey, and Henry Cook, Manchester, "Improvements in printing telegraphs."—Petitions recorded October 30, 1861.

2781. John Peter Bourquin, Newman Street, Oxford Street, London, "Improvements in ornamenting the covers of photographic albums, books, writing cases, and other like articles."

2783. Henry Orth, Wissenbourg, France, "An improved soap."

2784. George Tomlinson Bousfield, Loughborough Park, Brixton, Surrey, "Improvements in electro-plating or depositing metals."—A communication from Jabez Ellis, Walcot, Boston, Massachusetts, U.S.—Petitions recorded November 5, 1861.

2787. Alexander Prince, Trafalgar Square, Charing Cross, London, "Improvements in furnaces for reducing zinc ores."—A communication from Adolphe Charlier, Stolberg, near Aix-la-Chapelle, Prussia.

2795. John Richardson Wigham, Capel Street, Dublin, "Improvements in apparatus for the manufacture of gas, parts of which are also applicable for cooking purposes."

2800. William Albert Shepard, Pall Mall, London, "Improvements in preparing and treating gutta-percha and india-rubber."

2101. John Barrow, Dalton Chemical Works, West Gorton, near Manchester, Lancashire, Improvements in the manufacture of benzole, naphtha, naphthaline, aniline, and carbolic acid."—Petitions recorded November 7, 1861.

2819. Richard Archibald Brooman, Fleet Street, London, "Improvements in obtaining alkaline phosphates."—A communication from Edouard Aubertin, Paris.

2822. William Edward Newton, Chancery Lane, London, "Improved apparatus for manufacturing and containing gaseous liquids."—A communication from Pierre Prudence Henri Couillard, Rue St. Sébastien, Paris.

2831. George Fergusson Wilson and George Payne, Sherwood Works, Battersea, Surrey, "Improvements in treating fatty and oily matters."

2844. Louis François Duval and Louis Andre Beaudet, Boulevard St. Martin, Paris, "A new process of tanning."—Petitions recorded November 12, 1861.

2850. William Clark, Chancery Lane, London, "The application of electricity in refining cast iron for the purpose of converting it into wrought iron or steel with or without the addition of other agents."—A communication from François Alphonse Adéodat Dufournel, Boulevard St. Martin, Paris.

2860. Richard Archibald Brooman, Fleet Street, London, "Improvements in albums for containing photographs and other pictures."—A communication from Auguste Marion, Paris.

Notices to Proceed.

2540. Charles Noyes Kernot, Gloucester House, West Cowes, Isle of Wight, Southampton, and Martin Diederich Rucker, Fenchurch Street, London, "Improvements in the method of obtaining ammoniacal salts and other valuable products from liquors or substances containing ammonia, and for utilising the residuum."—Petition recorded October 11, 1861.

2754. John Charles Wilson, Bucklersbury, London, "Improvements in machinery or apparatus for the manufacture of sugar."—Petition recorded November 2, 1861.

1774. Rees Taylor and Thomas Price, Tydu Tin Plate Works, Bassaleg, Monmouthshire, "Improvements in the manufacture of tin andterne plates."

1792. Charles Denton Abel, Southampton Buildings, Chancery Lane, London, "Certain new alloys of silver with other metals, and the processes employed in their manufacture."—A communication from Henri Catharine de Rouz, Paris.—Petitions recorded July 16, 1861.

1819. Robert Laing and George Horwood Cossins, Ince, near Wigan, Lancashire, "An improved mode of obtaining nitrous acid gas for making sulphuric acid."

1836. Constantine Nicholas Kottula, Holborn, London, "Certain new compositions to be used in the manufacture of soap."

1956. William Clark, Chancery Lane, London, "Improvements in bleaching and clarifying saccharine matters, and in apparatus for the same."—A communication from Messrs. Edouard Théophile de Gemini and Edmond Oswald de Gemini, Boulevard St. Martin, Paris.—Petition recorded August 6, 1861.

2073. Thomas Sutton, King's College, London, "An improved camera for taking photographic portraits and instantaneous pictures."—Petition recorded August 20, 1861.

2860. Richard Archibald Brooman, "Improvements in albums for containing photographic and other pictures."—A communication from Auguste Marion, Paris.—Petition recorded November 13, 1861.

CORRESPONDENCE.

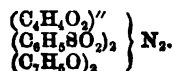
Chemical Nomenclature.

To the Editor of the CHEMICAL NEWS.

SIR,—As it appears to be the fashion to find fault with our unfortunate chemical nomenclature and to propose new systems, perhaps I may be allowed to make a few remarks on the subject. I have read with interest Mr. Newlands' ingenious proposal for the simplification of chemical nomenclature, but, unless I am somewhat mistaken, it resembles very much the methods proposed by Dumas, Gmelin, and

Laurent, not only in its principles, but also in its results. These celebrated chemists were induced to abandon, or at any rate to modify to a very considerable extent, their methods of nomenclature, on account of the very complicated names at which they arrived; and I am almost afraid that Mr. Newlands has scarcely overcome this inconvenience, as his name for acetic acid (dagdatic acid) is not very much simpler than Laurent's term, "linavinam" for alcohol, and, in Laurent's words, I fear that, "in spite of their shortness, they have no chance of being adopted." Again, as the number of elements contained in the body increases, so the name becomes more complicated; thus, for ordinary alum we have the oft-quoted appellation, atolan-telmin-ojafin-weso. This name, although expressing most completely the composition of alum, even to the water of crystallisation, I think would not be adopted by many chemists. It would be well, certainly, to assign names expressing the composition to every new body that is discovered; but it would be somewhat difficult to change the names of all existing bodies, although there would certainly be many advantages in making the name of a substance express its composition.

There is one thing that ought certainly not to be lost sight of in any new nomenclature that may be proposed, and that is, that the constitution and type of a body should be expressed by its name as far as possible. This, as far as I know, has only been done in the case of the derivatives of ammonia, which is really the only part of the present chemical nomenclature that is at all systematic. Here we find something like what a nomenclature ought to be. The derivatives of this type, when basic, are called amines, and when neutral, amides. A basic compound derived by the replacement of hydrogen in one equivalent of ammonia is called a monamine; one derived from two equivalents of ammonia is a diamine; and one from three equivalents, a triamine, &c. These, again, are divided into primary, secondary, and tertiary monamines, diamines, triamines, &c. If the nitrogen in ammonia and its derivatives be replaced by phosphorus, arsenic, or antimony, we then have the phosphines, arsines, and stibines, which are divided into classes in the same manner as the nitrogen compounds. The most complicated of all these bodies is expressed by a simple name, although probably a long one. Let us take, for example, the compound called succinylsulphophenylidibenzoyldiamide. This is certainly a long word, containing not less than fifteen syllables, but it immediately tells us, as well as a name can, what this body contains and some of its properties. Beginning at the end of the word, we find the substance is a diamide,—that is, a neutral derivative, produced by the replacement of hydrogen in two equivalents of ammonia, and the hydrogen is replaced by one equivalent of succinyl, two of sulphophenyl, and two of benzoyl. The formula is, therefore:—



The name does not give us the composition of the radicals. Some proposal that would give short, concise, and expressive terms to these radicals would be a great advantage.

As this portion of our nomenclature seems so simple, it would not be advisable to alter it, at least, not at present; and if a nomenclature that would not interfere with the designation of the derivatives of ammonia could be found, it would stand more chance of adoption. It scarcely becomes one like myself to suggest any such system, but perhaps I may be allowed to say a few words in explanation of a system which has struck me as being not altogether destitute of advantages, and which, although I cannot and do not expect that it will ever be adopted, may prove acceptable to some of your readers, who may be able to improve upon it.

Chemical compounds were arranged by Gerhardt under four types,—viz., hydrogen, HH; hydrochloric acid, HCl; water, H₂O; and ammonia, H₃N. However, hydrochloric acid is a derivative of hydrogen; therefore, all its derivatives are also derived from hydrogen. Consequently, the hydrochloric acid type may be left out of consideration as a separate type. Since Gerhardt's time, however, another type has been proposed,—viz., the marsh gas, H₄C, or the silicuretted hydrogen type, H₄Si.

The names of the ammonia derivatives we have already spoken of; so we have the remaining three types,—the hydrogen, HH; the water, H₂O; and the marsh gas, H₄C.

We have first to fix upon a general termination for the derivatives of the three great types; and in the first type, that of hydrogen, *ide* might be convenient, as this type embraces all the hydrides, chlorides, bromides, iodides, fluorides, &c.; and as the termination *ide* would remind us of these combinations, it might be thought applicable in this case, but, for reasons that will appear immediately, the French termination *ure* will be more appropriate. Thus, as ammonia is trihydramine (I never heard it called by this name), hydrogen will become dihydrure, and the designations of the derivatives of hydrogen follow as a matter of course.

The one equivalentic metals, potassium for example, will be called dipotassure; and the two equivalentic metals, as platinum, will be platincure. (These terms are never likely to be used, any more than the name potasside of potassium for KK.) In the following list are a few of the organic derivatives of hydrogen:—

Hydride of ethyl C₂H₅H ethylhydrure.
Dihydride of ethylene (C₂H₄)''H₂ ethylenedihydrodiure, or more simply, ethylenedihydure.

Ethyl C₂H₅ C₂H₅ diethylure.
Methylethyl CH₃, C₂H₅ methylethylure.

Some of these terms are nearly as simple as the ordinary ones, but I cannot say this for the diethylure. The acid hydrides are:—

Hydrochloric acid HCl hydrochlorure.
Water H₂O dihydro-oxydiure, or dihydroxure.
Ammonia H₃N trihydronitrotriure, or trihydroniture.

Marsh gas H₄C tetrahydrocarboretture, or tetrahydrocarbure.

The derivatives of hydrochloric acid are named without difficulty:—

Chloride of potassium KCl potassiochlorure.
Dichloride of platinum Pt''Cl₂ platino-dichlorodiure, or platino-dichlorure.

Trichloride of gold Au''Cl₃ aurotrichlorotriure, or aurotrichlorure.

Chloride of ethyl C₂H₅Cl ethylchlorure.
Dichloride of ethylene (C₂H₄)''Cl₂ ethylenedichlorodiure, or ethylenedichlorure.

Tribromide of glyceryl (C₃H₅)'''Br₃ glyceryltribromotriure, or glyceryltribromure.

Chloride of acetyl C₂H₃OCl acetylchlorure.
Dichloride of succinyl (C₄H₃O₂)''Cl₂ succinylchlorodiure, or succinylchlorure, &c.

The water type includes the oxides, sulphides, selenides, tellurides, oxy salts, sulphur salts, &c. The most convenient termination for this series is probably *ide*, as the terms oxide and sulphide are so familiar. If this termination were employed for the hydrogen derivatives, we should be compelled to look for a different termination for this type. The termination *ate* might be used, but I think it would scarcely be so appropriate.

Water $\begin{array}{c} H \\ H \end{array}$ } O dihydroxide.

Sulphuretted hydrogen $\begin{matrix} \text{H} \\ \text{H} \end{matrix}$ S dihydrosulphide.
As seldom to be used as oxide and sulphide of hydrogen.

Hydrate of potassium $\begin{matrix} \text{K} \\ \text{H} \end{matrix}$ O potassiohydroxide.

Sulphhydrate of potassium $\begin{matrix} \text{K} \\ \text{H} \end{matrix}$ S potassiohydrosulphide

Hydrogen being as much a radical as potassium, deserves to be represented in the name of the compounds of which it forms a part.

Oxide of potassium $\begin{matrix} \text{K} \\ \text{K} \end{matrix}$ O dipotassio-oxide.

Sulphide of potassium $\begin{matrix} \text{K} \\ \text{K} \end{matrix}$ S dipotassio-sulphide.

Oxide of platinum $\begin{matrix} \text{Pt}'' \\ \text{O} \end{matrix}$ platinico-oxide.

Hydrate of platinum $\begin{matrix} \text{Pt}'' \\ \text{H}_2 \end{matrix}$ O₂ platinico - dihydrodi-oxide.

Oxide of bismuth $\begin{matrix} \text{Bi}''' \\ \text{Bi}''' \end{matrix}$ O₂ dibismuthotrioxide.

Hydrate of bismuth $\begin{matrix} \text{Bi}''' \\ \text{H}_2 \end{matrix}$ O₂ bismutho - trihydrotri-oxide.

Alcohol $\begin{matrix} \text{C}_2\text{H}_5 \\ \text{H} \end{matrix}$ O ethylhydroxide.

Ether $\begin{matrix} \text{C}_2\text{H}_5 \\ \text{C}_2\text{H}_5 \end{matrix}$ O diethyloxide.

Glycol $\begin{matrix} (\text{C}_2\text{H}_4)'' \\ \text{H}_2 \end{matrix}$ O₂ ethylenedihydrodi-oxide.

Oxide of ethylene $\begin{matrix} (\text{C}_2\text{H}_4)'' \\ \text{O} \end{matrix}$ ethyleneoxide.

Glycerine $\begin{matrix} (\text{C}_3\text{H}_5)''' \\ \text{H}_2 \end{matrix}$ O₂ glyceryltrihydrotri-oxide.

$\begin{matrix} (\text{C}_3\text{H}_5)''' \\ (\text{C}_3\text{H}_5)''' \end{matrix}$ O₂ glyceryltri-oxide.

Hypochlorous acid $\begin{matrix} \text{H} \\ \text{Cl} \end{matrix}$ O hydrochloroxide.

Chloric acid $\begin{matrix} \text{H} \\ \text{ClO}_2 \end{matrix}$ O hydrochloryloxi-oxide.

Sulphuric acid $\begin{matrix} \text{H}_2 \\ (\text{SO}_2)'' \end{matrix}$ O₂ dihydrosulphuryldi-oxide.

Phosphoric acid $\begin{matrix} \text{H}_2 \\ (\text{PO})'' \end{matrix}$ O₂ trihydrophosphoryl-oxide.

$\begin{matrix} \text{H} \\ \text{Cl} \end{matrix}$ O hydrochloroxide.

$\begin{matrix} \text{H} \\ \text{ClO}_2 \end{matrix}$ O hydrochloryloxi-oxide.

$\begin{matrix} \text{H}_2 \\ (\text{SO}_2)'' \end{matrix}$ O₂ dihydrosulphuryldi-oxide.

$\begin{matrix} \text{H}_2 \\ (\text{PO})'' \end{matrix}$ O₂ trihydrophosphoryl-oxide.

Hypochlorous acid $\begin{matrix} \text{H} \\ \text{Cl} \end{matrix}$ O hydrochloroxide.

Hydrate of potassium $\begin{matrix} \text{K} \\ \text{H} \end{matrix}$ O potassiohydroxide.

The nomenclature of the anhydrides is of course similar to that of the anhydrous oxides,

Hypochlorous an- $\begin{matrix} \text{Cl} \\ \text{Cl} \end{matrix}$ O dichloroxide.

hydride $\begin{matrix} \text{ClO}_2 \\ \text{ClO}_2 \end{matrix}$ O dichloryloxi-oxide.

Chloric anhydride $\begin{matrix} \text{ClO}_2 \\ \text{ClO}_2 \end{matrix}$ O dichloryloxi-oxide.

Sulphuric anhydride $\begin{matrix} (\text{SO}_2)'' \\ (\text{PO})'' \end{matrix}$ O sulphuryloxi-oxide.

Phosphoric anhy- $\begin{matrix} (\text{PO})'' \\ (\text{PO})'' \end{matrix}$ O₂ diphosphoryltri-oxide.

dride $\begin{matrix} (\text{PO})'' \\ (\text{PO})'' \end{matrix}$ O₂ diphosphoryltri-oxide.

Acetic acid $\begin{matrix} \text{H} \\ \text{C}_2\text{H}_3\text{O} \end{matrix}$ O hydroacetyl-oxide.

Acetic anhydride $\begin{matrix} \text{C}_2\text{H}_3\text{O} \\ \text{C}_2\text{H}_3\text{O} \end{matrix}$ O diacetyl-oxide.

Succinic acid $\begin{matrix} \text{H}_2 \\ (\text{C}_4\text{H}_4\text{O}_2)'' \end{matrix}$ O₂ dihydrosuccinyldi-oxide.

Succinic anhydride $\begin{matrix} (\text{C}_4\text{H}_4\text{O}_2)'' \\ (\text{C}_4\text{H}_4\text{O}_2)'' \end{matrix}$ O₂ succinyl-oxide.

Citric acid $\begin{matrix} \text{H}_2 \\ (\text{C}_6\text{H}_6\text{O}_3)''' \end{matrix}$ O₂ trihydrocitryltri-oxide.

Citric anhydride $\begin{matrix} (\text{C}_6\text{H}_6\text{O}_3)''' \\ (\text{C}_6\text{H}_6\text{O}_3)''' \end{matrix}$ O₂ dicitryltri-oxide.

The sulphur acids and salts of course follow the same rules,

Sulphocarbonic acid $\begin{matrix} \text{H}_2 \\ (\text{CS})'' \end{matrix}$ S₂ dihydrosulphocarbonyldisulphide.

Sulphocarbonic anhydride $(\text{CS})''$ S sulphocarbonylsulphide.

The nomenclature of the salts is obvious.

Acetate of silver $\begin{matrix} \text{Ag} \\ \text{C}_2\text{H}_3\text{O} \end{matrix}$ O argentoacetyl-oxide.

Primary sulphate of potassium $\begin{matrix} \text{K} \\ (\text{SO}_2)'' \end{matrix}$ O₂ potassiohydrosulphuryldioxide.

We have now only to mention the last type, H_2C . There are only few derivatives of this type, but names must be in readiness for these compounds when discovered. The termination *uret* may be applicable in this case.

Marsh gas H_2C tetrahydrocarburet.
Siliciuretted hydrogen H_4Si tetrahydrodisilicuret.

I know that there are very many weak points in the system that I have taken the liberty of describing, and that it will be very much ridiculed. One of the greatest difficulties is to find appropriate names for the different radicals. I have tried once or twice to concoct some terms, but they became so barbarous that I abandoned the attempt. However, we have the names of the radicals of the fatty alcohols, if we like to employ them,—I mean the nomenclature of these bodies used by Gerhardt,—viz.: trityl, tetryl, &c.; we might have monyl, or elayl, and deutyl, but they would not replace the names methyl and ethyl, for some time at least. There is, unfortunately, too little, and yet too much, distinction between the alcohol and acid radicals; too little in the fact that they all end in *yl*, and too much, as we do not see the relation between the radicals of the same series. We see no relation between ethyl and acetyl by the name; and if we adopt Professor Williamson's suggestion, and call acetyl ethyl, then we must have omethyl, opropyl, &c. Perhaps, if the termination *oyl* were employed for the name of the acid radical, it might have some benefit; however, in this case, the similarity might be too great: thus—methyl methoyl, ethyl ethoyl, bear a great resemblance to one another. There would be a confusion in the case of caproyl and eapryl, but that would not be new.

In conclusion, I have only to apologise for having written at such length; but if you should think these remarks worth admission to your Journal, they might be the means of directing others who are more competent for the task than I am to propose some systematic nomenclature which would be of lasting benefit to our science.—I am, &c. HERBERT McLEOD.

London, December 4.

To the Editor of the CHEMICAL NEWS.

SIR,—Will Mr. Newlands be kind enough to say what will be the name for albumen according to his proposed new system of nomenclature?—I am, &c.

INQUIRER.

Bath for High Temperatures.

To the Editor of the CHEMICAL NEWS.

SIR,—Some time ago, having occasion to use a bath capable of affording a temperature considerably above that of boiling water, my friend Mr. Tichborne suggested to me the use of glycerine instead of the ordinary saline baths. I tried a mixture of glycerine and water and found it to answer so well the purpose for which it was intended, that I thought it would be worth while to make known a few particulars which might prove useful to chemists.

I found the boiling-point of a mixture of six parts water with one part glycerine (by measure) to be about 218° F.; with equal parts glycerine and water, 230°; and by using six parts glycerine with two of water, the temperature was raised to, and remained steady at 250°. If the proportion of glycerine be much increased beyond

this point, acrid fumes are given off on heating the mixture.

The glycerine which I used in these experiments, though otherwise pure, had frequently been accidentally exposed to the air, and had doubtless, absorbed some moisture from the atmosphere; therefore, the results given above may be looked upon as correct for the mixtures experimented upon, though perhaps they may not hold good when other samples of glycerine are used.

I am, &c. EMERSON REYNOLDS.

Estimating the Value of Aniline.

To the Editor of the CHEMICAL NEWS.

SIR,—Can any of your Correspondents inform me of any quick and sure method of estimating the value of aniline, and the amount of any one of its homologues present in a mixture of them? Precipitation of the platinum salt and calcination does not give all that is required; and to form a certain quantity of colour and test the amount produced is only approximative, and by rectifying at boiling points many errors may creep in.—I am, &c.

BENZOL.

On the Adulteration of Butter.

To the Editor of the CHEMICAL NEWS.

SIR,—Before proceeding to the detail of another test for the presence of fats in butter, I feel that the note of Mr. Horsley in the last Number of the CHEMICAL NEWS calls for a brief remark. So far from wishing to "condemn" the utility of the ether test, I think that we are all indebted to Mr. Horsley for publishing it; and I am willing to acknowledge and announce that he has been the first to make public any test at all by which this adulteration is to be discovered. I agree with him that ether does detect lard when mixed with butter in small proportions very readily and satisfactorily, but not exactly when used in the manner he details. I think, as he describes its use, the appearance of the deposit may have misled him, and induced him to think that lard is the common adulterant. It may so happen, too, that the "fresh" butter I have examined in London may really be adulterated with a different substance to the butter which he has analysed in Cheltenham.

Again, I think Mr. Horsley has misunderstood me when he says that the ether test can readily identify other kinds of fat than lard, while those I have put forward will not detect any but beef dripping. I don't think I stated this. On the contrary, the peculiarities of both mutton fat or dripping—for I have used the terms "fat" and "dripping" synonymously—and of lard come out as remarkably as those of beef fat on melting with boiling water in a beaker, &c., as I propose showing in my next paper. I do not desire to disparage the use of ether, but still it appears to me that a service is rendered if it is shown that what everybody has at hand suffices to detect these substances in butter. I am sure Mr. Horsley is equally desirous with myself to arrive at the truth; and with this view, as I shall always be pleased with any criticism that he may bestow upon my observations, I hope, in all friendliness, he will not object to my criticising his.

I am, &c. EDWARD BALLARD, M.D.

Parochial Laboratory, Islington, December 9.

MISCELLANEOUS.

Sulphur in California.—The refining of sulphur has been commenced as a business in Santa Barbara county, Cal. Twenty miles south-eastward of the town of Santa Barbara, and seven miles back from the Mission of San Buenaventura, which is upon the sea shore, is a great bed of native sulphur, deposited in remote ages by the vapors and waters of sulphur springs. The country in the vicinity bears strong marks of volcanic action. The sulphur deposits back of San Buenaventura have long

been known, but only lately has it been rendered valuable. Messrs. Davidson, Spence & Co. commenced about the first of this year to open the mine. The *Daily Alta* says, that there were then some half a dozen men at work in the mine, and this sulphur is so abundant and accessible, that the time is perhaps not far distant when it will be shipped to Europe. The crude deposit is stated to comprise 80 per cent. of sulphur.—*Scientific American*.

Chemical Society.—The next meeting of this Society will take place on Thursday evening next at Burlington House, at eight p.m., when the following papers will be read:—Dr. Bence Jones, "On the Simultaneous Variations of Hippuric and Uric Acids in Healthy Human Urine;" Mr. G. F. Rodwell, "On the Solubility of Sulphate of Lead in Hydrochloric and Nitric Acids."

Taking Notes of Lectures.—There is one cause especially which I believe has tended to depreciate the value of lectures as a means of instruction. It is the common practice which students fall into, of taking very copious notes whilst in attendance upon them. This is a great mistake, for if a student during a lecture have his eye constantly directed upon the paper on which he is simply recording the words of the lecturer, instead of directing his principal attention to the lecturer himself and the illustrations which he is employing, he necessarily loses in a great measure the effect which must be otherwise produced by the words as they flow from the earnest and high-minded teacher; and at the same time he will gain but an imperfect acquaintance with the details and mode in which experiments have been performed, or of the nature of the specimens exhibited for the elucidation and demonstration of the subject-matter of the discourse; and hence the advantages he would otherwise derive from attendance upon lectures over those of simple reading, are, in a great measure, lost. My advice to a student, therefore, is, to take notes only of the "heads" of the lecture, or at least, not to allow note-taking to interfere with his attention to the lecturer, and afterwards whilst the lecturer with his apparatus and specimens are photographed as it were in his memory, to fill in the details by reading and thinking over what he has heard.—*Professor Bentley*.

ANSWERS TO CORRESPONDENTS.

C. G.—It has not yet been mentioned in our pages. We will bear it in mind.

Treatment of Tar.—W. writes, Where can I get good information as to the treatment of tar, and refuse from gas works, the method of making naphtha, paraffin, and dyes from tar, &c.?

Ignoramus.—In inhaling air containing carbonic acid, a large quantity is taken into the lungs, where it comes in contact with the blood. But in drinking aerated beverages, a small quantity only is taken into the stomach. The action of an agent in one instance has nothing to do with its effect in the other case.

Browning for Gun Barrels.—Armourer.—Take tincture of perchloride of iron $\frac{1}{2}$ oz., spirit of nitric ether $\frac{1}{2}$ oz., sulphate of copper 2 scruples, distilled or rain water $\frac{1}{2}$ pint. Clean the barrel with lime and water, and apply with a sponge. When dry, polish with a stiff brush or iron scratch brush.

G. C. C.—1. Sulphite of soda is made by passing sulphurous acid into caustic soda; sulphide of sodium, by passing sulphuretted hydrogen into the same alkali. Which do you mean? 2. Your question would require a long explanation in answer; read some elementary work on Physics and Meteorology.

Filtering-Paper.—Our opinion has been asked respecting a kind of filtering-paper, supplied by Wilcox and Co., advertised as "chemically pure, and the cheapest ever made." It is of a whity brown colour and upon burning leaves a reddish brown ash. Four sheets $7\frac{1}{2}$ inches in diameter were found to yield, upon combustion, 0.694 grains of ash. This is much less than that yielded by ordinary white filtering paper, at even a higher price, although it is nearly double that left by the best Swedish paper. The paper, when in use, is very strong, and filters quickly, without, at the same time, allowing any of the precipitate to pass through. Taking into consideration the low price at which it is offered, it will be found a very valuable article for general qualitative work, although it is not to be recommended for delicate or quantitative experiments.

Books Received.—"Elementary Treatise on Physics." By Professor A. Ganot. English Edition. Part III. London: H. Baillière. "A Set of Chemical Tables," on six large sheets, arranged by Dr. Galloway. London: John Churchill.

THE CHEMICAL NEWS.

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SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Chemical Researches on the Assay of Silver by the Moist Way, by G. J. MULDER, *Math. Mag. Phil. Nat. Medicinæ, Artis Obstetriciæ et Pharmaceuticæ Doctor, Professor of Chemistry at the University of Utrecht, Grand Cross of the Order of the Netherlands Lion; of the Luxembourg Order of the Couronne de Chêne; of the Swedish Orders of Wasa and Polar Star, &c., &c.*

(Continued from page 298.)

It is well known that the temperature exerts in most cases a very great influence on the solubility of substances in liquids; the solubility is hereby often increased. What, now, is the influence of the temperature of the liquid submitted to the assay on the quantity of chloride of silver which remains soluble in the nitrate of soda? The answer to this question is of great importance, as much for the assay experiments as for scientific interest.

I took a solution of one gramme of pure silver, which was first treated at 18° C. with 100 cubic centimètres of salt standard solution, and was further brought to the limit of precipitation with decimal solution of salt, and then treated again with decimal silver solution, and again with decimal solution of salt, the temperature remaining at 18° C. The bottle which contained the liquid was now heated to about 30°, and at that temperature the decimal solution of silver was added unto the limits of precipitability. The temperature was then raised to from 45° to 55°, and at this it was kept or brought to it again previous—that, after strong shaking, a fresh addition of silver or salt solution was added. Finally, it was cooled down to 25°.

The results of these experiments are sufficiently significant to be quoted in full. The drops have been added two by two every time until a perceptible precipitate ceased to make its appearance:—

	Decimal solutions.	
At temperature of 18° C.	22 drops of solution of silver	
" " 18° C.	22 " " salt	
" " 30° C. about	35 " " silver	
From 45° to 55° C.	60 " " salt	
" 45° to 55° C.	60 " " silver	
" 45° to 55° C.	60 " " salt	
At temperature of 25° C. about	47 " " silver	
" " 25° C. about	29 " " salt	

The precipitation has been effected with the greatest care, and has never been carried over the limits; but the influence of the rise and fall of the temperature has occasioned a certain anomaly which was quite unavoidable. When we take the sum of the drops of silver and salt, we have:—

¹ It deserves to be noticed that at this temperature the precipitate as soon as formed gets immediately dissolved, as long, at least, as 40 drops have not been fully added; and it requires a very experienced eye to distinguish the precipitate properly.

Silver.	Salt.
164 drops, or 8.2 a.c.	171 drops, or 8.6 a.c.

This agrees so closely that we may declare the result a good one. The experiment remained the same at the end as in the beginning, and if it had been carried again up to 18° C. the required quantity of 22 drops would have made its appearance again; but the risk of discolouration of the chloride of silver prevented the longer continuation of the experiment. The main point is this, that at a temperature of from 45° to 55° the quantity of chloride of silver dissolved in 0.787 grammes of nitrate of soda and 100 cubic centimètres of water is almost three times as large as at 18° C. We have already learned that the silver which remains this way in solution when the assay is made with 1 gramme of pure silver and 100 cubic centimètres of standard solution, weighs 1 milligramme, the temperature being at from 15° to 17°. Without anything else being changed, the increase of temperature (45°—55° C.) increases the weight of silver dissolved three times—i.e., 3 milligrammes remain in solution. A decrease of temperature has just the opposite effect, as the following experiment will show:—

One gramme of pure silver was first dissolved in 5 cubic centimètres of nitric acid, then precipitated at a temperature of from 15° to 17° C. with 100 cubic centimètres of standard salt solution, and afterwards cooled to 5° C., and treated first with decimal solution of salt and *vice versa* with silver and salt (decimal solutions), the mutual precipitate was produced, the temperature being kept at 5° C. The results were the following:—

Silver.	Salt.
A 13 drops	B 13 drops
C 13 drops	D 13 drops

At a temperature of 5° C. 13 drops only of the solutions of silver and salt were required to produce the mutual precipitate unto the ultimate limit: 13 drops = 0.65 milligrammes of silver, or its equivalent proportion of salt. We have, therefore, the following series: 0.787 grammes of nitrate of soda and 100 cubic centimètres of water dissolve—

	Chloride of silver or silver.	
	Weight in milligrammes.	
	Cl. Ag.	Ag.
At 5° C.	0.86	0.65
At 15°—17° C.	1.33	1.0
At 45°—55° C.	3.99	3.0

Two other points deserve our attention: viz., the influence of and excess of free nitric acid, and the influence of nitrate of copper on the origin of the mutual precipitate. The former—i.e., excess of free nitric acid—occurs in the ordinary assay of pure silver, while the former again with the latter are met with in the assays of alloys of silver and copper. The quantity of nitric acid and its strength, degree of concentration, are not very strictly taken when an assay is made, while the quantity of copper in the alloys is subject to much variation, and the quantity of nitrate of copper must consequently vary also; and if these two agents, nitric acid and

nitrate of copper, have any influence, then it is necessary to ascertain the quantity and quality of that influence. The following experiments were made:—

a. 1 gramme of pure silver was dissolved in 5 cubic centimetres of nitric acid.

b. 1 gramme of pure silver was dissolved in 10 cubic centimetres of nitric acid.

c. 1 gramme of pure silver and 1 gramme of pure copper were dissolved in 7.5 cubic centimetres of nitric acid.

These solutions were precipitated with 100 cubic centimetres of standard salt solution, and were afterwards treated with decimal solution of salt, so as to bring them to the utmost limit; temperature—16.5° C.

We have further determined the quantity of silver required in order to reduce the precipitate produced by decimal solution of silver to the ultimate limit. The following quantities of decimal solution of silver were required at 16.5° C.:—

For a	20 drops
For b	22 drops
For c	20 drops

This result agrees so closely with that we have obtained for pure silver (see page 204 Vol. IV. No. 98) that we have every right to conclude that, provided the temperature of the vessels in which the assay liquids under experiment are kept remain constant, much nitric acid or much copper exert no influence upon the origin of the mutual precipitate. It is true the experiment was not carried on so as to precipitate again with decimal solution of salt and again with decimal solution of silver; but this was not necessary, for what has been once well observed remains constantly the same under the same circumstances. We have already learned that chloride of silver is more or less soluble in nitric acid, indifferently, whether it be more or less concentrated. Chloride of silver is also soluble in nitrate of silver, but it is so sparingly so that it is of no influence on the silver assays. From what has been treated here we may draw the following conclusions:—

That there are sources of errors in the method of assay by the wet way, even if pure silver be used, which errors depend, first, on the quantity of nitrate of soda; secondly, on the quantity of water; and, thirdly, on the temperature.

It is necessary, and I found it to have a great practical influence, to inquire more accurately into some of these particulars. The quantity of water, it is true, may differ about from two to three cubic centimetres. That difference has not been proved to me to exert any very decided influence upon the assays and experiments connected therewith; but the quantity of nitrate of soda may differ, if care be not taken to employ, when experimenting upon alloys, not from 1000 milligrammes of the alloy, but from 1000 milligrammes of pure silver present in the alloy, in order to secure always the presence of 0.787 gramme of nitrate of soda in the liquid. For instance, let the alloy to be tested be at $\frac{11.1}{1000}$ ths, it is then necessary to weigh according to this equation, $775 : 1000 = 1000 : x$, which gives $x = 1.2903$ in that quantity 1000 of silver occurs, and 1000 of silver will always yield exactly the constant quantity of nitrate of soda.

It will be said, that this is done merely because the pipette usually employed is filled with 100 cubic centimetres of the standard solution. I say it must be done,

* This expression *utmost limit* is a short expression for the one we so frequently require to use—viz., *utmost or ultimate limit of precipitability*.

not because the same pipette is used, but because every other quantity of nitrate of soda in the liquid gives rise to different results, if, at least, the experiment be carried so far as to precipitate with salt unto the limit of precipitability. It fortunately so happens that not many substances do exert such a disturbing influence as nitrate of soda exerts in these experiments. The following experiments may elucidate this point:—I made an assay experiment to which I will have to refer afterwards again, in which, with one gramme of pure silver, 0.5 gramme of sulphate of protoxide of manganese, 0.4 gramme of cadmium, 0.1 gramme of iron, and 0.5 gramme of zinc—the three latter in a metallic state, were dissolved in nitric acid at the end of the experiment—I applied decimal silver solution unto the limit, the temperature being 16° C. I repeated the experiment with 1 gramme of pure silver and 0.6 gramme of sulphate of nickel, also with 1 gramme of pure silver and 0.1 gramme of oxalate of cobalt. The nitrates of these metals had no perceptible influence on the quantity of silver required, after the decimal solution of salt had been used to precipitate to the limit. To come again to the limit of precipitability these quantities were the following:—

Pure silver	20 drops
Ditto	20 "
Manganese, iron, cadmium, zinc	20 "
Nickel	20 "
Cobalt	20 "

No trace even of influence of the presence of the nitrates of all these metals can be observed to act upon the solubility of the chloride of silver; and though we do not mean to deny that they (*i. e.*, these nitrates) may exert some influence, it is beyond the limits of our experimental observation and of no consequence to the assays.

It is different with the nitrates of the alkalies and alkaline earths, which we will treat of afterwards, when we discuss the equivalent weights of silver and common salt.

(To be continued.)

On the Adulteration of Butter with Animal Fats,
by EDWARD BALLARD, M.D. Lond., M.R.C.P.,
Medical Officer of Health for Islington, and Parochial
Analyst under the Adulteration of Food Act.

[SECOND COMMUNICATION.]

In the short paper which I took the liberty to send a few weeks ago for insertion in the CHEMICAL NEWS, I ventured to enumerate certain physical indications of the existence of fatty adulterations in butter. I do not pretend to assert that all of them are of equal value. The colour, for example, of true butter will vary with season, with the nature of the feed of the cows, and probably, also, with their state of health. And the pressure-test also may vary with a variety of circumstances; but of those I mentioned, there are three which are, I believe, always to be relied upon. These are, the size of the drops which rise when the butter is melted and well stirred circularly with boiling water, and the consequent fineness or coarseness of the resulting appearances of cellulation; the granulation or sago-like appearance of the butter when the cold cake is removed in the adulterated butter, the globules sometimes being as large or larger than the largest grains of sago; and, lastly, the taste of the dried butter. The latter, I believe, is especially to be relied upon. It must be a very small adulteration indeed that will not now be detected by the palate; and these indications I have recapitulated are

not confined to [adulterations with beef fat or dripping, as I have been misunderstood to assert. The same phenomena are observed when mutton-fat or lard are the adulterants; and in these cases not only is there a similar appearance in the melted layer and in the granulations, but the taste detects severally mutton fat, from its more tallowy flavour, especially if it have been melted down for some time, and lard from the very distinctive impression it makes upon the palate. I do not think any mistake can occur. I mentioned as an instance the adulteration with an equal weight of beef dripping; but a fourth part of fat, beef, mutton, or lard, gives similar indications, and very markedly too,—quite unmistakably.

As to the question whether lard or beef or mutton fat is the most common adulterant. There are parties in London whose sole business it is to deal with the butchers and others in fat. Among these I have had inquiries instituted. It appears that the destination of it is as follows:—The persons to whom it is sold first of all very carefully pick out every bit of pork fat, however small, and it is purchased by pastrycooks and chemists, both of whom find it to their advantage to prepare their own lard. The beef and mutton fat go to the melters, who select the suitable and best bits, which they boil down and pack in casks for parties at Ostend and other places, for the undisguised purpose of mixing with butter. There is no secret in this among persons engaged in the trade. The rest is prepared for the soap-boilers and others. I am further informed that for the preparation of butter beef fat is preferred. A few years ago there was, however, such a scarcity of fat, at Ostend that mutton fat, &c., were taken indiscriminately.

And now for the ether test, as put forward by Mr. Horsley. Since the publication of his letter I have made some experiments respecting it, the result of which I may as well detail before proceeding further. Mr. Horsley's statements are, that, first, pure butter ought entirely to dissolve when covered with ether at 65°; second, that the purest lard is more or less insoluble under similar circumstances, and that the same may be said of dripping, mutton suet, tallow, &c.; and, third, he distinguishes these several substances by the appearance of the precipitates, stating that that from lard is very fine and smooth, whilst that from dripping is granular and crystalline, and that from tallow long and thread-like, laying piled up one above the other. In my own experiments I used an apparatus which enabled me to weigh the precipitate, as well as to observe its character during the solution; and I used a constant quantity of the butter or fat to be tested, and a definite quantity of ether. In this way I hoped to attain more precise results. The difficulty I had to contend against was that of collecting and weighing the deposit without risking the loss of ether by evaporation, and the deposition of some of the dissolved-out matters by lowered temperature during the operation. The expedient I hit upon was this:—I take an ordinary test-tube, about 4½ inches long and about one inch wide, and fit it as accurately as possible with a good cork. I then take a piece of thin glass tube about ⅔ths or ½ an inch diameter, and from 1½ to 2 inches long—pieces of broken test-tubes do very well—and turn over the edges, so as to form a lip at each end. Over one end I tie with thread, tightly stretched, a little bit of thin linen rag, and round the tube beneath the other lip another piece of thread, leaving one end long. This, with a water-bath, constitutes my apparatus. In each experiment I used 20 grains of butter or fat to be tested, which I weigh in the cylinder,

and then let the latter down into the test-tube by the thread. I pour in the ether, directing it into the cylinder, and the thread hanging out of the tube. I put in the cork firmly and tie over with a leather cap. The tube is then immersed in the water-bath, at 65°, and at the expiration of an hour—this being the time I have mostly allowed for solution—the cap is removed, the cylinder drawn up by the thread without the necessity of removing the cork, the cap put on again, and the cylinder left for the ether to drain off from the deposit, the temperature of the bath still being maintained. As soon as the ether appears to have drained away, I remove the cylinder from the tube, and, closing the open end with my finger, press the other end upon a pad of blotting-paper, so as to free the linen and deposit by its imbibition from all remaining ether and soluble matter. When no further stain is imparted to the blotting-paper, I hang the cylinder in the air to dry; and when all smell of ether has passed off, weigh the cylinder and deposit, deducting the weight of the former.

In order to be sure about the matter, I obtained some butter churned specially for me by a friend in Bedfordshire, and with this, which arrived fresh and unsalted, I proceeded to experiment, and I adulterated it as follows:—1, I prepared an adulteration with equal weights of this and strained beef dripping; 2, one with three parts of butter and one of the dripping; 3, one with three parts of butter and one of strained mutton dripping; 4, one of equal parts of butter and pure, home-made, unsalted lard; 5, one of three parts of butter and one part of the lard. I also subjected to experiment separately the lard and beef dripping. My results were as follows:—

The butter and all the mixtures were washed with boiling water, and the cold cake thoroughly dried.

1. Results of exposure of 20 grains for one hour in the cylinder with one fluid drachm of ether.

- | | |
|---|--|
| a. Pure butter | Residue 2·4 grains—Residue firm, coherent, smooth, forming a little cupped mass of form represented in the margin; not absolutely white, not smelling of tallow or lard. |
| b. Beef dripping | Residue 12·6 grains—A rough looking mass, smelling strongly of tallow. |
| c. Lard | Residue 12·0 grains—Smooth, very white, smelling of lard. |
| d. Butter containing an equal weight of beef dripping | Residue 9·0 grains—A rough looking mass, smelling of tallow. |
| e. Butter containing one-fourth of beef dripping | Residue 4·0 grains—Resembling in shape and appearance that from butter, but whiter, and smelling somewhat of tallow. |
| f. Butter containing one-fourth of mutton dripping | Residue 4·0 grains—Resembling the preceding. |
| g. Butter containing one-fourth of lard | Residue 2·0 grains—Residue in shape, when dried, flatter than that from butter or that adulterated with one-fourth of dripping; less smooth on surface, less coherent, and fissured as in margin, whiter also, more brittle, and not smelling of tallow. |

I prepared another sample of this adulteration, and in place of allowing the cake to cool undisturbed, stirred it from time to time, so as to incorporate a little water with it, which is then not separable entirely either by blotting paper or by the desiccator. This powerful physical retention of water in interspaces of the fat is a distinguishing character of lard, and it is imparted to a marked extent to the butter adulterated with it. A specimen thus prepared and dried, as far as it could be, we will mark γ .

- γ . Butter with one-fourth lard } Residue 1.8 grains—Forming a flat cake upon the linen covering it universally, and fissuring or drying in all directions. Through this deposit the ether drained off very slowly.



2. Results of exposure of 20 grains for one hour in the cylinder with two fluid drachms of ether.

- a. Pure butter } Residue 1.8 grains—Of same appearance and character as in experiment with 3j., but the mass is thinner. Before separation the residue was suspended in the ether in the form of a coherent flocculence, entangling a few air bubbles. The ether drained away rapidly.

- b. Butter containing one-fourth part of beef dripping } Residue 1.1 grains—Residue much like that of pure butter. The precipitate before separation was powdery-looking, not so fine as that from lard. Ether drained away quickly.

- c. Butter containing one-fourth of lard } Residue 1.1 grains—Residue resembling that from 3j. of ether, but whiter. Before draining it formed a close flocculence in the ether. Ether drained away quickly.

- γ . Butter with one-fourth lard } Residue 0.9 grains—Residue very white, but resembling that from 3j. ether. Before draining it formed a dense, very firm, smooth deposit on linen, and the ether drained away extremely slowly.

A sample of δ , prepared in a similar manner to γ , also formed a dense deposit on the linen, but the ether drained away more quickly, and the deposit when dry was of the ordinary character, and weighed 1 grain.

The conclusions, I think, which may fairly be drawn are as follows:—

1. A certain portion of butter is insoluble at 65° in ether, used in the moderate quantities which are fitted for the proposed analysis. Perhaps other samples of butter would give even a larger per-centage of residue.

2. The appearance of the deposit as it lies in the ether (3j.) assists the diagnosis.

3. So also does the slowness with which the ether drains away from that formed from butter adulterated with lard and prepared as γ .

4. The form and appearance of the dried residue as it lies in the cylinder distinguishes butter adulterated with lard on the one hand from pure butter or butter adulterated with beef or mutton fat on the other.

5. The tallowy odour of the residue from butter adulterated with beef or mutton fat distinguishes it from that afforded by pure butter or lard.

6. Not quite half the weight of beef dripping and lard is taken up in the course of an hour by 3j. of ether at 65° . A longer exposure of lard (for several hours) caused all but six grains to be taken up. In each instance much less is taken up than is taken up from butter.

7. The presence of an excess of butter increases the solubility of the fat or lard mixed with it. The residue from the adulteration with one-fourth of dripping (beef or mutton) was greater than that from butter whether 3j. or 3ij. of ether were used; but the residue was less than it should have been (by calculation) for the quantity of fat present. In the case of lard, the solubility was so much increased by the butter, that the residue, with 3j. of ether, was absolutely less than that from pure butter, and the colour was so much whiter that one must believe that the presence of the lard promoted the solution of the butter also.

8. When the butter was not in excess, the residue from that adulterated with beef dripping was remarkably large, exceeding that which might be inferred from calculation. I obtained nine grains from the adulteration with an equal proportion of beef fat in two consecutive analyses.

9. So far as the comparative quantity of residue is concerned, nothing is gained from the use of more than one fluid drachm of ether to twenty grains of the butter to be tested. In fact, where 3ij. were used, the residue from the butter adulterated with a fourth of lard or of dripping was actually less than that from pure butter.

Parochial Laboratory, Islington, December 12.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 309.)

Synthesis of Sulphate of Lead.—In nearly all determinations of lead, chemists bring the metal to the state of sulphate, in order to weigh it. After having performed so large a number of delicate analyses, I do not hesitate to declare that there does not exist any which present so many difficulties and uncertainties, even when surrounded with the most minute and fastidious precautions. Berzelius has already remarked the extreme difficulty which is experienced in bringing to a satisfactory conclusion a synthesis of sulphate of lead.

I first attempted to perform these determinations in a refractory glass, but in this manner I obtained the most contradictory results. When it is attempted to heat the sulphate of lead the glass is attacked even before the excess of sulphuric acid is entirely driven off. It is, therefore, indispensable to perform the operation in platinum vessels. These are the arrangements which I eventually adopted:—

So as to save the labour—more considerable than would be supposed—of preparing the quantities of lead necessary to determinations of this kind, I combined some of the syntheses of nitrate of lead with those of the sulphate of this metal. Thus, the nitrate obtained in the synthesis No. IV. was transformed into sulphate. No. I. in the following Table shows the result. The nitrates produced in the experiments Nos. V., VII., VIII., IX., and X. were likewise converted into sulphates, the results being successively shown in Nos. II., III., IV., V., and VI. I effected this transformation in the fol-

lowing manner:—The nitrate of lead derived from a known weight of lead was dissolved in the smallest possible quantity of pure water, and the solution was poured into the weighed cucurbit of a platinum retort, containing an amount of pure dilute sulphuric acid, a little more than sufficient to transform all the lead into sulphate. After a convenient repose the acid liquid, which had become quite clear, was removed with a pipette and introduced into a very large platinum dish, which had been weighed, together with the platinum retort, and immediately submitted to evaporation over a water-bath, protecting as much as possible the surface from contamination from suspended matters in the air. The glass globe which contained the nitrate was repeatedly washed in pure water. All the washing-waters were added to the cucurbit already containing the sulphate of lead.

A small quantity of sulphuric acid was then poured into the liquid in the cucurbit to be certain that there was no more lead in the state of nitrate. In proportion as the liquid in the dish evaporated I replaced it by a fresh quantity taken from the cucurbit. In order to remove as much as possible the excess of sulphuric acid and all fixed soluble matter, I washed the sulphate two separate times in pure water. All the washing-waters, perfectly cleared by standing, were added to the liquid in the dish and evaporated in their turn. Then I exposed the cucurbit to a steam heat to dry the sulphate of lead. When, under the influence of a temperature of 100°, it disengaged no more vapour whatever, I embedded it deeply in a bath of magnesia, and raised it gradually to dull redness. After the volatilisation of traces of sulphuric acid, I saw appear in all my experiments nitrous vapours. As these vapours might be derived from the presence of some traces of nitrate of lead carried down by the sulphate in each experiment and after the vessel was cool, I moistened the salt with water very slightly acidulated with pure sulphuric acid. I again dried the mass over the water-bath, and calcined the sulphate a second time by heating the vessel in a magnesia bath. I never, in this second calcination, saw red vapours appear.

In all my experiments the sulphate of lead was of a dazzling whiteness. Its desiccation was effected with such care that the side of the cucurbit did not show any trace of projection.

To preserve the sulphate of lead during its cooling from atmospheric moisture, I suspended in the interior of the cucurbit a vessel containing chloride of calcium. This vessel was attached to a glass rod passing through the neck of the moveable head. The opening of the neck was besides closed with a cork.

I now return to the washing waters of the sulphate of lead. The platinum dish which contained them was kept on the water bath as long as its contents disengaged vapours. After all the liquid capable of volatilising at 100° was dissipated, I covered the platinum dish with a porcelain capsule larger than it, and having a hole bored in its upper part. Through the hole in the capsule I passed a glass tube nearly down to the surface of the liquid residue contained in the platinum dish. I then disengaged pure ammonia to transform the sulphuric and nitric acid which the dish contained into sulphate and nitrate. When the mass was almost solidified, I placed the dish in a bath of magnesia and heated it very gently to redness. Towards 300° the sulphate of ammonia decomposes into volatile products, and the capsule contains absolutely nothing but the sulphate of lead mixed with the traces of foreign matters existing

in the lead or the sulphuric acid. To eliminate the soluble sulphates which the sulphate of lead in the capsule could contain, I always wash the salt in water very slightly acidulated with sulphuric acid. After the washing and drying, I calcined a second time the small quantity of sulphate thus obtained.

In the first two determinations which I made, I had the misfortune to throw away the washing waters. I am therefore ignorant whether they contained any fixed product. In the four following I evaporated them down in a weighed platinum dish. The residue which I heated to redness weighed:—

In No. III. . .	0.0140	grammes, consisting of alkaline sulphates with traces of iron and copper.
In No. IV. . .	0.0193	do.
In No. V. . .	0.0190	do.
In No. VI. . .	0.0205	do.

These alkaline sulphates were evidently derived from the potassium and the sodium contained in the lead employed. I ought to add that the little sulphate of lead obtained from the evaporation of the washing waters was not absolutely white, even after having been a second time calcined with a mixture of sulphuric and nitric acid. It had a grey tinge.

After cooling for five hours free from moist air, the retort and the platinum dish were both weighed together in the large balance.

Before giving the result of these laborious experiments, I ought to point out a cause of error which I overlooked. I weighed the sulphate of lead in air; but this salt can condense so large a quantity of air as to be sensible with the enormous masses experimented upon. There is a means of ascertaining this fact and correcting the result subsequently if there is really an error of this kind, which would not surprise me.

I have abstained from correcting the weight of the sulphate of lead produced from 100 parts of lead by the weight of the foreign metals obtained in the state of sulphate in the residue of the washing waters of the lead salt, because I consider this correction as being more or less arbitrary, ignorant as I am of the proportion between the potassium and the sodium in these alkaline sulphates. Moreover, the presence in the lead of these excessively minute traces of foreign metals diminishes the weight of the sulphate of lead. The result would therefore be higher if the metal employed had been absolutely pure, and that would remove me still further from the figure, calculated according to Prout's law.

Syntheses of Sulphate of Lead.

Numerical order.	Weight of the Lead in air.	Weight of the Lead reduced to a vacuum.	Weight of the Sulphate of Lead in air.	Weight of the Sulphate of Lead reduced to a vacuum.	Weight of the Sulphate of Lead produced from 100 parts of Lead.
I.	141.999	141.9925	207.9295	207.9388	146.443
II.	148.623	148.616	217.6045	217.6141	146.427
III.	100.007	100.000	146.4125	146.419	146.419
IV.	200.014	200.000	292.850	292.864	146.432
V.	250.0175	250.000	366.0365	366.0525	146.421
VI.	250.0175	250.000	366.041	366.0575	146.423
				Mean ..	146.4275

(To be continued.)

On Alum in Bread and Flour, by JOHN HORSLEY, F.C.S. Lond., Analyst to the County of Gloucester.

THE question of alum in bread having often proved a knotty point, and many errors having been made by mistaking other earthy precipitates for alumina, because a white precipitate was obtained on the addition of ammonia to the water in which bread had been soaked, as well as a similar precipitate by the use of either nitrate of baryta, or chloride of barium, the latter being supposed to have reference to the sulphuric acid of the alum salt, when, in fact, it may be due to the sulphate of lime existing in the water which was incorporated with the flour in making the loaf, or to the hard water in which the bread was soaked, I have drawn up the following paper, which contains *in extenso* the precise mode of manipulation which I am accustomed to follow, and which is of a thoroughly practical and reliable character.

The detection of alum in bread or flour consists of three points—viz., the existence of sulphuric acid, potassa, and alumina. If the two former are found, the latter may also be inferred.

Neither sulphuric acid nor alumina are to be found in incinerated wheat corn or flour, and a minute quantity only of potash obtains in it, and that in the form of a phosphate. An abnormal quantity, therefore, of that alkali, conjoined with sulphuric acid, being found in the water on making a preliminary trial by steeping a portion of the suspected bread in it, forms a satisfactory guide to further operations.

In order to get at these ingredients, we must first burn a thousand or more grains of bread or flour to a cinder in an iron dish or earthenware crucible, powder it finely, and burn it afresh till it be reduced to a small bulk—an operation of several hours; boil this in distilled water for a short time, and decant on to a filter, boil the residue again and again with fresh portions of distilled water till it ceases to produce any reaction on turmeric paper, indicating the entire extraction of the phosphate and sulphate of potash, the amount of liquid required being about 12 or 14 ounces.

By this treatment you will have extracted not only all the sulphuric acid and the potash—two of the aforesaid elements of baker's alum—but whatever common salt had been used. By concentration, if necessary, of this liquor, freely acidifying it with nitric acid, and then treating it with nitrate of baryta solution, the sulphuric may be precipitated and the sulphate of potash converted into nitrate of potash. The chlorine likewise may be separated from the common salt by nitrate of silver forming nitrate of soda and chloride of silver. The liquor filtered from the last precipitate should now be evaporated to dryness, and the joint weight of both nitrates taken. These being re-dissolved in water and treated with bichloride of platinum, the resulting dry precipitate determines the amount of potash as well as the soda remaining in solution.

The alumina, the only remaining ingredient left behind in the charcoal as an insoluble phosphate, can be extracted by digesting the charcoal first in cold nitro-hydrochloric acid diluted with a little water, and allowing it to stand a few hours; then boiling, filtering, and washing out all soluble matter with distilled water till ammonia ceases to produce any precipitate. Evaporate the filtrate and mixed washings to absolute dryness over a water bath, when we obtain as a residuum chloride of calcium and

chloride of magnesium, with peroxide of iron and phosphate of alumina, the latter, owing to the continued heat, having parted with its chlorine. The peculiar white pearly aspect of the dried mixture will at once indicate the presence of alumina, which, on the addition of distilled water, separates in flocculi. Project the whole on a filter, and wash the residue from time to time with water until it ceases to precipitate with oxalate of potassa, ammonia, or nitrate of silver. Reserving the residue on the filter, we proceed to precipitate, if required, the lime from the filtrate by oxalate of potassa in slight excess, followed by the addition of a little acetic acid, filtering, and evaporating the clear liquor to dryness, incinerating and digesting the residue in distilled water, when carbonate of magnesia and a little oxide of iron precipitate.

The precipitate of iron and phosphate of alumina previously obtained should now be treated with a boiling solution of pure caustic potash, which separates the alumina from the insoluble oxide of iron—sometimes a white precipitate of phosphate of iron may be obtained without any alumina existing—filter, freely acidulate with hydrochloric acid, add ammonia in excess, and boil, when the alumina will be thrown down, mixed with some phosphoric acid, to remove which it may be re-dissolved in hydrochloric acid, diluted with distilled water, carbonate of potash added in excess to precipitate the alumina, and then taking it up again by the careful addition of more hydrochloric acid till the liquid is clear. Now add a tolerable quantity of a solution of tannin, followed by q. s. of a solution of carbonate of potash, not to neutralise the tannic, but the hydrochloric acid, which, by the precipitation of the tannin with the alumina, becomes too free. This must be kept in check or the tannate of alumina cannot fall. By carefully operating in this way, first with tannin and hydrochloric acid and then with q. s. of solution of carbonate of potash, the whole of the alumina may be extracted, and on evaporating the residuary liquor to dryness and incinerating the salt obtained, it will be found to consist of phosphate of potash and chloride of potassium. The dried tannate of alumina should now be boiled with a little strong sulphuric acid, so as to char the organic matter; white fumes will be given off, and after a few minutes distilled water should be added, and the mixture boiled till all the alumina is dissolved. Add caustic potash in excess, then some free hydrochloric acid, and lastly, precipitate the alumina with carbonate of ammonia and boiling. Or instead of tannin, silicic acid may be used for precipitating the alumina, thus: Dissolve the phosphate of alumina in liquor potassæ, dilute with water, and add q. s. of soluble silicate of potassa. A gelatinous silicate of alumina forms, which must be projected on a cloth filter and well washed. Press the residue and collect in an evaporating dish; treat with hydrochloric acid, and evaporate to dryness over a steam bath. The residue being then digested in distilled water, the chloride of aluminum is dissolved out, and the insoluble silica precipitated. On this liquor being filtered, the addition of carbonate of ammonia will, with a slight boil, yield the pure alumina, the whole of the phosphoric acid remaining behind in the filtered liquor as phosphate of ammonia.

As liquor potassæ generally contains traces of alumina, previous experiments should be made on it so as to ascertain the probable amount, for which a deduction or allowance should be made in estimating the alumina extracted from the bread.

County Analyst's Laboratory, Police Station, Cheltenham.

PHARMACY, TOXICOLOGY, &c.

New Process for the Extraction of Quinine and the Estimation of the Alkaloids of the Quinquinas, by M. RABOURDIN.

Extraction of Quinine.—I base this process on the property possessed by fixed caustic alkalies employed in excess to dissolve tannin, insoluble cinchona red, the colouring and resinous matters of quinquinas, without touching the quinine.

My method consists in reducing the bark to powder, and treating it in the cold in a displacement apparatus with water containing three per cent. of hydrochloric acid. I cease pouring in liquid when that which passes has but little taste.

Into the liquid which has passed through, and which amounts to eight or ten times the weight of the bark employed, I pour solution of caustic soda, in quantity about equal in weight to the original bark, and well agitate the whole. The quinine precipitates in white caseous flocks, which settle rapidly; the deep-red supernatant liquid is decanted, and the precipitate collected on a linen filter and washed with a small quantity of water.

Almost entirely composed of quinine, this precipitate may be purified by two methods:—

1. If it is acted on by a quantity of dilute hydrochloric acid insufficient to dissolve the whole of the alkaloid, there is obtained on filtration a colourless solution, which yields with ammonia white and pure quinine.

2. If the precipitate is to be entirely exhausted, a slight excess of acid must be used. In this case there is obtained upon filtration a slightly-coloured liquid, which is bleached in the following manner:—Pour into the hydrochloric solution dilute ammonia until a brown precipitate appears, and upon filtration a colourless liquid is obtained. If, in consequence of the addition of an excess of ammonia, white flocks appear mixed with the brown precipitate, acidulated water must be added until these are dissolved. Ammonia precipitates quinine from the clear and colourless filtrate. In this state it is perfectly white, and only contains traces of cinchonine.

This process appears to me to introduce great economy in the manufacture of salts of quinine,—first, by suppressing the employment of alcohol as a solvent; and second, by avoiding the precipitation of the quinine by lime, and, consequently, the drying of this calcareous precipitate by artificial heat; third, by suppressing the employment of animal black as a decolorising agent; and, fourth, I may affirm, by yielding a more abundant product; for, indeed, this method permits me to obtain so readily the whole quantity of alkaloid contained in quinquinas that I propose it as a means of estimating the value of barks in commerce. The three official quinquinas are equally well treated by this method. When I wish to estimate gray bark I take forty grammes, but only ten grammes in the case of the Calissaya and red barks.

Mode of Assay.—I take ten grammes of Calissaya quinquina in fine powder, and moisten it with water acidulated with four per cent. of hydrochloric acid. The powder is then arranged uniformly in a long tube, provided with a plug of cotton, and exhausted with the same acidulated water with which it was moistened. If the displacement takes place with regularity, and no local caking together of the powder takes place, it is exhausted when there are obtained between 100 and 120 grammes

of filtrate. Into this liquid there are then poured twelve to fifteen grammes of caustic soda solution, which is well mixed by agitation. The precipitate is then collected on a filter and washed with a little water as long as any red liquid passes through.

When the filter has well drained it is removed from the funnel, placed in a dish, and moistened with very dilute hydrochloric acid, so that there is a slight excess of the latter. When solution has taken place, the paper is squeezed out with a glass rod and the liquid poured into a small filter. The paper which has been squeezed is washed twice with water, which is afterwards used to wash the second filter. There is thus obtained a liquid slightly coloured of a reddish-yellow, which is decolorised in the manner indicated above,—that is to say, by adding dilute ammonia until a white cloud begins to appear. If this latter does not disappear on agitation, a few drops of acidulated water are poured in, so as to keep the precipitate of a dark red colour. It is then to be filtered, the filter washed with a little distilled water, and ammonia poured into the clear and colourless liquid, so as to precipitate the quinine. This latter is collected on a paper filter, washed with distilled water, and dried in the air. When dry it is easily detached from the paper. The weight is then taken, which shows the richness of the bark.

Ten grammes of dull Calissaya quinquina should yield 30 to 32 centigrammes of white quinine, containing a very small quantity of cinchonine, which may be isolated with rectified ether.

Red quinquinas vary much. Whilst some very dense, bright, red barks have yielded me 0.33 grammes of a mixture of quinine and cinchonine from 10 grammes, others of a good appearance have only yielded 0.24 grammes when working on the same quantities.

Gray quinquinas have yielded me 0.10 grammes to 0.20 grammes of cinchonine when operating upon 40 grammes of bark; this gives the proportion between the cinchonine and the bark of 5 to 1000.

I have decided upon the treatment in the cold and by displacement to exhaust the barks because it is the method which has succeeded best with me. Decoction in acidulated water gives a liquid much changed with colouring matters. When caustic soda is added to this, the precipitate remains of a red colour, the filtration takes place with extreme slowness, and less product is finally obtained.

I have exhausted quinquinas with alcohol of various strength. I have added water to the alcoholic liquid, evaporated off the alcohol, and then added caustic soda to the residue. As with the liquids obtained by decoction the precipitate of quinine was mixed with colouring matter, the filtration was difficult and the product less abundant.

I prefer, therefore, the treatment in the cold, which gives real and positive reactions. When a little skill is acquired in this process, the assay of a quinquina will only be found to occupy about an hour's work.—*Journal de Pharmacie et de Chimie.*

On the Deleterious Effect of Light on Potatoes.

THE influence of light on vegetation is now regarded as a matter of the utmost importance, and although the precise mode of action may not be always understood, yet powerful effects of it are everywhere perceptible. In its absence leaves become blanched that would otherwise be green. Roots that are white underground become

green when exposed. Turnips, white beneath, are green or perhaps red above, and many kinds of fruit, naturally pale, colour under bright sunlight. By the action of light on leaves, the different secretions peculiar to plants are formed, such as gum, sugar, starch, oils, and even, in certain kinds of plants, deadly poisons. In some plants, too, the secretions due to the action of light are in certain portions harmless and nutritious, whilst in other parts of the same plant, through the same agency, highly deleterious principles are formed. The potato offers an example. Everybody knows that its tubers contain wholesome food, and it is also generally known that the stems, and especially the apples or seed vessels, are deleterious. But the treatment to which the potato is sometimes subjected is calculated to develop the poisonous quality in the tubers themselves, a change which can only take place during exposure to light. The poison found in the green parts of potatoes is called "solanine." This exists in several species of *Solanum*, and is found in considerable quantity in the shoots of potatoes. To obtain it the shoots are bruised and acted on by water acidulated with sulphuric acid. It is very poisonous. (Turner's "Elements of Chemistry.") Liebig says it is a powerful poison.

Although the stems of potatoes, according to the authorities just quoted, contain in notable quantity the noxious and easily-extracted principle, so dangerous in its concentrated form, yet the tubers grown underground and kept in the dark are floury and white when cooked, if the variety of potato is good, and quite free from acrid taste, which is one of the characteristics of solanine, and a sure indication of its presence. But the potato tuber is in reality a sort of stem; for it is furnished with buds, which, under favourable circumstances, push into shoots, as buds do on stems above ground. It is therefore, highly susceptible of the influence of light; for although both its skin and flesh are white, they soon become green by exposure; and the continued presence of light renders them as green as stems above ground. It is said that pigs have been killed by giving them potatoes greened to this extent. Such, of course, could not be sold for human food. For this purpose potatoes exposed to light must be housed or otherwise shaded before the green tinge is apparent, at least to the naked eye. But under the impression that the tubers keep better after having well basked in the sun, many cultivators are in the habit of turning them up, and spreading them out on the surface of the ground in bright sunny weather. This has the effect of greatly deteriorating their quality. Notwithstanding disease, really good potatoes can be found; but even slightly diseased ones, with the infected portions cut away, are infinitely better than quantities of others which, though they have a goodly appearance, have been greened. Instead of being white and floury when cooked, they are yellow, and have a disagreeable acrid taste, which can scarcely be disguised, or, if it should, there is no proof that the deleterious effects of the acrid principle are counteracted. At all events, it would certainly be very desirable that such means should be adopted as would prevent that principle being generated, or, in other words, the tubers should be kept as much as possible in the dark instead of exposing them to light. The advantage of exposure as regards better keeping is doubtful, whereas the deterioration it occasions in the quality of the tubers as an article of food is certain. I have thus endeavoured to draw attention to the subject, and it is the duty of every one who is aware of the deleterious effects of light on the potato to explain it to those who are not; for a knowledge

of it, if acted upon, would prove beneficial to both rich and poor.—*The Gardener's Chronicle and Agricultural Gazette.*

PHYSICAL SCIENCE.

On Spectrum Analysis, by Dr. K. M. GILTAY, Director of the Société Batave of Rotterdam. (Extract from a Letter to the Abbé Moigno.)

I READ in the *Cosmos* of September 13, that MM. Bunsen and Kirchhoff are blamed for not having mentioned M. Plucker as their forerunner in spectrum analysis. Not only do I perfectly agree with you, but, in my opinion, this is their least fault. M. Plucker has taken a great step further which they have entirely overlooked, and which is of great consequence in the explanation of these phenomena, perhaps also to the conclusions to be drawn from them.

M. Plucker has proved by vigorous experiments that the spectra of incandescent gases (simple or compound) are formed by luminous bands of an obscure background; that by properly narrowing the slit these bands can always be decomposed into bands of the same width as the slit; that there are no smaller ones¹; and that consequently these bands are formed by the homogeneous light, of which the index of refraction may be measured by determining that of the centre of the band; and that, strictly speaking, they may be broader than the slit, and the light is then of a continuous refrangibility, between the limits of dispersion of the extreme rays of the band. These larger bands may then form a considerable proportion of the entire spectrum. M. Plucker has measured, not only the index of refraction of these bands, but the length of the wave, so as to render the measurements wholly independent of the prism. In discussing the results, M. Plucker states his opinion that Fraunhofer's lines were not to be compared with these luminous bands, on account of the small diameter of the lines, almost innumerable in the spectrum which Fraunhofer, and after him Baden Powell and Brewster, have produced with a good sized slit. These lines of Fraunhofer's are, then, obscure lines on a luminous background. They are the particular lines wanting in a continuous spectrum. In Plucker's spectra, on the contrary, the bands are of homogeneous light on a dark background. These are the spectra of discontinuous refrangibility.

In their spectrum analyses, MM. Bunsen and Kirchhoff have made no strict measurements. They give the position of the lines only in relation to the solar spectrum by comparing the luminous rays of their spectra with Fraunhofer's obscure lines. I have repeated their experiments, and I can affirm that their conclusions are not a legitimate interpretation of the phenomenon. The luminous bands of different spectra are for the most part bands of homogeneous light, having the same breadth as the slit when they are properly contracted; if enlarged they are superposed. The calcium spectra gives the result admirably. The bands of lithium and potassium

¹ We believe that M. Plucker regards as an exception to the general rule the instance in which in the light examined by the prism there are solid and incandescent particles, which are all drawn in one common direction. The light being near the slit, there should be, besides the spectrum of the light, the spectrum of the minute, infinitely narrow line described by the incandescent particle, and seen across the slit. We adduce, as an example, the magnificent spectrum obtained by the prismatic analysis of the interior of the small flame obtained by burning ordinary lighting gas with the aid of a jet of oxygen gas.

being far apart, either do not give this superposition or give it very imperfectly. But it is chiefly by the aid of these spectra that we can demonstrate that the bands are always the same width as the slit. I repeated these experiments with one of Babinet's goniometers, and with a slit varying from 3' to 45'. In the calcium, strontium, and barium spectra there are bands somewhat wider than the slit, but, as M. Plucker says, the explanation of this is not difficult. Thus, these bands are not to be compared to Fraunhofer's lines.

So much for the facts; now for the consequences. MM. Bunsen and Kirchhoff have pointed out these spectra, first, as the best, or at least the most sensitive, means of chemical analysis. This is perfectly correct; but M. Plucker has said it before them, and you have proved it. Second, as a means of discovering the elements of the celestial bodies. They start with the law discovered (?) by M. Kirchhoff, that incandescent gases absorb instead of transmitting the rays which they emit. By superposing the solar spectrum, made sufficiently intense, upon the spectra of artificial lights, they found that some of these luminous bands became obscure. These were the bands coinciding (?) with certain lines of Fraunhofer. From this they concluded that the solar atmosphere gives a spectrum the inverse of what it would have given had it been the only luminous body; that this solar atmosphere contains the elements which give these characteristic bands, which become obscure by superposition.

Does not this conclusion require extreme circumspection, since it has not been proved that these luminous bands and Fraunhofer's lines are comparable? In the spectra of MM. Bunsen and Kirchhoff each band is only the length of a wave; each of Fraunhofer's lines, however fine it may be, has an infinity of them. That the luminous band should become obscure, the solar spectrum being superimposed, is very simple, since with a light so intense the rays of an isolated refrangibility, without the neighbouring rays of refrangibility to the right and left, would become very dark.

Allow me to remind you of Forbes' experiments, by which he proved that Fraunhofer's lines do not become darker during a total eclipse—that is to say, when the rays coming from the edge of the sun have to traverse a thicker solar atmosphere. This is, however, what must take place according to the hypothesis of MM. Bunsen and Kirchhoff. This is a little hasty (see your *Optique Moderne*), but it is worth discussing in relation to the other conclusions.

Has it not been observed that Fraunhofer's lines are less obscure at a great height above the level of the sea, and even that some of them are missing? This has caused their absorption to be attributed, at least in part, to our atmosphere.

Finally, one definite substance can perfectly absorb rays of different refrangibilities, as is proved by the spectrum of a luminous ray which has passed through certain gaseous bodies, nitrous acid, iodine, bromine, &c.—*Cosmos*.

PROCEEDINGS OF SOCIETIES.

PHARMACEUTICAL SOCIETY.

Wednesday, December 4, 1861.

Mr. P. SQUIRE, President, in the Chair.

THE adjourned discussion on the *Preparation of Extracts* was re-opened by the Chairman, who deprecated any

departure from the directions of the Pharmacopœia which the previous discussion might be supposed to justify. He acquitted the compilers of the Pharmacopœia of ignorance. They did not consult the makers of extracts; and the makers did not know that they produced a better article than could have been made if the directions of the Pharmacopœia had been followed. That book was compiled by the highest scientific authorities in the country; but, until now, practical pharmacutists had not been consulted. A delegate from the Pharmaceutical Society was now, however, assisting in the preparation of the British Pharmacopœia, and pharmacutists had the opportunity of conveying information to the authors. When, therefore, the work is constructed, it will be the duty of pharmacutists to implicitly follow its directions. The Chairman then quoted the processes for the preparation of extract of belladonna given in several foreign Pharmacopœias. The French Codex was not quoted, the last edition having appeared twenty-five years ago, and a new one being now in progress. The Prussian Pharmacopœia, used in Germany and Switzerland, ordered the leaves, stem, and flowering branches to be taken, moistened with one-third the weight of water, pressed, strained, and evaporated, filtering at a certain stage to get rid of the chlorophyll. A dry extract was also ordered to be made by taking a certain amount of the moist, drying it, and then mixing sufficient sugar of milk to make up the original weight. The Belgian Pharmacopœia of 1854 directs the flowering herb to be used. The Norwegian Pharmacopœia, published last year, orders the extract to be made from the powdered dried leaves. The process of the American of 1855 is the same as our own. The Chairman then gave a *résumé* of the previous discussion; the tendency of which, he said, was to support the use of all the soft parts of the plant in making the extract.

Professor REDWOOD said the subject under discussion was a very interesting one as well as of considerable importance. While admitting that at the last meeting (which illness had prevented him from attending) it had been most fully and well discussed, he felt very dissatisfied with the results in two or three respects. In the first place, he thought the general tone of that discussion was to weaken the authority of the Pharmacopœia, and encourage pharmacutists to deviate from its directions. The Chairman had cleared up this point in his opening speech, and nothing further need be said on that part of the subject. The Professor then called on the Chairman to describe more particularly the parts of the plant used in the preparation of the extract said in the paper to be the strongest and best. Was it really made from stalks, or partly from flower and fruit? From the report of the last meeting it would certainly appear that the stronger extract was made from stalks, and, therefore, the manufacturers would reserve the leaves for purposes to which the stalks were not applicable, and extracts would be made from these and nothing else. The tendency of the foreign Pharmacopœias, the Professor said, was to deviate from the process received as the best in this country, and to reject the albuminous matter and chlorophyll. Extracts made in this way would keep good an indefinite time. Mr. Scanlan had pointed out twenty years ago that the mouldiness of extracts was due to the albumen, take that out and the extract will keep.

THE CHAIRMAN, in reply, said that no manufacturer would think of using the thick, hard stalks of plants for making extracts, and his paper indicated that large stalks were not to be used. The Pharmacopœia directed the extract of belladonna to be made from leaves only: he had taken the rejected "soft parts," and had prepared an extract which was one-fourth stronger. By "soft parts" he did not mean the thick, hard stalks. His meaning was well given by Mr. Holland in the last discussion, who said he used all the soft parts "which could be easily broken by the hand."

A long conversation followed, in which, however, nothing of novelty or interest was elicited, and the meeting adjourned, the other papers announced being postponed until the next.

MANCHESTER

LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, November 26, 1861.

J. P. JOULE, LL.D., President, in the Chair.

Mr. JOSEPH SIDEBOTHAM exhibited a photograph of the contorted schists seen in the cliff near the South Stack Lighthouse, Holyhead.

Mr. JOHN PARRY showed some photographs of fossil woods from the South Lancashire coal field. These displayed beautifully the structure, even to the most minute vessels.

A Paper was read by Mr. E. W. BINNEY, F.R.S., Vice-President, entitled "*Additional Observations on the Permian Beds of South Lancashire.*" This was a continuation of two previous papers read before the Society and printed in its Memoirs.¹ Since that time the author had made further observations on the Permian strata at Heaton Norris, near Stockport; Medlock Vale, between Ashton and Manchester; Chorlton-upon-Medlock, and Ordsal, near Manchester; and Skillaw Clough and Bentley Brook, near Newburgh, in the West of Lancashire.

At Heaton Norris, in the sand delf of Mr. Howard, near the railway station, the lower new red sandstone was seen dipping to the south-west at an angle of 25°. This was succeeded by red and variegated marls having a similar dip. These last named strata were overlapped by the Trias, which dips to the south-west at an angle of 12°.

At Heaton Mersey the following section was met with:

	Feet.
Trias.	45
Permian—Red and variegated marls containing lime-stones	9
Lower new red sandstone grooved	402

The Permian beds were cut off by a fault near the railway station at Heaton Norris, first shown to me by Mr. Hull, B.A., F.G.S., of the Geological Survey, which brought in the Trias. This rock occupied the district between that town and Goyt's Hall, in the Marple valley, where the lower part of the middle coal measures was seen in a nearly vertical position.

The author considered that Mr. Howard's sand delf is a likely place for ascertaining the existence of a coal field under the town of Stockport worth working.

The next was a section made by Mr. John Wood, at Medlock Vale, between Waterhouses, near Ashton-under-Lyne, and Manchester. It was as follows:

	Feet.	In.
Drift	26	0
Trias	23	0
Permian—Red marls, with beds of limestone and five beds of gypsum.	246	3
Lower new red sandstone.	375	11
Coal measures	about 90	0

761 2

What these coal measures were, whether above or under the Bradford Four Feet Mine, it was at present impossible to say; but it was to be hoped that some mine would be met with to enable us to determine the value of the great tract of coal measures lying between Ashton-under-Lyne, Oldham, Middleton, and Manchester. Mr. Wood had done more than any other gentleman to clear up this point, and it was to be desired that he should meet with

a good seam of coal, both for his own sake and that of the public.

The third section mentioned was at the sugar works of Messrs. Fryer and Co., in Chester Street, Chorlton-on-Medlock, Manchester. The following beds were there met with.

	Feet.
Trias.	114
Permian—Red marls with limestones	237
Coarse red sandstone with pebbles	45
Coarse red sandstone.	24
Coal measures consisting of red shaly marls and limestones (Ardwick).	126

546

The limestones in the last named strata contained specimens of *microconchus carbonarius* and scales of *paleontiscus*, which clearly proved them to be similar beds to those of the upper coal field at Ardwick, to which they bear every resemblance in physical character.

The occurrence of coal measures on the south side of the city of Manchester is quite new and of great importance, showing that such strata at places are met with under permian and trias deposits much nearer the surface than was previously suspected, and where the upper rocks gave no evidence of their proximity. The above bore has proved beyond doubt that a band of coal measures lies under the south of Chorlton-on-Medlock, and possibly extends to Heaton Norris, being probably brought up by the great Pendleton fault, which most likely passes through the south of Manchester and joins the fault seen near the railway station at Heaton Norris previously alluded to, and described more fully in the Paper.

In the fourth section, at Ordsal, Messrs. Worrell found the trias beds 460 feet in thickness without going through them. At the bottom of the bore the water became so salt that they discontinued the work, it being no longer fit for dyeing and such like purposes. This is the first instance, to the author's knowledge, where salt water has been met with in the trias near Manchester.

The fifth and sixth sections were at Skillaw Clough and Bentley Brook, to the north of the Newburgh station on the Manchester and Southport Railway. These were some time since discovered by Mr. E. Hull, B.A., F.G.S., of the Geological Survey, and described shortly by that gentleman in the sheet explaining the map of the district. Further particulars were given of the details of both sections, and an analysis of the limestone was produced, which showed it to differ in its chemical characters from the thin ribbon bands found in the permian marls near Manchester, Patricroft, Astly, and Leigh, and was very like the yellow magnesian found at Stank, in Furness, North Lancashire. Probably it might prove to be a different bed, and more like the great central deposit of magnesian limestone of Yorkshire than the thin beds previously alluded to.

MICROSCOPICAL SECTION.

November 18, 1861.

E. W. BINNEY, F.R.S., F.G.S., in the Chair.

Dr. Edward Stephens was elected a member of the Section.

The SECRETARY read a communication from Mr. THOMAS G. RYLANDS, of Warrington, "*On the Classification of the Diatomaceae.*" The author desired to call attention to the want of systematic arrangement which characterises this favourite branch of microscopical investigation, and to the necessity of a thorough revision of the entire classification of the natural order. The author presented to the Section two slides to illustrate his arguments. The predominant form of frustules was first named by Dr. Brebisson *Cocconeis lavis*. In 1857 it was published by Mr. Roper (M. I. vol. vi. p. 22) under the provisional name *Cocconeis*.

¹ Vols. xii. and xiv. (New Series) of the Society's Memoirs.

discus ovalis; but in consequence of finding on the valves eight to twelve submarginal obtuse processes with tumid bases, quite distinct from the spines or teeth which occur in the *Coscinodiscæ*, the author considers this species must be placed in the genus *Eupodiscus*, and may fitly be called *Eupodiscus levis*. The specimens were obtained at Llandudno, in ripples in the sand below mid-water; and the Paper concludes with a description of some peculiarities connected with their sudden disappearance.

Mr. JOHN WATSON read a paper "On certain Scales of some Diurnal Lepidoptera," in which he recommends a new and careful study of this subject. In some genera peculiar scales, called plumules, have long been known; but examination with the binocular microscope shows that they are not flat like the ordinary scales, but cylindrical and hollow. They have been found only in certain genera (named in the paper) at present, and on the males alone; they possess generic resemblances and specific differences, each species displaying its own distinguishing variety. One of great beauty and novelty, found only on two African butterflies, *Pieris Agathina* and *Pieris Chloris*, was described, and some very fine drawings of it, by Mr. Joseph Sidebotham, were exhibited, and also other figures by him of about one hundred species never figured before. The names and habitats of the insects were given, and the author pointed out the value of these scales for the assistance of the scientific entomologist in arranging genera and species; he then entered into the question of their probable use as air vessels in the economy of the insects possessing them.

The CHAIRMAN remarked that the scales of the Lepidoptera may prove to be as valuable in determining species as the scales of fishes.

Mr. SIDEBOTHAM alluded to the value of the binocular microscope in defining the cylindrical form of the plumules, and described the mode of finding them *in situ*, by breaking the wing.

Mr. WATSON stated that some of Mr. Sidebotham's excellent drawings were taken under the eighth objective, magnifying 750 diameters. Mr. Watson further said that he had examined the wings of 400 specimens of the *Papilionidæ*, but had not discovered any plumules in that genera; he also alluded to several so called species from South America, of which no males have yet been found, others of which no females have yet been discovered, and suggested the possibility of some of these being male and female of the same species; to ascertain which, careful examination of the scales might be useful.

Mr. WATSON exhibited a number of mounted specimens of the plumules, and four cases of the Lepidoptera from whose wings the 98 drawings figured by Mr. Sidebotham were taken. They were principally *Pieris*, *Anthocaris*, *Thestias*, *Euterpe*, and *Eronia*; amongst the former were some new and unnamed species from Celebes, with rare specimens from Venezuela, Quito, East and West Indies, Africa, and other parts of the world.

Mr. DALE placed on the table a number of washed soundings, of which several members took specimens for examination.

Mr. DALE also reported that the sulphate of copper and magnesia has been long known to be a compound salt. When the magnesian salt is in excess, it has seven equivalents of water; and when the copper salt is in excess, it has five equivalents of water. Mr. Dale has also made for the occasion samples of the double sulphate of copper and potash, the double sulphate of copper and ammonia, and the double chloride of copper and ammonia, which he distributed amongst the members. They were all beautiful polariscopic crystals, the chloride particularly so, and each has its characteristic form.

Mr. LINTON exhibited the hairs on the *Loasa Coccinea* (Chili nettle), mounted by Mr. Heys.

Mr. DANCER exhibited a specimen of the *Aulocadiscus formosus*, which in consequence of a fracture since it was

mounted, shows more distinctly the form of the projections that Mr. Sidebotham brought under the notice of the Section at the last meeting. These handle-shaped spines Mr. Dancer observed, when seen with the one-eighth power and binocular microscope, are found to project outwards from each of the four elevations on the surface of the valve.

Mr. BROTHERS exhibited the *Floccularia ornata*, and a fine group of *Lacinularia socialis*.

Mr. MOSLEY exhibited a specimen of *Amœba*, and described the peculiarity of its motion. He had observed one of them rise in a straight line four-fifths of its entire length, its internal particles appearing to force outwards a transparent or invisible integument; the point then fell over, forming an arch, after which it subsided into the common three or four-lobed form. In many specimens no vacuole was observed; in others, an apparent air or water space was noticed. A portion of the one under examination appeared to be blown outwards like a bladder, in which appeared a small animalcule making desperate efforts to escape. The best illumination for observing the internal motion, he considers to be Wenham's parabola and Lieberkühn, with a four-tenth objective.

NOTICES OF PATENTS.

769. *The Preparation of Hydrated Oxide of Iron, and the Application of such Prepared Oxide for the Absorption or Separation of Sulphur from Certain Gases.* J. G. WILLIAMS, Belfast. Dated March 27, 1861.

ACCORDING to this invention the brown hæmatite, or other natural form of hydrated oxide of iron, is reduced to fine powder by grinding under mill-stones, the small particles are then separated from the coarser, either by sifting, or by means of currents of air, or water, when a material is obtained admirably adapted for absorbing the sulphuretted hydrogen from foul coal gas and other gaseous mixtures containing it. The second part of the invention relates to the treatment of sundry ferruginous products, such as roasted pyrites, forge cinder, hammer scale, and furnace slags, containing not less than 30 per cent. of iron, which are to be subjected to heat in contact with carbon, or any appropriate deoxidising agent, with the intention of producing a crude kind of metallic iron, and afterwards leaving this exposed to the rusting influence of air and moisture, to obtain a partially oxidised material well calculated to retain the sulphuretted impurities of common coal gas.

This proposal appears likely to be serviceably employed in the preparation of ochrey iron ore, and other forms of hydrated oxide which occur not only in Ireland, but away from coal, and hence comparatively valueless in some parts of England. A mixture of sulphate of iron, lime, and sawdust (really equivalent to hydrated oxide of iron), has formed the subject of more than one patent claim for a similar application. The ochrey ironstone of Oxfordshire has also been employed for the purification of gas.

775. *Brewing.* L. J. VANDECASTELLE, Lille, France. Dated March 28, 1861.

THE improvements comprehended in the specification refer to the employment of lime in the mash tub, for the purpose of more thoroughly removing the suspended impurities from the wort. After clarifying in this manner, the excess of lime is precipitated from the beer by passing a current of carbonic acid gas, using for this purpose a perforated worm tube.

Ordinary beer holds in solution a large proportion of earthy phosphates, which would, no doubt, be partially removed by the addition of lime. It is a serious consideration whether perchance so dark coloured a beverage might

not sometimes disguise the presence of carbonate of lime particles, held in suspension in the beer, the administration of which to the human digestive system might be attended with dangerous consequences.

Grants of Provisional Protection for Six Months.

2750. William Bickford Smith, Camborne, and William Bennetts, Tuckingmill, Cornwall, "Improvements in the method of, and apparatus for, preventing the injurious effects occasioned by smoke, sulphur, and the deleterious gases which escape from stacks, chimneys, calcining houses, chemical and other furnaces."—Petitions recorded November 1, 1861.

2790. Frederick George Stuber, St. James's-road, Brixton, Surrey, "An improved hygrometer for measuring the humidity of the atmosphere, dampness of beds, garments, and for other similar purposes."—Petition recorded November 6, 1861.

2864. John Leslie, Esq., Conduit Street, Hanover Square, London, "Improvements in the manufacture of gas."—Petitions recorded November 13, 1861.

2900. George Parry, Ebbw Vale Ironworks, Monmouth, "Improvements in the manufacture of iron and steel."—Petitions recorded November 18, 1861.

Notices to Proceed.

1863. William Longmaid, Inver, Galway, Ireland, "Improvements in the manufacture of iron."—Petitions recorded July 24, 1861.

1869. Edward Haefely, Kearsley, Lancashire, "Improvements in extracting copper from its ores."—Petition recorded July 25, 1861.

2001. Antoine Garzand, Boulevard St. Martin, Paris, "A new or improved apparatus for cutting up and reducing dye and other wood."—Petition recorded August 12, 1861.

2031. John Bethell, King William Street, London, "Improvements in the manufacture from steatite of journals, axle-boxes, and bearings for machinery, axles, and spindles to work in, smoking pipes, buttons, crucibles, and pots, for chemical and smelting purposes, and also of a lubricating compound for railway and other carriages."—Petition recorded August 15, 1861.

2618. Frederick John Evans, Gas Works, Horseferry Road, Westminster, "An improved mode of, and apparatus for, carburetting gases for the purpose of illumination."—Petition recorded October 19, 1861.

CORRESPONDENCE.

On the Chemico-Microscopical Characters of Beef and other Fatty Matters.

To the Editor of the CHEMICAL NEWS.

SIR,—Following out my experiments on fatty matters, as suggested in my paper contained in your last impression, I find that beef dripping treated with benzole, and then with ammonia, assumes under the microscope a similar character to that of butter, with this difference, that the crystals of beef fat are more strongly marked and of greater angular divergence, whilst the uncrystallizable portions are more opaque than those of butter. There is a striking difference, however, in their solutions in ether by which any adulteration is clearly indicated. Beef fat depositing considerably after a time, but butter as before observed is entirely and permanently soluble at 65° and it is only at about 52° or 53° that anything falls when masses of beautiful velvety crystals not unlike benzoic acid form; mutton dripping similarly treated is of a firmer and waxy like character, and under the microscope does not show any tendency to crystallization, being in the form of transparent layers or plates, and deposits from its ethereal solution in a closer and more granular form than beef dripping.

These are all the observations I have thought necessary to complete the subject. No doubt Anatomists and Physiologists will find the microscopic method I have suggested of some value in their researches.

The crystals I speak of are probably those of margarine, being when broken in plumose tufts or brushes, but when whole in the stellar form, the long needle points radiating from one common centre. They are smallish in cow fat, i.e., butter, ox fat being, as before observed, similar but larger; mutton fat being nearly all stearin, while lard consists principally of olein and stearin, with comparatively very little margarine; hence its unctuousity and apparent indisposition to crystallise.

I am, &c., JOHN HORSLEY,
County Analyst's Laboratory, Police-Station, Cheltenham.

Chemical Nomenclature.

To the Editor of the CHEMICAL NEWS.

SIR,—Will you allow me to make a few observations in reply to Mr. McLeod's remarks on my new system of nomenclature?

Mr. McLeod objects to my system because of the complicated names to which it leads, though, at the same time, the longest of these is not a quarter the length of those he himself proposes.

None of the names which I have hitherto constructed from empirical formulæ are more than four syllables in length; the greater part are composed of two or three syllables,—so much for the charge of complexity.

I subjoin the names of a few bodies taken at random:—

Citric acid	$C_6H_8O_7$	kamlatic acid
Tannic acid	$C_{12}H_{10}O_{11}$	gremfetic acid
Benzoic acid	$C_7H_6O_2$	lakdatic acid
Salicylic acid	$C_7H_6O_3$	lakfatic acid
Gallic acid	$C_7H_6O_5$	lakhatic acid
Amygdalin	$C_{20}H_{27}NO_{11}$	chetranine
Harmaline	$C_{15}H_{15}N_2O$	tarbanamine
Harmine	$C_{15}H_{15}N_2O$	taabanamine
Aconitine	$C_{34}H_{47}NO_7$	slolhanine
Morphine	$C_{17}H_{19}NO_3$	zehfanine

Mr. McLeod disposes of my system in a very summary manner. After having said that it was similar to those of Gmelin and others, he thinks that my name "for acetic acid (dagdatic acid) is not much simpler than Gmelin's term for alcohol (linavinam)," and he therefore condemns both.

By what peculiar process of oxidation or reduction Mr. McLeod was led to compare my name for acetic acid with Gmelin's for alcohol, I cannot for a moment conceive, and am equally at a loss to understand why, upon the ground of the one name not being much simpler than the other, he concludes that my system "has no chance of being adopted."

The names assigned by me to acetic acid, and ethylic alcohol, are dagdatic acid and dakkatic alcohol, both of which are as short and simple as those now in use, besides having the great advantage of expressing the formulæ of the bodies in question.

Gmelin furnishes names for all substances whatever; I restrict myself to what are commonly called organic bodies, for, as carbon, hydrogen, and oxygen appear to possess extraordinary faculties of entering into combination, the compounds of these elements require a special nomenclature of their own.

The systems of Gmelin, Laurent, and others, require long study before they can be comprehended, let alone made use of; mine, on the other hand, is very brief, easy to be understood, and can be at once turned to practical account, even if it be only as an aid to the memory.

Gmelin's names are lengthy, very unmusical, difficult to pronounce, and utterly unlike those at present employed, which, moreover, they were intended to supersede. Mine,

on the contrary, are short, possessed of considerable euphony, very like those at present employed, and may be either used in conjunction with these, or *per se*.

The plan of naming a body by enumerating all the radicals it contains, or is supposed to contain, must infallibly lead to names of great length; but, since this appears to be quite the fashion, it is a great pity that the names of these radicals do not give us the least idea of their composition.

The following are a few of the radicals named after my method:—

Methyl CH_3 bafyl	Phenyl C_6H_5 kahyl
Ethyl C_2H_5 dahyl	Ethylene C_2H_4 dagyl
Propyl C_3H_7 fahyl	Glyceryl C_3H_7 fahyl
Butyl C_4H_9 gahyl	Acetyl $\text{C}_2\text{H}_3\text{O}$ dasfayl
Amyl C_5H_{11} hahyl	Benzoyl $\text{C}_7\text{H}_5\text{O}$ lahbayl
Caproyl C_6H_{13} kahyl	&c. &c. &c.
C_7H_{15} lehyl	
C_8H_{17} mehyl	
C_9H_{19} nehyl	

Are not these terms "short, concise, and expressive"? Mr. McLeod says that, "if a nomenclature that would not interfere with the designation of the derivatives of ammonia could be found, it would stand more chance of adoption." To this I answer that my system does not interfere with the method at present employed in naming these compounds, except so far as the radicals they contain are concerned; for instance, I call

Methyl-ethyl-amyl-phenyl-ammonium,
Bafyl-dahyl-haryl-kahyl-ammonium;

the only difference being that the names methyl, ethyl, &c., indicate nothing, whereas bafyl is CH_3 , dahyl C_2H_5 , haryl C_3H_7 , and kahyl C_6H_5 .

I have indeed employed certain syllables to indicate the amount of nitrogen in a substance when its nature was not thoroughly understood; thus, I have called morphine sehfanine, expressive of the formula $\text{C}_{17}\text{H}_{19}\text{NO}_5$; but, by and by, when the radicals which enter into the composition of this alkali are better known, we shall be able to construct a name for it five or six times as long as the preceding, and one which will therefore, no doubt, "stand more chance of adoption."

Empirical names may of course be framed for the compound ammonias, such as

Methylethylamylamine	$\text{C}_9\text{H}_{17}\text{N}$ mehnine,
Ethylamylamine	$\text{C}_{13}\text{H}_{27}\text{N}$ telnine, and
Methylethylamylphenylammonium	$\text{C}_{14}\text{H}_{21}\text{N}$ vepnine.

But I do not advocate the use of such terms except under certain conditions, though I conceive that empirical names are at least as useful as empirical formulæ, and that all the arguments of Laurent and others in support of the one may be adduced with equal force in behalf of the other.

An "Inquirer" desires to know what name I give to albumen. My reply is that, when the formula of albumen is established beyond a doubt, it will be time enough to think of constructing a name expressive of its composition; but, whether a new appellation be devised or not, I would always retain the name of albumen, my desire being to preserve, as far as possible, names which are associated with the labours of illustrious men, which we have long been accustomed to pronounce, and which have become "familiar in our mouths as household words."—I am, &c.

JOHN A. R. NEWLANDS, F.C.S.

Chemical Notices from Foreign Sources.

I. GENERAL CHEMISTRY.

Compounds of Two Acids.—M. Schutzenberger announced some time ago, the formation of a compound of anhydrous sulphuric and anhydrous hypochlorous acids (CHEMICAL NEWS, vol. iii. p. 226). He has now (*Comptes-Rendus*, t. liii. p. 532) ascertained by analysis the exact

proportions in which the acids had united. The compound is obtained by passing a current of hypochlorous acid over the anhydrous sulphuric. As soon as the two acids come in contact, the sulphuric liquefies, becomes hot, and takes a deep red colour. The product is thick, and on cooling soon solidifies to a mass of bright red acicular crystals very like chromic acid. These fuse about 55°C , and are immediately decomposed by water into hydrated sulphuric and hypochlorous acids. Organic matters such as sugar and alcohol, reduce the compound producing incandescence, iodine acts on it violently disengaging chlorine and forming iodic acid; in a word, it behaves as an energetic oxidiser. Heated quickly it detonates. By saturating it very carefully with pure potash, dividing the solution into two equal parts, estimating the chlorine in one and the sulphuric acid in the other, the author came to the conclusion that it was a compound of 4SO_2 and ClO .

Hypochlorous acid gas passed into liquid sulphurous acid sets up a lively reaction; chlorine is evolved and there remains in the receiver a thick red liquid which contains both acids, but in what proportions the author has not yet determined.

Acetic and Arsenious Acids unite in equivalent proportions when boiled together. The product is a thick, colourless, syrupy body, which on cooling becomes vitreous and quickly attracts moisture. Water instantly decomposes it into the two acids. About 220°C . it evolves a good deal of carbonic acid and traces of arseniuretted hydrogen, hydrated acetic acid passing off at the same time and leaving a residue of metallic arsenic. The new compound is an acetate of arsenious acid: $\text{C}_4\text{H}_5\text{O}_3$, As O_3 .

Acetic and Boracic Acids in the anhydrous state behave like the two former, but the solution takes place slower. The compound obtained is also a vitreous mass decomposable by water. By dry distillation the acetic acid is separated, and a reddish brown residue remains which contains all the boracic acid used. Like the above the compound of the two acids will no doubt have the formula $\text{C}_4\text{H}_5\text{O}_3$, Bo O_3 .

Acetic and Tartaric Acids when boiled together form a thick, yellowish, syrupy liquid which begins to decompose at about 130°C , evolving a mixture of carbonic acid and carbonic oxide, the volume of the former a little exceeding that of the latter.

The same compound is formed by the action of chloride of acetyl on tartrate of lead, chloride of lead being also produced. This leads to the formula $\text{C}_8\text{H}_4\text{O}_{10} \cdot 2(\text{C}_4\text{H}_5\text{O}_3)$.

Acetic and Sulphuric Acids. When anhydrous acetic acid is properly cooled it absorbs vapours of anhydrous sulphuric acid, forming a yellowish, gummy mass, which is soluble in water. The solution saturated with baryta water and separated by filtration from the sulphate of baryta, yields crystals having the characters and composition of sulphoacetate of baryta. It follows that by the direct action of the two acids there is formed anhydrous sulphoacetic acid, or the anhydrous bisulphate of the oxide of acetyl, 2SO_3 , $\text{C}_4\text{H}_5\text{O}_3$.

Action of Perchloride of Phosphorus on Several Elements.—M. E. Baudrimont has made a long series of experiments, from the results of which he draws the following general conclusions. (*Comptes-Rendus*, t. liii. p. 637.) 1. The metalloids exert a more varied action on perchloride of phosphorus than the metals. 2. Oxygen forming a chloroxide of phosphorus presents a remarkable case of the substitution of this gas for chlorine. 3. Sulphur and selenium do not act in the same way. Sulphur gives compounds of PCl_3 and S_2Cl_2 . Selenium forms protochloride of phosphorus and protochloride of selenium, both free. Further, selenium does not form a chloroselenide of phosphorus, but a compound of PCl_3 with PCl_3 may be obtained. 4. Iodine acts on PCl_3 and forms ICl , which combines with the excess of PCl_3 to produce the compound $\text{PCl}_3 \cdot \text{ICl}$. 5. Metals, by simple mixture,

at a temperature short of volatilising the PCl_5 form protochloride of phosphorus and metallic chlorides, which mostly unite with the excess of PCl_5 to produce double chlorides. This is the case with aluminium, iron, tin, bismuth, platinum, and perhaps zinc and copper. 6. When the metal is red hot, the "dechloruration" is more complete, and free phosphorus is obtained, which in some cases combines with an excess of the metal and forms metallic phosphides. 7. When the phosphorus becomes free it is always at the expense of the PCl_5 first formed and then decomposed. 8. Gold and platinum are acted on by perchloride of phosphorus, the latter metal most easily. 9. Of all metals antimony is the most easily acted on. 10. In all these reactions, the perchloride may be considered to act like strongly condensed chlorine, and the action seems to be greatly facilitated by the tendency which the PCl_5 has to unite with most of the chlorides to which it gives rise.

II. ORGANIC CHEMISTRY.

Compound of Aldehyde and Oxide of Ethylene.

—Wurtz (*Comptes-Rendus*, t. liii. p. 378) formed a compound of the above by heating aldehyde and an excess of glycol together for eight days. The experiment was made with the view of ascertaining whether the two would combine to form bodies isomeric with the polyethylenic alcohols formed directly by the union of oxide of ethylene with glycol. The two did combine, but the author found a new compound having the formula $\text{C}_4\text{H}_8\text{O}_3$, showing the unexpected result that the aldehyde dehydrated the glycol and combined with the oxide of ethylene so produced. The new body is a colourless liquid, having an agreeable odour; the density at $0^\circ = 1.0002$; the boiling point 82.5°C . It is soluble in $\frac{1}{2}$ times its volume of water, but chloride of calcium and potash separate it from the solution. Nitric acid acts on it violently, forming, among other products, glycolic and oxalic acids. It is not affected by caustic potash; at 100°C . it reduces partially and slowly an ammoniacal solution of nitrate of silver.

The author found that aldehyde and oxide of ethylene would not combine when heated together for several days. The aldehyde became resinified, and the oxide of ethylene remained unchanged.

The Formation of Oil in Olives.—M. S. de Luca has made an important series of observations on this subject (*Comptes-Rendus*, t. liii. p. 386). His object was to ascertain at what stage of the growth the fatty matter began to form, and what matter or matters preceded and gave birth to the oil. He found that a very young olive had the density 1.008, and that from this it slowly increased until the density reached 1.097. From this point, as the oil was formed, the density diminished, and the author found a perfectly ripe olive to have the specific gravity 1.007. Perhaps the most important point determined by the author was the presence of mannite, which, he says, appears to be essential to the formation of the fatty matter. The human physiologist may perhaps take a note of this fact. A peculiar bitter principle was also found in the green olives, which the author could not isolate. Like the mannite, it is also to be found in the leaves, which, perhaps in virtue thereof, appear to possess decided febrifuge properties.

Cinnamates and Nitro-cinnamates.—M. E. Kopp has prepared and described many of these (*Comptes-Rendus*, t. liii. p. 634). The general properties of the cinnamates he describes as follows:—Those with an alkaline base are soluble in water. The alkaline-earth salts are but slightly soluble in the cold, but more so when heated. The earthy salts are insoluble, and are partially decomposed by boiling water. The metallic salts are almost insoluble even in boiling water, but are in general dissolved on the addition of a little acetic acid.

Nitro-cinnamic, though a weak acid, forms neutral salts, and decomposes alkaline carbonates. The alkaline salts

are very soluble, the others are but slightly so, or are altogether insoluble. They all deflagrate when quickly heated.

Cinnamene, C_{10}H_8 , is not only isomeric but completely identical with styrol. When pure it will change spontaneously into metacinnamene perfectly solid, transparent, and having all the properties, chemical and optical, of metastyrol.

III. ANALYTICAL CHEMISTRY.

Separation of Natural and Artificial Camphor.

—We find the following in the *Archiv. für Pharmacie*, bd. clvi. s. 332. In an alcoholic solution of natural camphor ammonia gives but a slight precipitate which is redissolved on shaking the mixture. A similar solution of artificial camphor, under the like treatment, gives a flocculent precipitate which remains undissolved.

MISCELLANEOUS.

Royal Institution.—The following meetings will take place next week:—Thursday, December 26, at three o'clock, Professor Tyndall, "On Light" (juvenile lectures); Saturday, December 28, at three o'clock, Professor Tyndall, "On Light" (juvenile lectures).

An Appeal for the "American Journal of Science."—Professors Silliman and Dana, the proprietors and editors of the *American Journal of Science*, have issued the following appeal to their friends:—"Cut off by the exigencies of the times from a large number of our subscribers—residents in disloyal states, and participating in the inconveniences consequent on the general derangement of public affairs, the proprietors of this journal trust they may be permitted to express to their subscribers and to the friends of science generally the hope that they will in all cases renew and continue their subscriptions to the *American Journal of Science* for the ensuing year. Claiming the privilege of bearing cheerfully our share of the present demands on all patriotic citizens, our hope is to receive such a measure of co-operation from the friends of science as will enable us to sustain with credit the public trust so long, by their indulgence, committed to our hands. This co-operation we look for not only from those whose names have been long on our lists, but from many others also who may desire thus to testify their good-will to the cause we represent."

ANSWERS TO CORRESPONDENTS.

* * All Editorial Communications are to be addressed to the Editor; and Advertisements and Business Communications to the Publisher at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

A Subscriber.—We will endeavour to ascertain.

C.R.—See Dr. Cattell's patent noticed in a previous number of the *CHEMICAL NEWS*.

Analyses of Yeast.—D. Clifton wishes to know where he can find Analyses of German dried yeast, and also Brewers' ale yeast.

O.—Noad's "Lectures on Electricity" is about as useful a book on the subject as can be placed in the hands of a person quite ignorant of the science.

M.D.—An electric light will have to be used, in order to throw metallic spectra on to a screen. The salts experimented upon must be placed in a small cavity bored out of the lower carbon pole, and the slit prism and lens must be arranged at such relative distances from each other and from the light, that a sharp image of the coloured bands is formed on the screen. This is not difficult to manage after a few trials, but it cannot be more fully explained without a diagram.

Iodide of Cadmium.—W.C.—The best way of preparing this salt is to take one part of cadmium in filings and two parts of pure iodine. Place them together in a capacious flask with a sufficient quantity of alcohol to cover them. Action will commence at once, attended with considerable evolution of heat. When the action ceases, heat the mixture till it is colourless. Filter from a few grains of cadmium, which will remain undissolved, evaporate, and crystallise.

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SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Researches on the Relations existing between Atomic Weights, by M. J. S. STAS, Member of the Royal Academy of Belgium.

(Continued from page 325.)

Analysis of Chlorate of Potash.—The determination of the oxygen contained in chlorate of potash has been made so often that it might be asked if there was any utility in repeating it. What has led me to undertake this analysis is, that the concordant results of Berzelius, M. Pelouze, and M. de Marignac disagree with those of M. Maumené and of M. Penny, to the accuracy of which I can certify. To perform this determination I made use of two different methods which ought to yield errors in opposite directions. My object in acting thus was to ascertain the extreme limits between which the figure would be found. One of the methods consists in decomposing the chlorate by heat, so as to weigh the residuary chloride: this is the process employed successively by Berzelius, Pelouze, De Marignac, and Maumené. The other consists in decomposing the chlorate with hydrochloric acid, and then weighing the residuary chloride; this is the plan adopted by Penny, but which I have modified in its details.

The chlorate experimented upon was prepared by the means which I pointed out when speaking of chloride of potassium obtained from the chlorate. I need not now return to the subject. I will only say, that I am sure it contains neither iron, manganese, copper, chloride, nor alkaline sulphates. I am, however, certain that it contains traces of silica, which it has not been in my power to separate from the salt without decomposing it.

I will now explain the arrangement which I made to perform the operation by each of these two methods.

Determination of the Oxygen in Chlorate of Potash by Decomposing the Salt by Heat.—I have performed five experiments by this method: two in a hard glass globe, of about two litres capacity, furnished with a neck 55 or 60 centimètres long and 2 centimètres wide: and three in a sphere of hard glass, with two necks opposite to each other, analogous to the apparatus employed by M. Dumas in his syntheses of water.

In the experiments with the simple globe I curved the neck at right angles as close as possible to the reservoir. To the end of this neck I fastened, by means of a resinous cement, a metallic cap, on which a stop-cock screwed. The complete apparatus held a vacuum perfectly. In the weighings I used as a counterpoise a globe of absolutely the same capacity and weight as the one used in the experiment, placing, at the same time, on the two pans of the balance a weight superior to that which I had to determine. I thus performed the successive weighings by subtraction. I placed the same supplementary weight

on each pan in order to remove any doubt about maintaining the quality of the arms of the balance during the successive weighings. The correction of the weight for the reduction to a vacuum was thus performed only upon the differences from the original weight,—differences which in this case corresponded to the weight of the substance.

When the equilibrium of the globe perfectly dry and devoid of air was established, and when, for fear of accident, I had even determined its absolute weight, I introduced in it the pulverised chlorate of potash, which I had heated in a current of pure and dry air to near its point of fusion. The neck of the globe having been cleaned to remove all traces of adhering chlorate, I plunged it into an oil-bath, heated to nearly 200°, and pumped the air out five or six times, passing into it each time pure and dry air, which had not been through the air-pump. After having brought the internal tension to exactly the same as it was at the first weighing, I brought it again into equilibrium by subtracting for this purpose weights from the pan to which the globe with the chlorate was attached. The globe being again filled with dry air, I unscrewed the stop-cock and filled the neck from its junction with the globe to within 10 centimètres of the cap fixed to its extremity with amianthus calcined with aqua regia. This amianthus I had transformed into a kind of felt, by stirring it up with water and then pouring the paste on to linen. Gases pass through this felt exactly as a liquid through a paper filter.

I then made a vacuum in the apparatus, heating the neck throughout nearly the whole of its length to carry off with the air the least traces of humidity which might have gained access during the introduction of the heated amianthus, and I determined the weight a third time, after having rarified the air to the same degree as in the two preceding weighings.

The apparatus being thus arranged, I placed the globe in a bath of magnesia, keeping its neck perfectly horizontal. To the stop-cock which terminated it, I adapted, by means of a caoutchouc connector, a hard, Bohemian glass tube, 60 centimètres long, by 1½ wide, drawn out at each end. The tube contained at each end a plug of felted amianthus, the remaining nine-tenths being filled with pure pulverulent silver precipitated by phosphorus. Before being placed in communication with the retort, it was carefully dried and weighed. This tube was followed by two others, curved U-shaped, filled with pumice-stone, moistened with sulphuric acid, likewise weighed; and finally, by a third U-tube, containing sulphuric pumice-stone, and intended to prevent the entrance of moist gas. The apparatus was terminated by a curved tube plunging to the depth of a millimètre only into water contained in a Woulf's bottle, which was furnished with a tube, curved, so as to allow of the connection with the disengaged oxygen.

As is well known, the decomposition of chlorate of potash demands excessive precautions to be effected

Numerical Order.	Apparent Weight of the Chlorate.	Weight of the Chlorate reduced to a vacuum.	Apparent Weight of the Evolved Oxygen.	Weight of the Oxygen reduced to a vacuum.	Weight of the Chlorine fixed by the Silver.	Weight of the Oxygen corresponding to the Chlorine fixed.	Corrected Weight of the Oxygen.	Oxygen evolved from 100'000 parts of Chlorate.	Chloride of Potassium produced from 100'000 parts of Chlorate.
I.	grammes. 69'8835	grammes. 69'8730	grammes. 27'3735	grammes. 27'3694	grammes. 0'0075	grammes. 0'0068	grammes. 27'3636	39'1620	60'8380
II.	82'1385	82'1260	32'1745	32'1697	0'0110	0'0085	32'1612	39'1605	60'8395
III.	86'5140	86'5010	33'8775	33'8725	0'0025	0'0020	32'8705	39'1560	60'8440
IV.	132'9430	132'9230	52'0570	52'0492	0'0085	0'0060	52'0430	39'1527	60'8473
V.	127'2315	127'2125	49'8205	49'8130	0'0036	0'0028	49'8102	39'1550	60'8450
							Mean. .	39'1572	60'8428

experiments which I have made with the identical globes used in experiment No. I. and in experiments Nos. III., IV., and V.

No. I.

1. Weight of the globe of 1.729 litres capacity exhausted of air to 0.003 millimètres . . . 823.917
2. Weight of the globe after having been heated for six hours in a bath of magnesia to the point of rendering the lower part milky,—
 - a. After cooling for three hours . . . 823.913
 - b. After eight hours . . . 823.917
 - c. The next day . . . 823.9175

Nos. III., IV., and V.

1. Weight of the globe exhausted to 0.003 millimètres . . . 1736.409
2. Weight of the globe heated to redness in magnesia, after cooling for eight hours . . . 1736.410
3. Weight of the globe heated in magnesia from eight in the morning till four in the afternoon. The bottom was completely milky. Exhausted to 0.003 millimètres. After five hours' cooling . . . 1736.408
4. Weight of the globe the next day . . . 1736.409

I have employed for the analysis of chlorate of potash three specimens of the salt from different sources, but purified by the same process. One specimen served for experiment No. I., another for Nos. II. III., and a third for Nos. IV. and V.

(To be continued.)

On the Specific Gravity of Substances in the Liquid and Solid State, by O. RICHTER, Ph.D.

I beg leave to draw the attention of chemists to the singular fact, that, whereas in our handbooks and periodicals the various chemical and physical properties of substances are duly recorded, comparatively little is said about their specific gravity in the liquid and solid state. In most cases nothing at all is said about it, or we must content ourselves with a vague statement that the body is lighter or heavier than water, &c. Considering, however, that a correct and thorough knowledge of this property of matter is of paramount importance, both in a theoretical and in a practical point of view, I think that the simple mention of this fact will convince the chemical public of the necessity of at once setting to work in order to complete this portion of our science. Confining myself to the theoretical point of view, I shall observe, in the first place, that a knowledge of the atomic volume, which is obtained by dividing the atomic weight by the specific gravity, may lead to an insight into the nature and *modus operandi* of that mysterious cause which determines and modifies it. This volume is evidently the result of the combined repulsive energies proper to the various elements which are grouped together in the constituent molecule. Now, it is natural to suppose that every chemical element, in addition to its weight-equivalent, possesses also a volume-equivalent, with which it enters into combination, so that, supposing this latter to be as constant as the former, the atomic volume of chemical compounds would always be equal to the sum of the atomic volumes of their component elements. Experiment, however, teaches us that the cases which favour this hypothesis are comparatively of rare occurrence. Hence, in order to reconcile and explain the anomalies, we are obliged to suppose

a certain rate of increase or decrease in the atomic volume of one or more or all the constituents. The speculations of Kopp, Schroeder, Playfair, and others in this direction, made with the view of discovering the fundamental law of variation, to which the atomic volumes of all the elements are held to be subject, have as yet led to no satisfactory result. The inquiry was almost exclusively confined to inorganic chemistry; and here the number of ascertained cases was far too limited to arrive at valuable and extensive generalizations, while the organic part presented even more scanty material for the solution of this all-important problem.

The remarkable discoveries of Playfair relative to the atomic volume occupied by the acid and basic constituents of salts, and by these latter according as they are either anhydrous or associated with a larger or smaller amount of water of crystallisation, appear to conceal within them a profound philosophical truth, viz. that the eighth part of the volume of ice, which Playfair calls the unit volume, is really the standard whereby all the volume equivalents of the various chemical elements will one day be correctly measured and calculated. And here I may express the hope that this highly gifted philosopher, who is so intimately familiar with all the difficulties of this species of research, may still persevere in his efforts to penetrate the secret of the wonderful mechanism which controls and accomplishes all those endless variations in the atomic volume of chemical substances, and, what is very remarkable, which causes these variations to be superinduced upon the molecule without the slightest derangement taking place in the relative positions of its constituents. Hence, on the hypothesis that the chemical elements consist, not of single atoms, but of a definite number of such atoms associated according to some fixed principle of grouping, all the modifications in the physical properties of matter which we are in the habit of distinguishing by the terms allotropism, polymorphism, fusing and boiling points, crystalline form, optical and electro-magnetic polarity, &c., ought to be considered as so many functions of one and the same variable magnitude—namely, the atomic volume.

This view of the genetic relations between atomic volume and physical properties appears also to be fully borne out by experiment; for we invariably find that a change in the former involves generally a corresponding change in the latter, and *vice versa*; and, moreover, in thus modifying the physical conditions under which the chemical affinities are called into action, it is reasonable to expect likewise a corresponding change in the range and intensity of these latter. Accordingly, it is easy to see why sulphur and phosphorus, and, indeed, many other elements, if not the whole of them, if presented in one of their allotropic states, will be readily attacked by oxygen, while in another they remain passive towards that re-agent, at least up to a certain temperature.

I shall now, in the second place, take this opportunity in stating, that, for the last twelve years, I have devoted much time and labour in composing a catalogue of the densities and atomic volumes in the liquid and solid state of as many chemical substances as I could gather together from the various compendiums and journals within my reach. This catalogue contains, in round numbers, 600 inorganic and 350 organic substances, the sum of which amounts to what comes evidently to be a mere fraction in comparison with the hundreds of thousands of chemical productions, which, however carefully they may have been investigated in other respects, have yet been entirely neglected with regard to their densities in the liquid and solid state. The publication of

this catalogue, which, even in its present imperfect form, furnishes abundance of material for speculation, has only been delayed in the hope that chemists would soon make an united and vigorous effort in this direction.

With reference to this subject, I request the analysts to bear in mind that, when there exists a striking difference in the densities of a body as stated by different observers, it may in general be assumed that they have indeed examined one and the same substance, but which is the same in a chemical point of view only, and altogether different in a physical point of view. But even in such cases as the family of the sulphurs, or that of the oils of turpentine, where the differences in density are comparatively small, experiment clearly shows that, by varying their atomic volume, these substances have at the same time experienced certain changes in their physical properties, and which are sufficiently marked to individualise them as members of the same family. In the preceding remarks I have endeavoured to prove that chemistry has for its foundation not a simple, but a complex principle.

On the one hand, it is the purely chemical principle, whatever its nature may be, which determines the various forms of molecular arrangement, and it is the ponderable portion alone of the various kinds of atoms which in their constant weight-equivalents furnishes the basis of calculation. On the other hand, it is the purely physical principle, whatever its nature may be, which determines the atomic volume of the molecules, and it is the imponderable portion alone of the various kinds of atoms which in their variable volume-equivalents furnish the basis of calculation.

Accordingly, our tree of chemical knowledge splits up into two main branches, for which I would propose the terms Pondo-Chemistry and Impondo-Chemistry. If this division be correct, it follows that, in order to unravel the mixed and complex character of the phenomena, our present methods of analysis, however well adapted they may be for that branch which is engrafted upon the chemical principle, will require to be modified so as to become available for the other branch also.

CORRESPONDENCE.

Chemical Nomenclature.

To the Editor of the CHEMICAL NEWS.

SIR,—As special attention has lately been drawn to the subject of Chemical Nomenclature, perhaps a few additional remarks thereupon might not be out of place in your valuable journal.

I think it is sufficiently obvious that many of those who have offered us a new terminology expect more from words than they are lawfully capable of expressing. It is too much to suppose that a single word shall denote the constitution of a body, the number of atoms it contains of certain elements, and the names of the elements themselves. Your correspondent "Inquirer" (in No. 106) appears to have perceived this; and humorously asks of one propounder by what name Albumen is recognised in his system. I, for my part, am considerably at a loss to see what is to be gained by the employment of this "concentrated essence" of language; and it would be more difficult, perhaps, for the memory to retain the all-comprehensive name than the facts and theories vainly sought to be included under it.

The purely arithmetical systems hitherto proposed abound, to excess, with the most unmusical and distasteful names,—names which will never be adopted as components of a great and useful arrangement, any more than,

to take a well-known instance, the Latin verses constructed from tables can be combined to a poem. From such systems, too, the Natural History element is too much excluded; so that it would be hardly an unwarrantable presumption to anticipate for them the fate which, in Botany, has attended the classification of Linnaeus.

So extensive a scheme of innovation as must be necessarily contemplated by any one who wishes to revolutionize an extensive nomenclature is one only suited for veteran practitioners. And, even from them, how slowly have alterations been adopted! The well-matured and excellent views of Gerhardt have been a considerable number of years meeting with their present success, and that is by no means complete. What, then, shall be said of some claims which have been recently advanced?

On the whole, therefore, I would venture to recommend that the steps which are taken to alter our nomenclature should be as gradual as possible, inasmuch as small changes are always discussed more soundly and adopted with greater security. This proceeding would also be incalculably more just to inventors, whose proposals could thus be examined with brevity and ease.

I have submitted these remarks in the hope, on the one hand, of moderating over-hasty theoretical zeal, and, on the other, of perhaps furnishing one or two tests for the stability of proffered systems. Without adopting some such considerations as these, we shall have our terminology laid open to the just though unwelcome charge, "Tot chemici, tot theoriæ."—I am, &c.

EDMUND J. MILLS.

Analysis of Yeast.

To the Editor of the CHEMICAL NEWS.

SIR,—In reference to your correspondent's (Mr. D. Clifton) enquiry about analysis of yeast, allow me to communicate briefly the following:—There are analyses of brewer's yeast quoted in Schubarth's well-known work, *Handbuch der technischen Chemie und Chemischen Technologie*, Vol. iii. Berlin: 1851. On page 506 we read. According to Schlossberger and Wagner, brewer's yeast, deduction being made of the ashes, is composed as follows:—

	Schlossberger.		Wagner.	
	Oberhefe. ¹	Unterhefe. ²	Oberhefe.	Unterhefe.
Carbon	50.05	48.03	44.372	49.761
Hydrogen	6.52	6.25	6.040	6.804
Nitrogen	11.84	9.80	9.203	9.171
Oxygen	31.59	35.92	40.385	34.264

Beside this a most elaborate paper has been published in 1845, by Professor G. J. Mulder, at Utrecht in his *Scheikundige Onderzoekingen*, Vol. ii., pp. 409 to 482, this paper contains, Mulder's researches on the so-called German dried yeast, in reality a product obtained in very large quantities from the distilleries at Schiedam, Holland; that yeast is composed of a cellular tissue $C_{12}H_{10}O_{10}$, and of a proteine compound $15(C_{40}H_{22}N_{10}O_{12}) + 8S$. The ash in the anhydrous dried yeast amounts to 10 per cent. and contains phosphoric acid, phosphate of lime, and other saline matter usually met with in substances of vegetable origin. The cellular tissue amounts to from 30 to 45 per cent. and the proteine compound from 55 to 70 per cent. Yeast contains moreover a very small amount of margaric and elaic acids. In Mulder's works on Wine, and that on Beer, valuable information is given about fermentation and yeast. I could quote several other sources which will give information on this subject, but think this may satisfy your correspondent.

I am, &c.

DR. A. ADRIANI, F.R.N.I.E., &c.

Laboratory, Edinburgh, December 23.

¹ Yeast from upper, or surface fermentation.

² Yeast from under fermentation.

On Butter and Other Fats.

To the Editor of the CHEMICAL NEWS.

SIR,—When I first put forward my original paper, as well as the subsequent ones, on Butter, I felt so convinced of the accuracy of my statements that I had no idea they would have elicited so much discussion.

In his conclusion No. 1, Dr. Ballard has set out with an erroneous quantity, and would appear to make out that butter is not entirely soluble in ether at 65°. Of course, if a person uses an immoderate quantity, which I contend he has, what otherwise could be expected?

Since reading Dr. Ballard's last paper, I have made the following experiments, which go far to make good all I have previously asserted:—

Experiment 1.—Ten grains of pure butter, on being dissolved in one drachm of ether, by agitation of the phial in the warm hand for a minute or so, and kept at a temperature of 65° for twelve or more hours, remained permanent; indeed, that which I made two months ago, when I first wrote my paper, has scarcely deposited, and that little is due to cold.

Experiment 2.—Twenty grains, similarly treated, dissolved at first, but did not continue permanent longer than an hour or so, owing to an insufficiency of ether.

Experiment 3.—Ten grains each of butter and beef dripping, dissolved in one drachm of ether, and the bottle held in the hand for a minute or so, the beef fat, from being softer than the butter, dissolved soonest; but when both were dissolved and kept at a temperature of 65° the beef fat deposited to a certain extent, and at 55° the deposit of beef fat occupied full half the bottle. By this we can readily detect beef fat in butter, the deposit being scalliform to the eye, but is in reality a mass of margaric crystals, and which did not re-dissolve in a room heated to 65°.

Experiment 4.—Twenty grains of beef fat, similarly treated, deposited considerably on standing, and at 55° to more than half the bottle full.

Experiment 5.—Ten grains each of butter and lard, similarly treated, although held in the hand for a long time, could not well be dissolved, but remained suspended just like a soap of hartshorn and oil, occupying full four-fifths of the bottle at 65°.

Experiment 6.—Ten grains of mutton dripping, similarly treated, at 65°, with one drachm of ether, deposited to the extent of three-fourths of the bottle full.

Hence, we come to the practical conclusion that the stearin of both lard and mutton dripping is nearly, if not quite, insoluble in ether at 65°, which establishes the point I originally founded my paper upon, and that Dr. Ballard, who evidently means well, is greatly mistaken in supposing that butter renders lard more soluble, according to his conclusion No. 7, as proved by my experiment No. 5.

The above simple set of miniature experiments shows unmistakably, notwithstanding all that has been advanced against it, that the ether test is preeminently worthy of confidence in every respect; in short, combined with the microscopic method I have since published, comprises all that is required in the investigation of fatty bodies generally.—I am, &c.

JOHN HORSLEY, F.C.S.

County Analyst's Laboratory, Police Station, Cheltenham.

MISCELLANEOUS.

The approaching Eclipse of the Sun.—At the last meeting of the Manchester Literary and Philosophical Society, Mr. Dickinson read a "Note on the Eclipse of the Sun, December 31, 1861." This eclipse, being small compared with several that have lately occurred,

will not attract so large an amount of general attention; it will, however, be of considerable interest to scientific persons, and in the hope that the members of this Society will not consider it an unimportant phenomenon, the following results are submitted to enable them to compare their observations with the computed times. Calculations for Manchester (Royal Infirmary), lat. N. 53° 29', long. W. 2° 14'.

	H.	M.	S.
Eclipse begins, December 31	1	49	19
Greatest Phase	2	49	9
Eclipse ends	3	45	29

mean time at Greenwich. Magnitude of the Eclipse (Sun's diameter = 1) 0.413 on the Sun's southern limb. Angle from North Pole, of first contact, 143° towards the West; last contact, 108° towards the East. Angle from vertex, of first contact, 157° towards the West; last contact, 80° towards the East, for direct image.

Long Tube Barometer.—After a recent meeting of the Institute of Civil Engineers, Mr. R. Howson exhibited in the library a Barometer, consisting of a long tube freely suspended open end downwards, a cistern which was of a tubular shape, and a "stalk." The stalk was a glass tube, sealed at both ends, attached firmly at its lower end to the bottom of the cistern, and rising axially up the tube until it nearly reached the surface of the mercurial column. The consequence of this arrangement was, that the top of the stalk came into the region of very low pressure, and there was an excess of pressure tending to force the cistern upwards. This excess was represented by the weight of the cistern (and stalk), and the contained mercury, so that under a given atmospheric pressure, the cistern would always hang suspended at a given level. When the pressure of the atmosphere rose, a portion of mercury left the cistern and passed into the tube, and the cistern alone arose, until the level was replaced by the immersion of the glass which formed the tube. When the pressure fell, the converse took place, an elongated scale was thus produced, the extent of range being dependent upon the relative areas of the tube, and of the glass which composed it. The action might also be simply viewed as that of a long piston, or plunger, with a liquid packing, having a vacuum on its upper side, and a self-graduating weight attached to its lower side.—*Mechanics' Magazine.*

ANSWERS TO CORRESPONDENTS.

* * All Editorial Communications are to be addressed to the EDITOR; and Advertisements and Business Communications to the PUBLISHER at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

* * In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Magenta.—The correct way is by distillation. Different naphthas may be compared by ascertaining what per-centage comes over below 212°. The more the better. Benzine and benzol are the same thing. The quality of nitrobenzol can be readily ascertained by the smell, it should be very delicate and pure, and should leave no unpleasant odour when a drop is evaporated on the hand.

G. H. F.—Your question is exceedingly elementary. Sulphide of calcium is composed of one equivalent of sulphur and one of calcium, the metallic basis of lime. Sulphate of lime, on the contrary, consists of one equivalent of lime and one equivalent of sulphuric acid. The former has the formula CaS, and the latter, CaO.SO₃. They are totally distinct bodies, and have no resemblance to each other in their properties.

J. Shields.—A letter addressed to M. Kopp, at the Chemical Society of Paris, would find him.

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